















THE  
PHILOSOPHICAL TRANSACTIONS

OF THE  
ROYAL SOCIETY OF LONDON,

*FROM THEIR COMMENCEMENT, IN 1665, TO THE YEAR 1800;*

*Abridged,*

WITH NOTES AND BIOGRAPHIC ILLUSTRATIONS,

BY

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RICHARD PEARSON, M.D. F.S.A.

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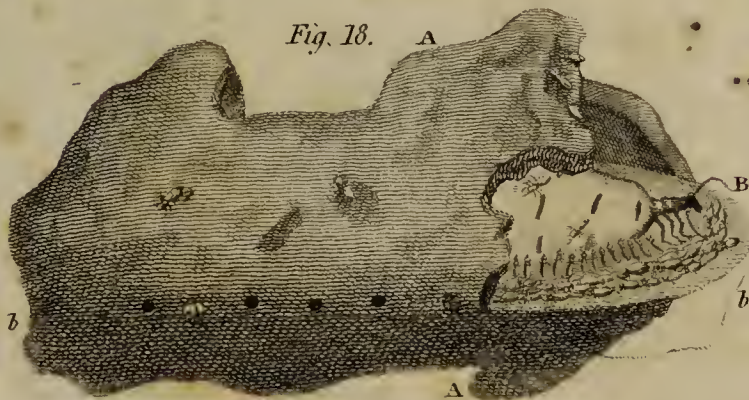
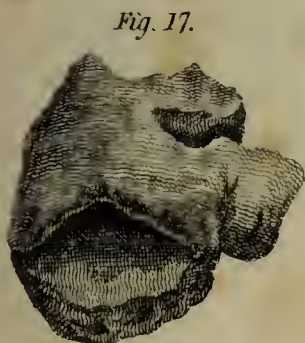
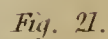
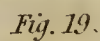
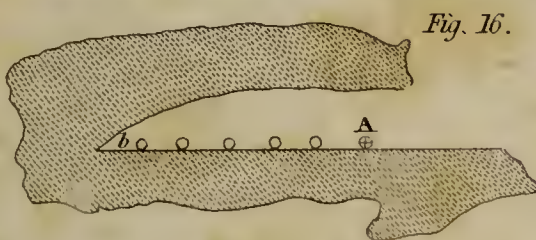
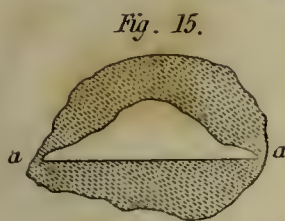
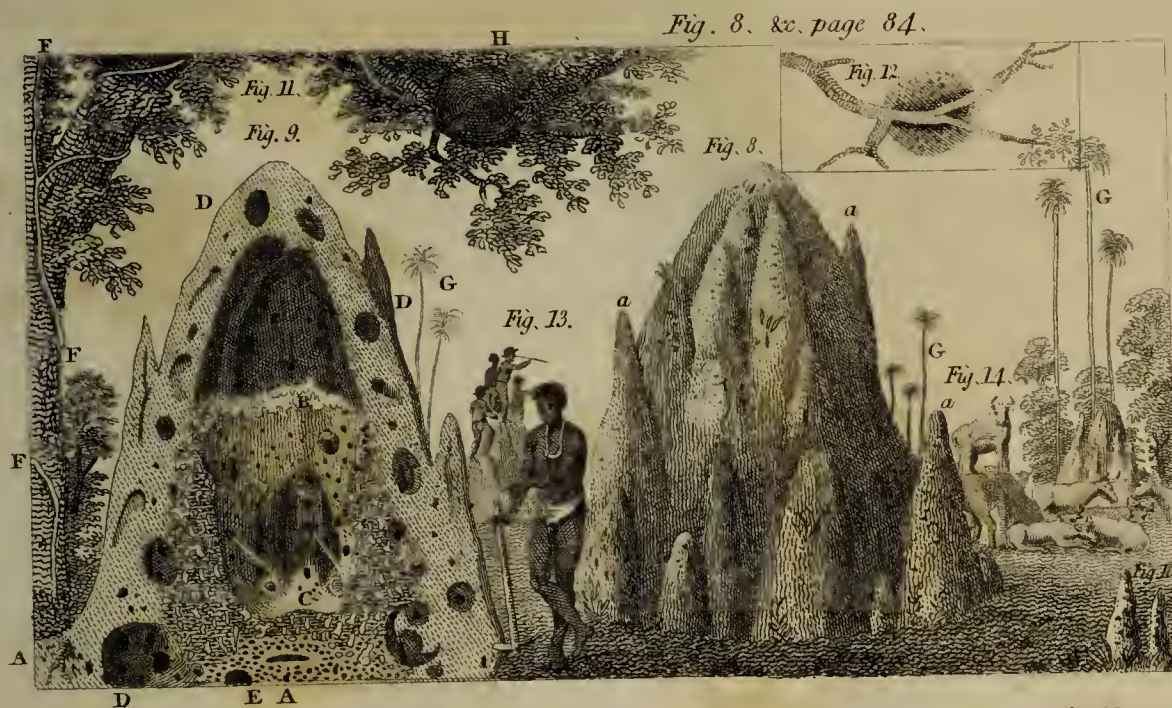
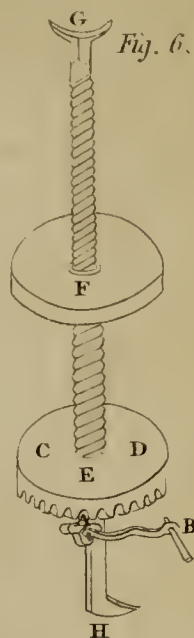
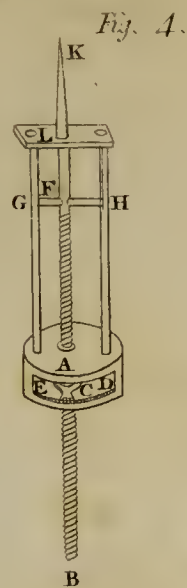
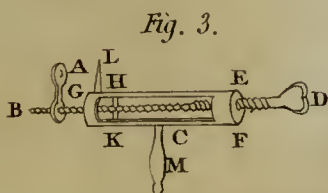
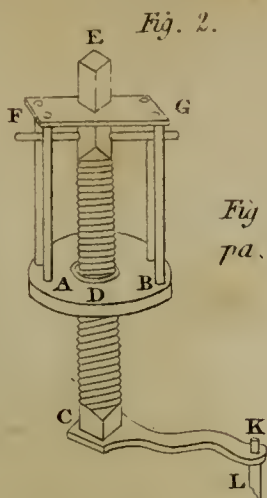
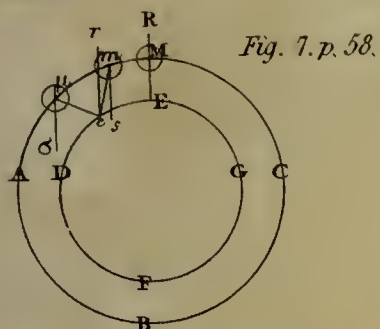
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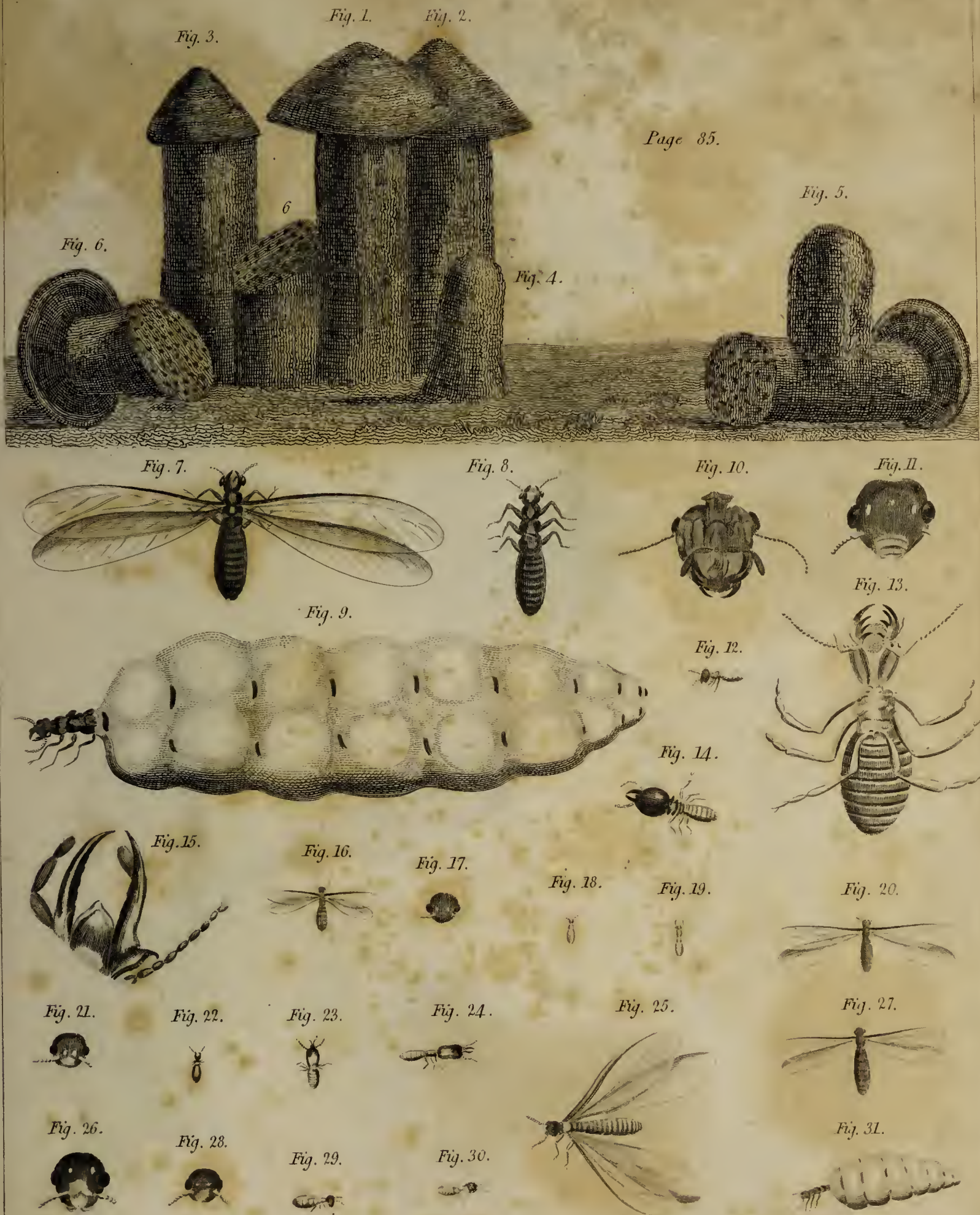
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*Ophidium Barbatum* Linn.

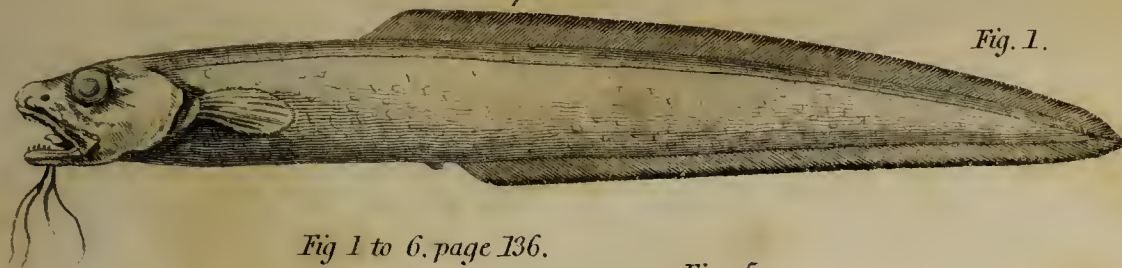


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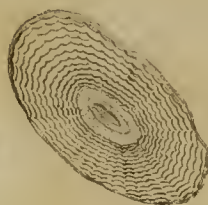


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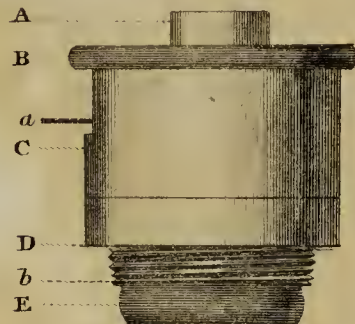


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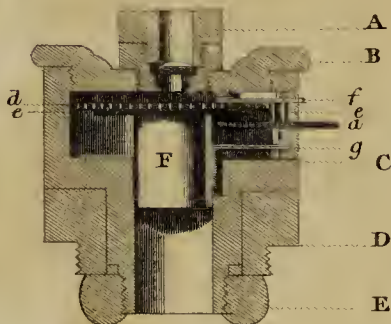


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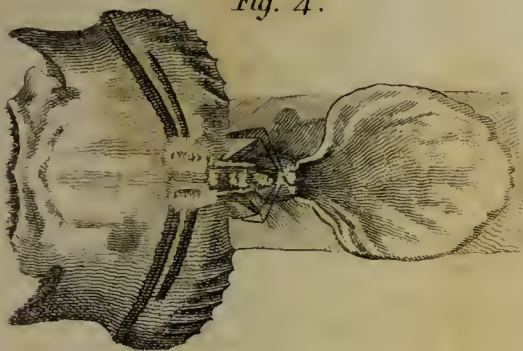


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Fig. 6.



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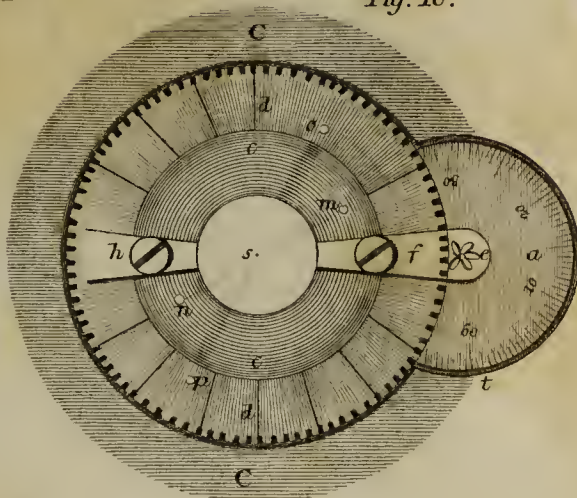


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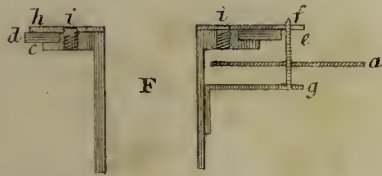
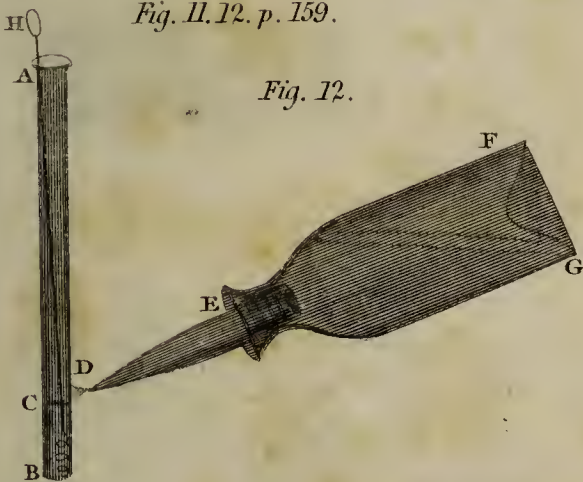


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Fig. 11.



Fig. 12.



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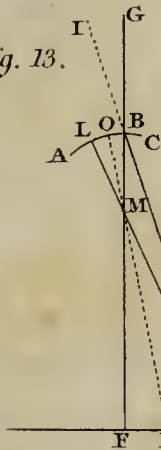


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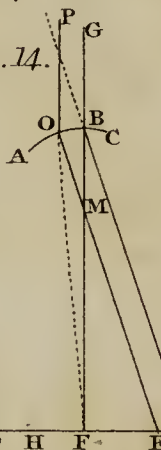
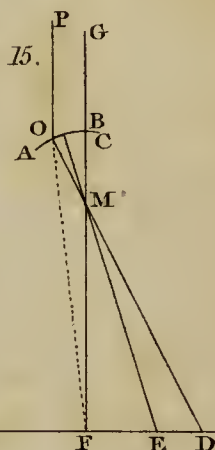


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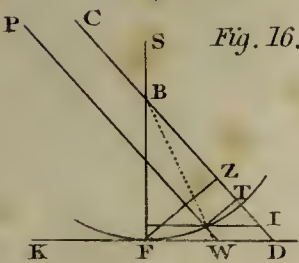
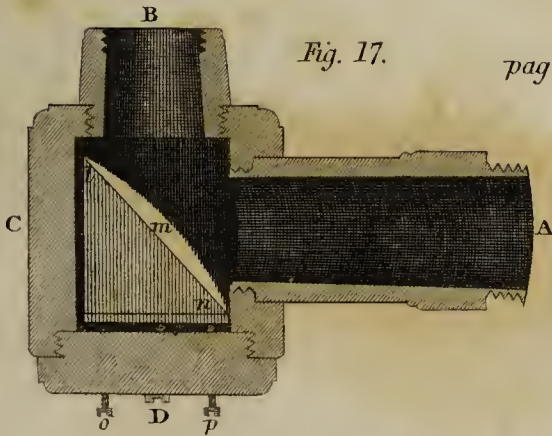
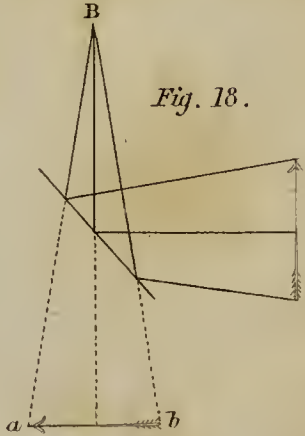


Fig. 17.



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Fig. 18.









Herschel on the Parallax of the Fixed Stars.

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Fig. 2.



Fig. 1.

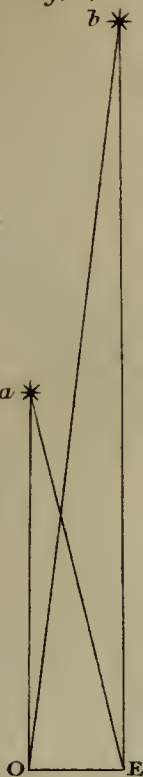


Fig. 3.



Fig. 5.



Fig. 4.



Fig. 7.



Fig. 6.



Fig. 11.

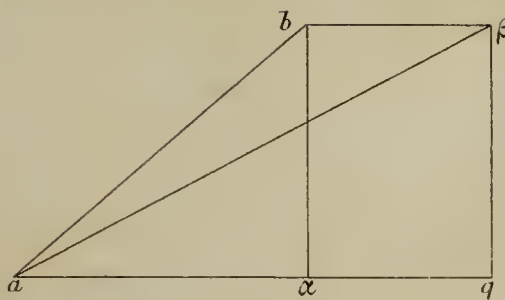


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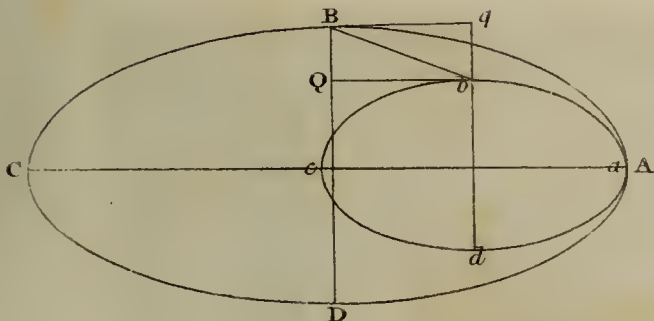


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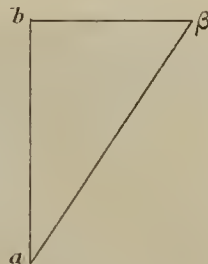
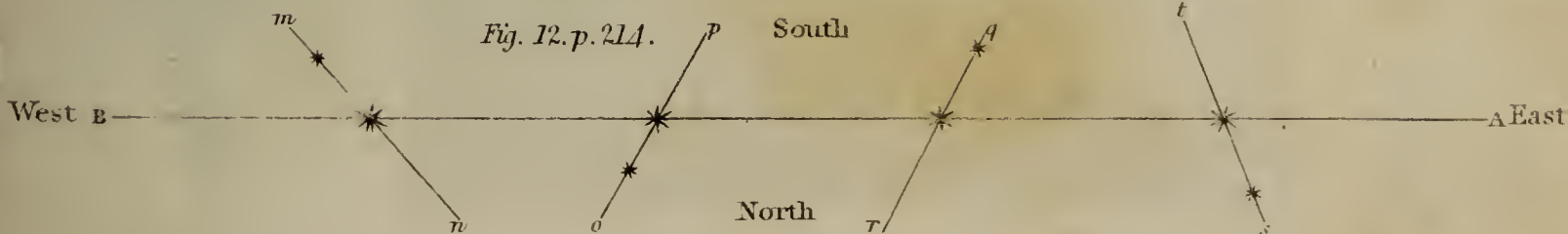


Fig. 9.



Fig. 12. p. 214.



Mutlow Sc. Russell Co<sup>t</sup>





*Herschel's Lamp-Micrometer.*

Pa. 230. &amp;c.

Fig. 4.



Fig. 3.



Fig. 5.



Fig. 1.

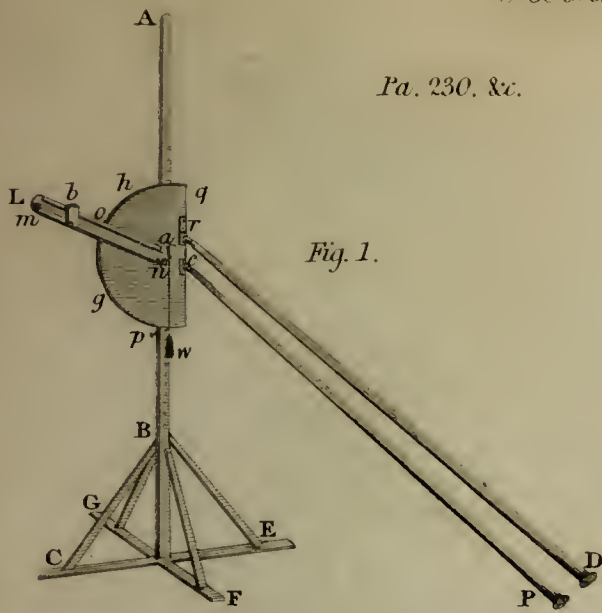
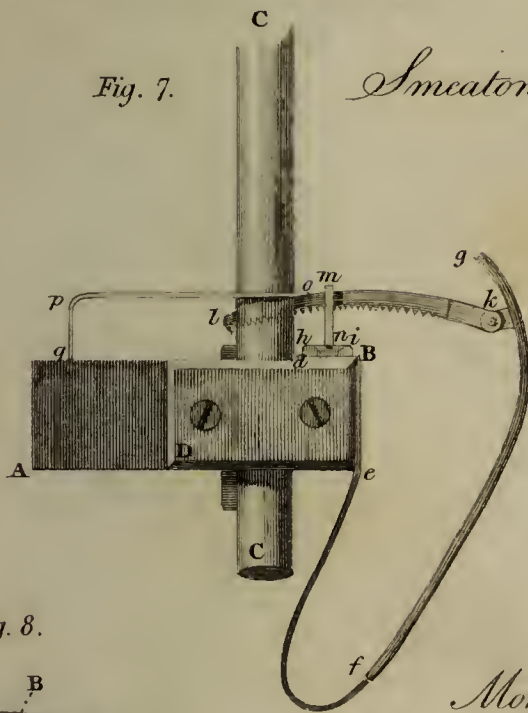


Fig. 2.



Fig. 7.

*Smeaton's Machine for Collision.*

Pa. 301.

Fig. 6.

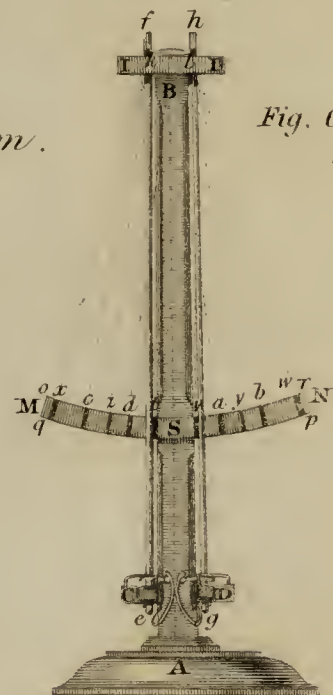


Fig. 8.

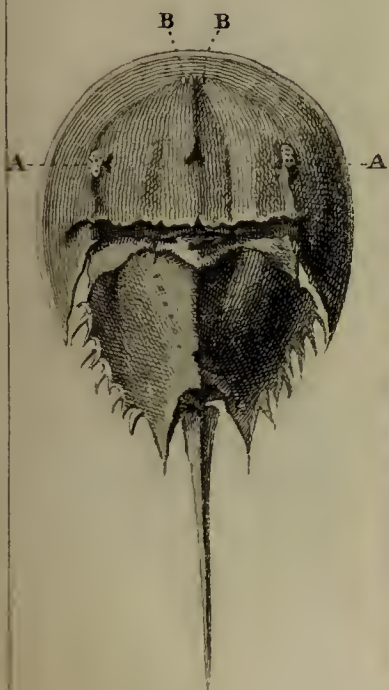


Fig. 8 to 12. Pa. 323.

*Monoculus Polyphemus.*

Fig. 9.

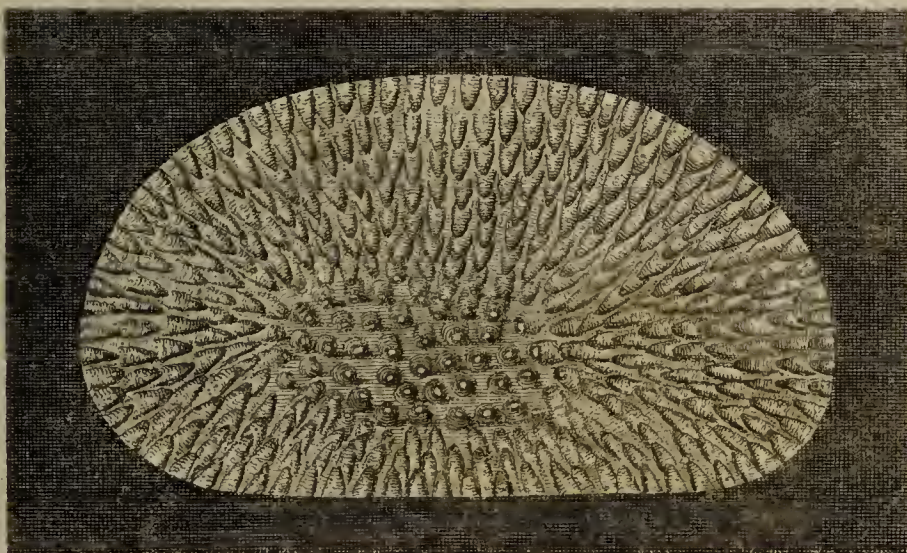


Fig. 12.

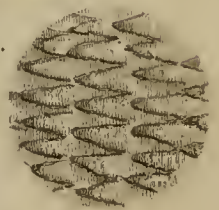


Fig. 10.

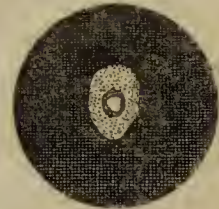
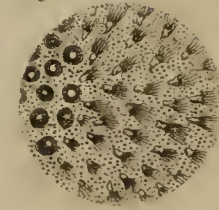


Fig. 11.



Mudlow Sc. Engrs. Co.





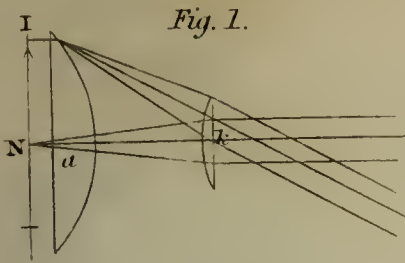


Fig. 1.

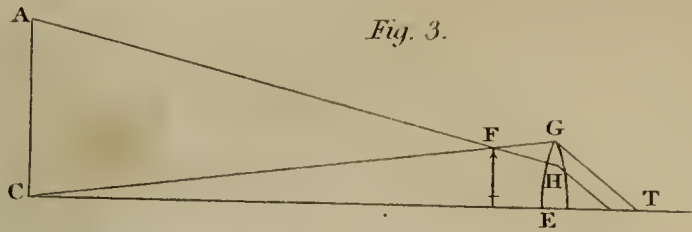


Fig. 3.

Fig. 1 to 4. Pa. 351.

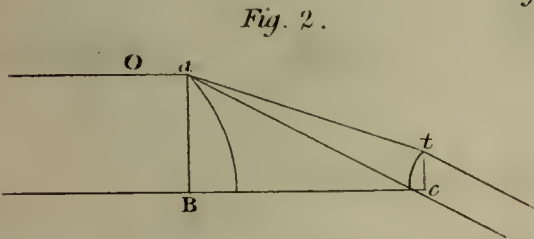


Fig. 2.

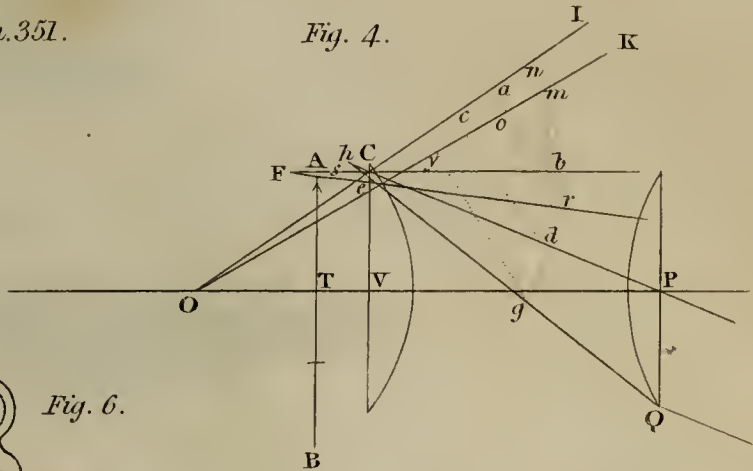


Fig. 4.

*Mr. Cavendish's Eudiometer.*

Pa. 355.

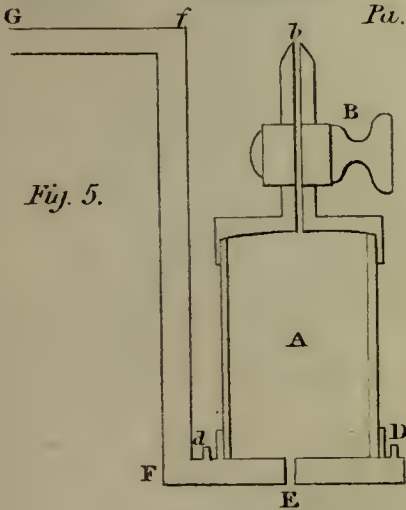


Fig. 5.

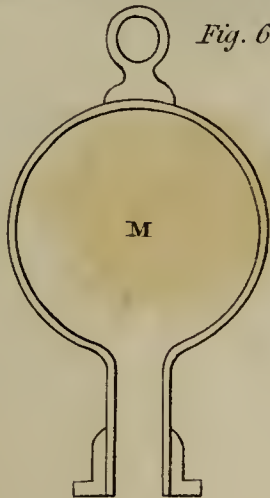


Fig. 6.

Fig. 11. Pa. 405.

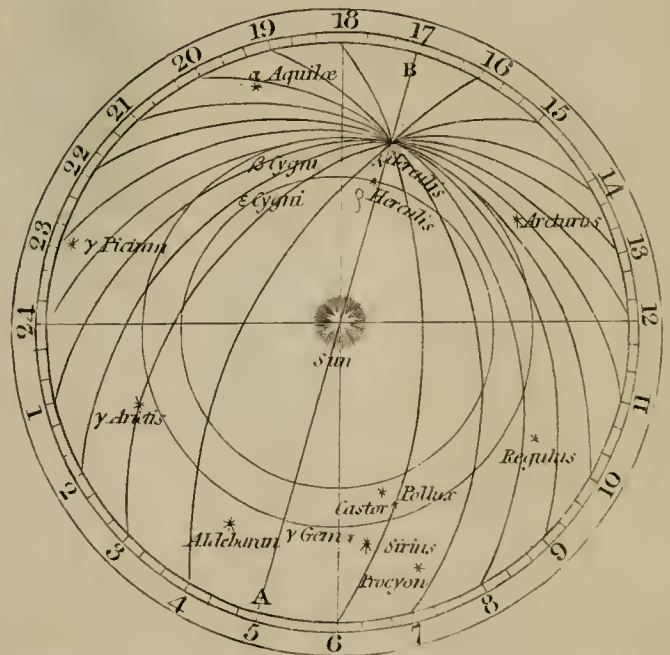


Fig. 7.



Fig. 8. Pa. 402.

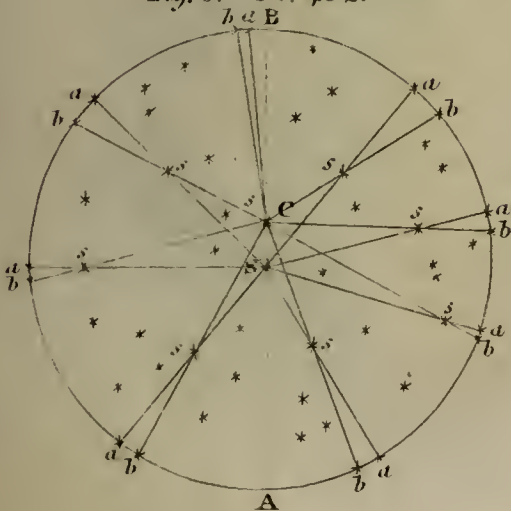


Fig. 10. Pa. 404.

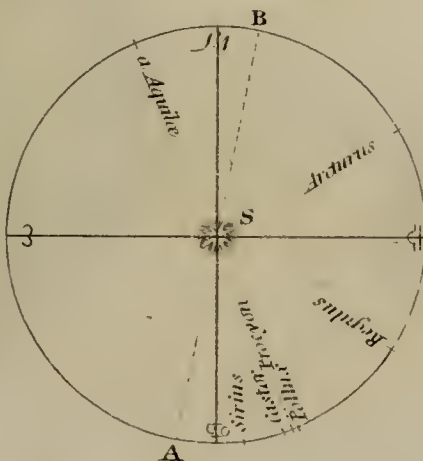


Fig. 9. Pa. 403.

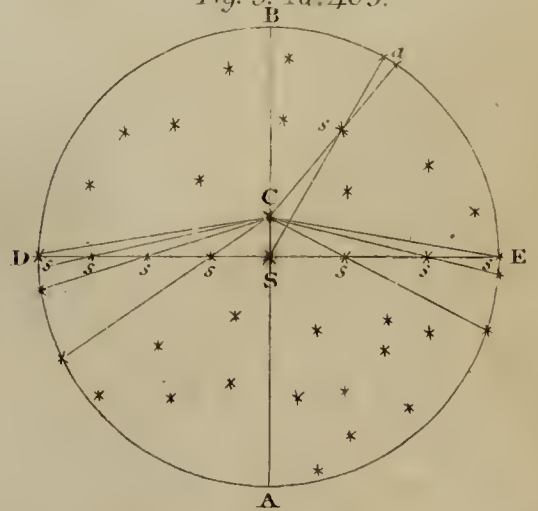






Fig. 1. Pa. 454.



Fig. 3. Pa. 455.

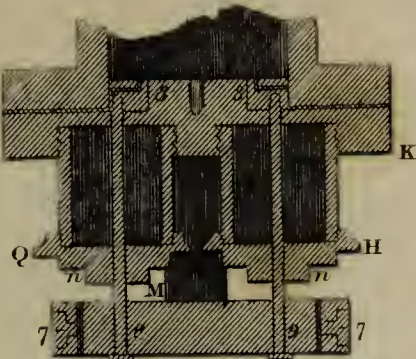


Fig. 4. Pa. 466.

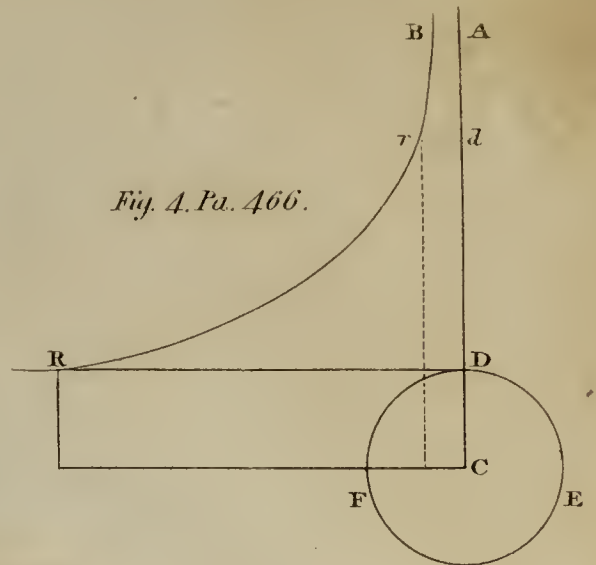
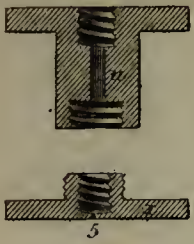


Fig. 2. Pa. 455.



The Fire Ball. Pa. 478.

Fig. 5.



Fig. 6.



Fig. 7.

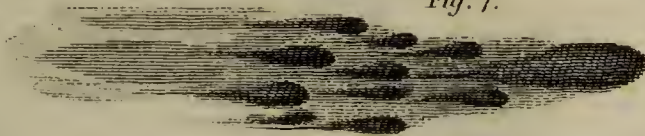


Fig. 8. Pa. 532.

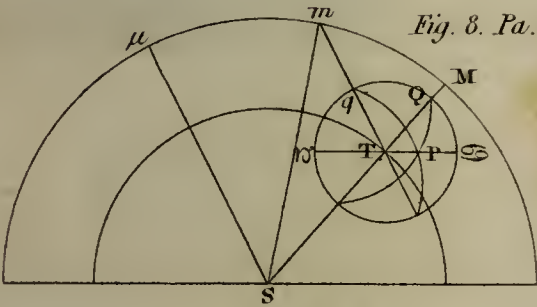


Fig. 9. Pa. 535.

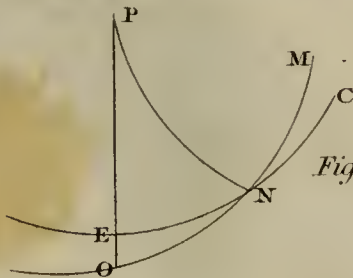


Fig. 14. Pa. 572.

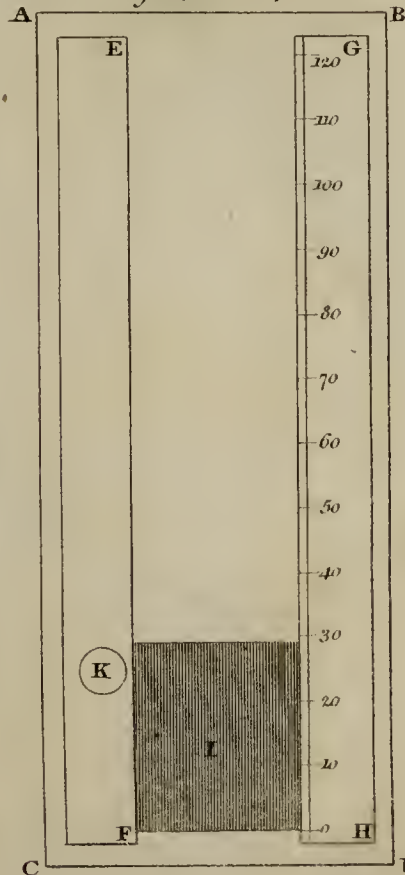
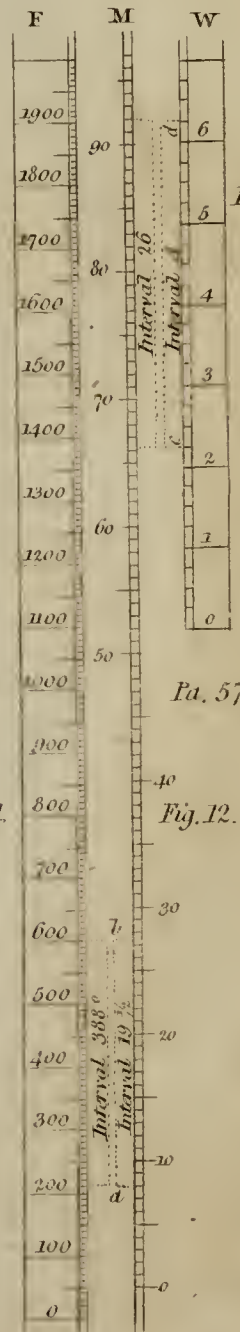


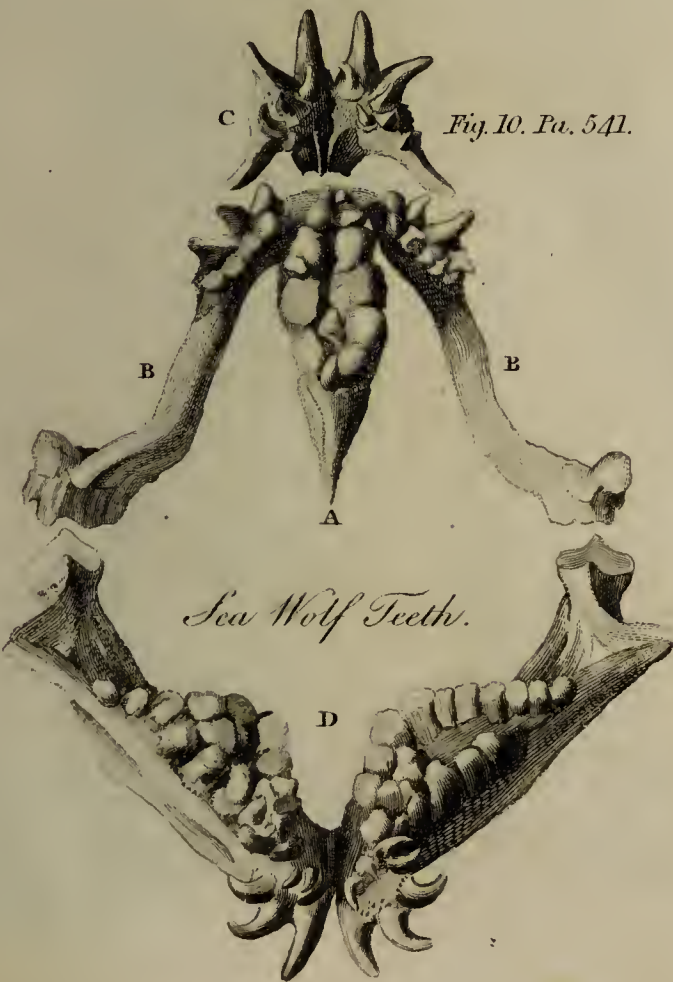
Fig. 11.



Pa. 571.

Fig. 12.

Fig. 10. Pa. 541.



Sea Wolf Teeth.

Fig. 15. Pa. 575.

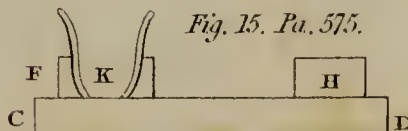
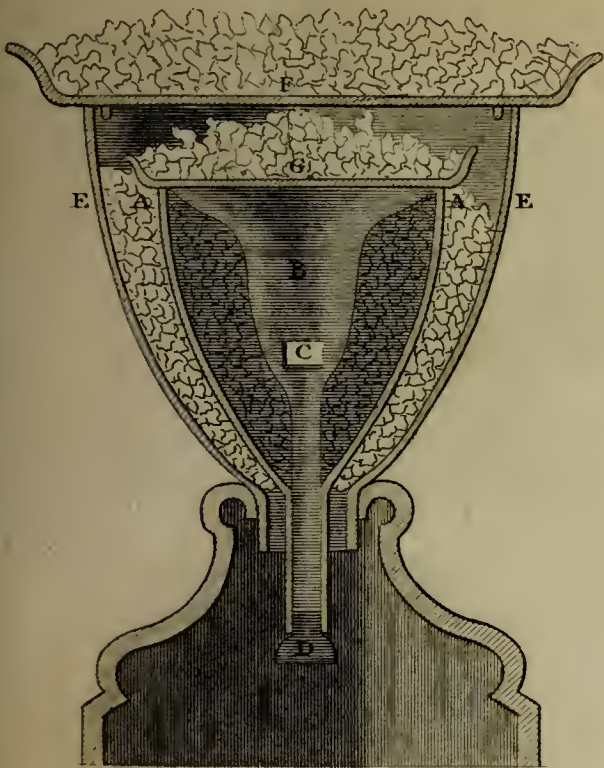






Fig. 1. Pa. 581.



Herschel's construction of the Heavens. Pa. 613.

Fig. 2.

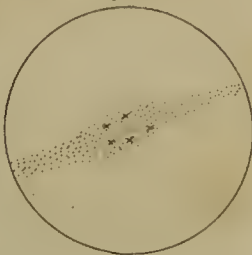


Fig. 3.

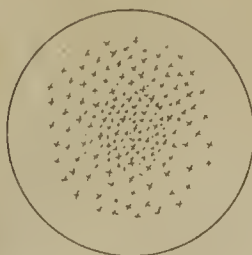


Fig. 4.

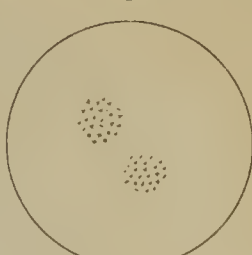


Fig. 5. Pa. 614.

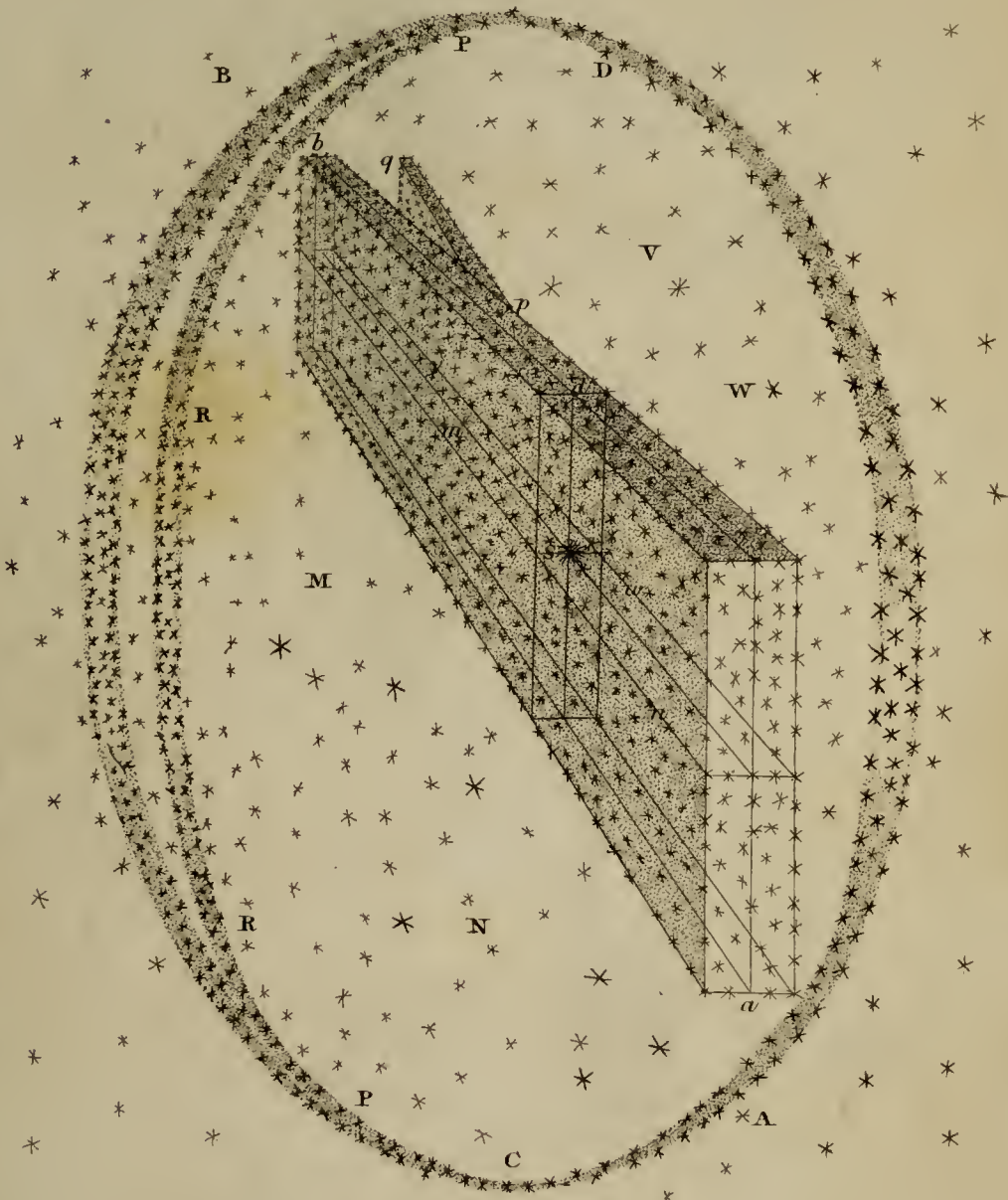


Fig. 6. Pa. 616.

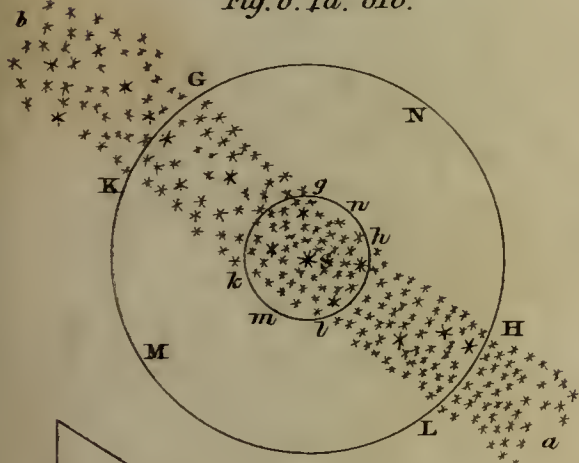


Fig. 7. Pa. 617.

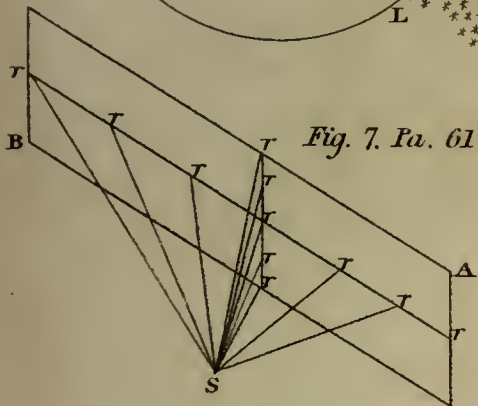


Fig. 8. Pa. 620.

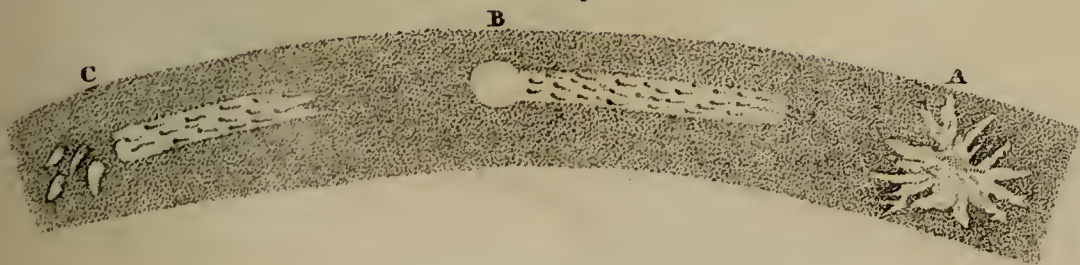
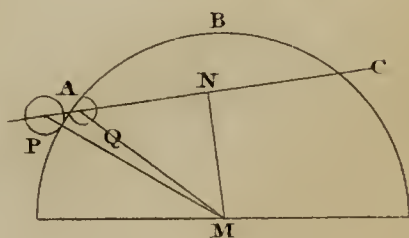


Fig. 9. Pa. 652.



Mutlow Sc. Russell. Gt.





*Vince on Friction. Pa. 661 &c.*

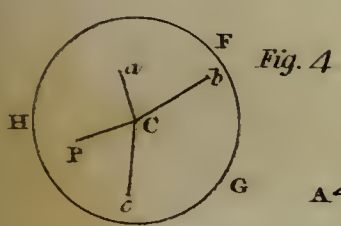
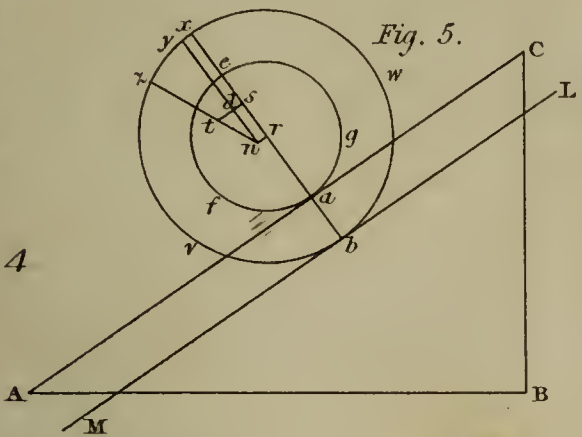
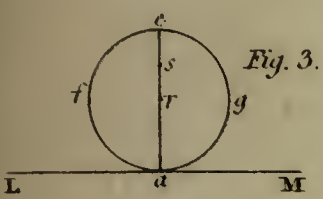
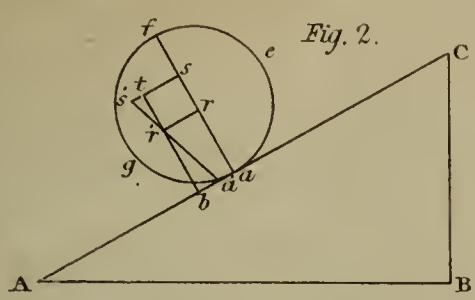
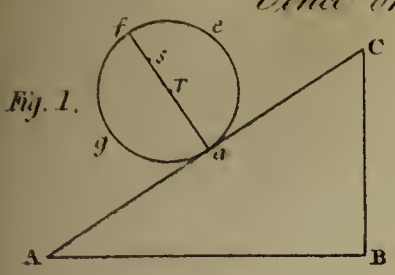


Fig. 6. Pa. 684.

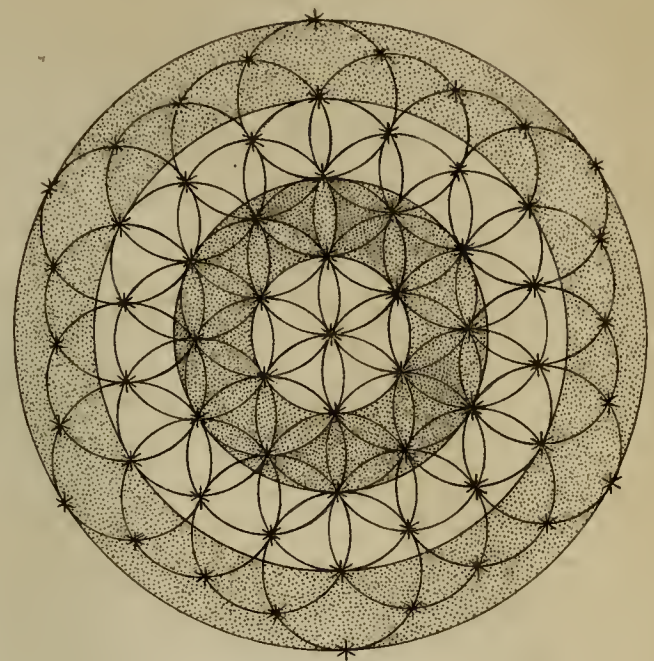


Fig. 7.

Pa. 685.

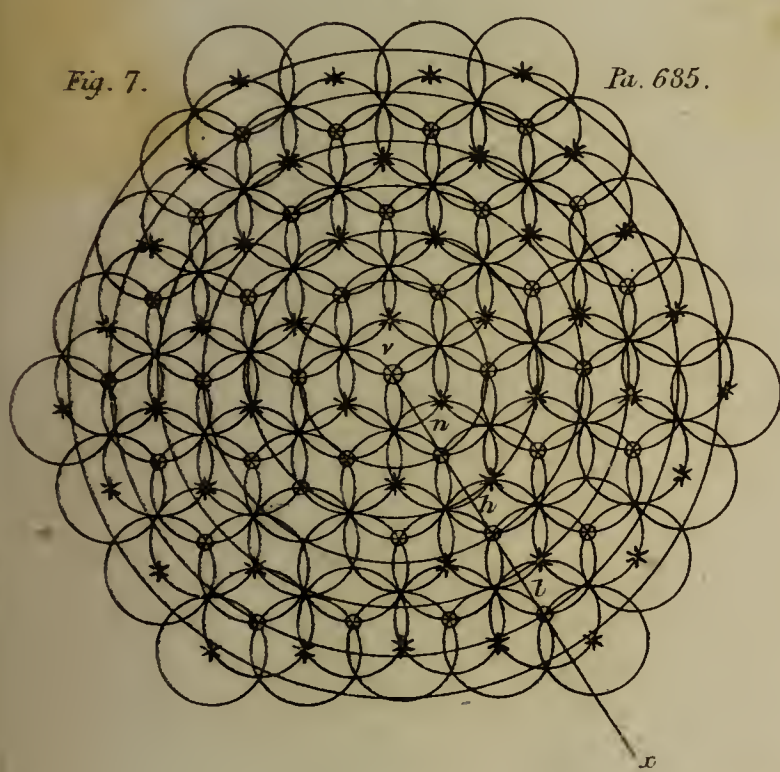


Fig. 8. Pa. 685.

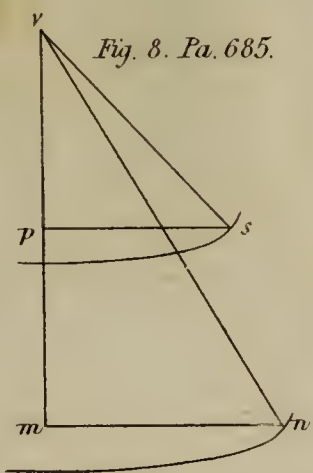


Fig. 10. Pa. 694.



Fig. 11. Pa. 699.

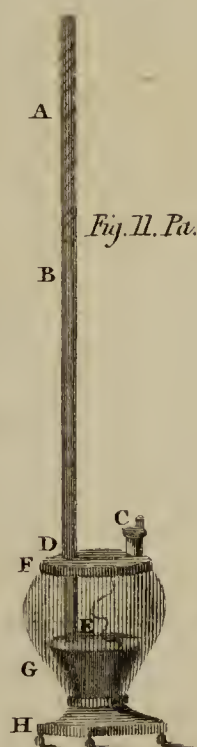


Fig. 12. Pa. 702.

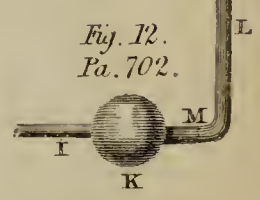
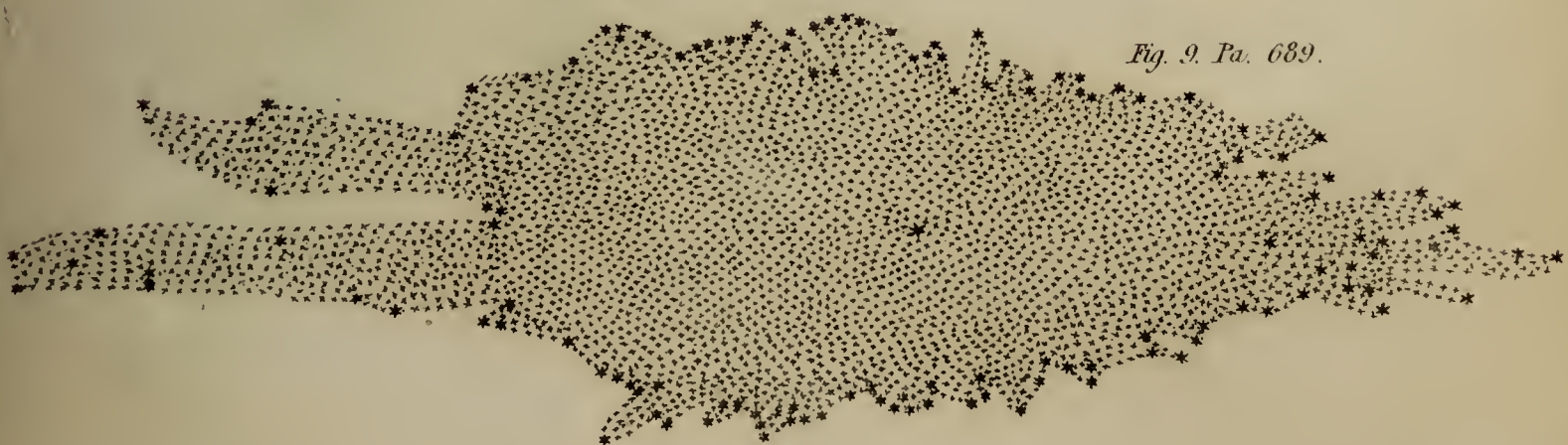


Fig. 9. Pa. 689.







THE  
PHILOSOPHICAL TRANSACTIONS

OF THE  
ROYAL SOCIETY OF LONDON;

ABRIDGED.

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*I. Natural History and Description of the Tyger-cat of the Cape of Good Hope.*  
*By John Reinhold Forster, LL. D., F.R. and A.S. Vol. LXXI. Anno*  
*1781. p. 1.*

Few tribes of quadrupeds have in Africa more representatives of their different species than that of the Cat. The genus of Antelopes may perhaps be excepted, since, to my knowledge, says Dr. F., about 20 different Ghazels and Antelopes are to be met with in Africa; but no more than about 8 or 9 of the cat tribe have hitherto been discovered on that continent. However, I know about 21 different species of this great class; and I suppose these by no means exhaust it.

The greater and more numerous the different genera of animals are, the more difficult it must be to the natural historian properly to arrange the whole of such an extensive division of animals, especially if they are not equally well known. To form new genera, in order to arrange them, is a remedy which increases the evil, instead of curing it. The best method therefore is to make great divisions in each genus, comprehending those species which, on account of some common relation or character, have a greater affinity to each other. The genus of cat offers 3 very easy and natural subdivisions. The first comprehends animals related to the cat tribe, with long hair or manes on their necks; secondly, such as have remarkable long tails without any marks of a mane on their necks; lastly, such as have a brush of hair on the tips of their ears, and shorter tails than the second subdivision. I shall confine myself to that species which has been hitherto imperfectly known to naturalists.

The first notice we had of the Cape Cat is to be met with in Labat's Relation Historique de l'Ethiopie occidentale, tom. 1, p. 177, taken as is supposed from Father Carazzi. Labat mentions there the 'Nsussi, a kind of wild cat of the size of a dog, with a coat as much striped and varied as that of a tyger. Its appearance bespeaks cruelty, and its eyes fierceness; but it is cowardly, and gets its prey only by cunning and insidious arts. All these characters are perfectly

applicable to the Cape Cat, and it seems the animal is found in all parts of Africa, from Congo to the Cape of Good Hope, in an extent of country of about  $11^{\circ}$  of latitude. Kolbe, in his *Present State of the Cape of Good Hope*, vol. 2, p. 127, of the English edition, speaks of a Tyger Bush-cat, which he describes as the largest of all the wild cats of the Cape-countries, and is spotted something like a Tyger. A skin of this animal was seen by Mr. Pennant, in a furrier's shop in London, who thought it came from the Cape of Good Hope; from this skin Mr. Pennant gave the first description which could be of any utility to a natural historian.\* All the other authors mention this animal in a vague manner.

When I touched the 2d time at the Cape of Good Hope in the year 1775, an animal of this species was offered me to purchase; but I refused buying it because it had a broken leg, which made me apprehensive of losing it by death during the passage from the Cape to London. It was very gentle and tame. It was brought in a basket to my apartment, where I kept it above 24 hours, which gave me the opportunity of describing it, and of observing its manners and economy. These I found perfectly analogous to those of our domestic cats. It ate fresh raw meat, and was much attached to its feeders and benefactors: though it had broken the fore-leg by accident, it nevertheless was very easy. After it had been several times fed by me, it soon followed me like a tame favourite cat. It liked to be stroked and caressed; it rubbed its head and back always against the person's clothes who fed it, and desired to be made much of. It purred as our domestic cats do when they are pleased. It had been taken when quite young in the woods, and was not above 8 or 9 months old; I can however positively aver, having seen many skins of full-grown Tyger-cats, that it had already very nearly, if not quite, attained its full growth. I was told that the Tyger-cats live in mountainous and woody tracts, and that in their wild state they are very great destroyers of hares, rabbits, yerbuas, young antelopes, lambs, and of all the feathered tribe.

*Description of the Cape Tyger-Cat.*—Cat with subelongated, annulated tail, and fulvous body, marked above by lengthened and beneath by orbicular spots, with black ears marked by a white lunated spot.

The body is ovate, and elegant: on the neck, rising between the bases of the ears, are four longitudinal deep-black lines or stripes, which on the back are broken or interrupted: the upper parts of the sides are marked by oblong, linear, oblique spots: the lower parts of the sides are marked by round scattered spots: the abdomen is of a cinereous white, with small, round, scattered, black spots.

This animal is the *'Nussi*. Labat *Ethiop. occident.* tom. 1. p. 177.

*Tyger-Bosch-Katten.* Kolbe *Cape of Good Hope*, vol. 2. p. 127. Engl. edit.

*Cape-Cat.* Pennant *Synops. Quadr.* p. 181.—Measure, from the nose to the base of the tail 18 inches; the tail 8 inches. See fig. 1, pl. 1.

\* Pennant's *Synopsis of Quadrupeds*, p. 181, first edit.—Orig.



*II. Experiments and Observations on the Specific Gravities and Attractive Powers of various Saline Substances. By Richard Kirwan, Esq., F. R. S. p. 7.*

The doctrine of chymical affinities has received great improvements from the labours of Mr. Bergman of Upsal, and the still later researches of Mr. Wentzel; but the order of these attractions has hitherto been the only point attended to by these philosophers, as well as by most preceding chymists; for I know of none (says Mr. K.) except Mr. Morveau of Dijon, who has thought of ascertaining the various degrees of force of chemical attraction, by which one body acts on various other bodies, or even on the same body in various circumstances. He has however so ably shown the advantages arising from such an inquiry, that I have made it the object of my attention, and bestowed much pains on it for some time past; and have thence been enabled to determine pretty exactly the proportion of the ingredients of many neutral salts, and the specific gravity of the mineral acids in their purest state, and free from all water. The principles on which these determinations are founded are the following.

1st. That the specific gravity of bodies is as their weight, divided by the weight of an equal bulk of rain or distilled water, this being at present the standard with which every other body is compared.

2dly. That if bodies, specifically heavier than water, be weighed in air and in water, they lose in water part of the weight they were found to have in air; and that the weight so lost is just the same as that of an equal bulk of water, and consequently that their specific gravity is equal to their weight in air, or absolute weight, divided by their loss of weight in water.

3dly. That if a solid, specifically heavier than a liquid, be weighed first in air, and then in that liquid, the weight it loses is equal to the weight of an equal volume of that liquid; and consequently if such solid be weighed first in air, then in water, and afterwards in any other liquid, the specific gravity of this liquid will be as the weight lost in it by such solid, divided by the loss of weight of the same solid in water. This method of finding the specific gravity of liquids I have found much more exact than that by the areometer, or the comparison of weights of equal measures of such liquids and water, both of which are subject to several inaccuracies.

4thly. That where the specific gravity of bodies is already known, the weight of an equal bulk of water may also be found, it being as the quotient of their absolute weight divided by their specific gravity. This I shall call their loss of weight in water.

Hence, where the specific gravity and absolute weight of the ingredients of any compound are known, the specific gravity of such compound may easily be calculated, as it ought to be intermediate between that of the lighter and that of the heavier, according to their several proportions: this I call the mathematical



specific gravity. But, in fact, the specific gravity of compounds, found by actual experiment, seldom agrees with that found by calculation, but is often greater without any diminution of the lighter ingredient. This increase of density must then arise from a closer union of the component parts to each other than either had separately with its own integrant parts; and this more intimate union must proceed from the attraction or affinity of these parts to each other: I therefore imagined that this attraction might be estimated by the increase of density or specific gravity, and that it was proportionable to it, but was soon undeceived.

I must also premise, that the absolute weights of many sorts of air have been accurately determined by Mr. Fontana, the thermometer being at  $55^{\circ}$ , and the barometer at  $29\frac{1}{2}$  inches, or nearly so. Their weights were as annexed:

	Grains.
Cubic inch of common air . . . .	0.385
Fixed air . . . . .	0.570
Marine Air . . . . .	0.654
Nitrous air . . . . .	0.399
Vitriolic air . . . . .	0.778
Alkaline air . . . . .	0.2
Inflammable air . . . . .	0.035

*Of spirit of salt.*—From the time I first read in Dr. Priestley's Experiments on Air (that inexhaustible source of future discoveries) of the exhibition of marine acid in the form of air, free from water; and that this air, reunited with water, formed an acid liquor in all respects the same as common spirit of salt; I conceived the possibility of discovering the exact quantity of acid in spirit of salt of any given specific gravity, and by means of this the exact proportion of acid in all other acid liquors; for if a given quantity of pure fixed alkali were saturated, first by a certain quantity of spirit of salt, and then by determined quantities of the other acids, I concluded, that each of these quantities of acid liquor must contain the same quantity of acid; and this being known, the remainder being the aqueous part, this also must be known; but this conclusion entirely rested on the supposition that the same quantity of all the acids was requisite for the saturation of a given quantity of fixed alkali; for if such given quantity of fixed alkali might be saturated by a smaller quantity of one acid than of another, the conclusion fell to the ground. This point might indeed be in some measure determined by weighing the neutral salts, formed by these acids, when thoroughly dry; but still a source of inaccuracy remained: for if they were exposed to a considerable heat, part of the acid would necessarily be expelled, and more of one acid than of another; and if the heat were not considerable, much of the water of crystallization would remain; so that, if the weights were found to be equal, this equality could not be ascribed to equal quantities of acid, but might perhaps arise from a smaller proportion of acid in one of them, and a larger proportion of water, and in another from a larger proportion of acid and a smaller proportion of water; and if the weights were unequal, no certain conclusion could be drawn. To obviate this difficulty, I used the following expedient. 1st. I supposed the quantities of nitrous and vitriolic acids, necessary to



saturate a given quantity of fixed alkali, exactly the same as that of marine acid whose quantity I determined; and to prove the truth of this supposition, I observed the specific gravity of the spirit of nitre and oil of vitriol I made use of, and in which I supposed, from the trial with alkalis, a certain proportion of acid and water; to these I then added more acid and water, and calculated what their specific gravities should be on the above supposition; and finding the result to accord with the supposition, I concluded the latter to be exact.

The experiments made on the marine acid were as follow: I took 2 bottles, which I filled nearly to the top with distilled water, of which they contained in all 1399.9 gr. and introduced them successively into 2 cylinders filled with marine air, which I had obtained from common salt by means of diluted oil of vitriol and heat, in a mercurial apparatus; and this process I renewed till the water had imbibed, in 18 days, about 794 cubic inches of the marine air. The thermometer did not rise all this time above  $55^{\circ}$ , nor sink, unless perhaps at night, under  $50^{\circ}$ , and the barometer was between 29 and 30 inches. This water, or rather spirit of salt, I then found to weigh 1920 gr. that is 520.1 more than before. The quantity of marine air absorbed amounted then to 520.1 gr. I then examined the specific gravity of this spirit of salt, and found it to be 1.225. Its loss of weight in water (that is, the weight of an equal bulk of water) should then be 1567.346 gr. nearly; but it contained only, as we have seen, 1399.9 gr. of water: therefore, subtracting this from 1567.346, the remainder (that is, 167.446) must be the loss of 520.1 gr. of marine acid; and consequently the specific gravity of the pure marine acid, in such a condensed state as it is in when united to water, must be  $\frac{520.1}{167.446} = .3100$ . But still it might be suspected, that the density of this spirit did not entirely proceed from the mere density of the marine acid; but in part also from the attraction of this acid to water, and though the length of time requisite to make water imbibe this quantity of acid made me judge that the attraction was not very considerable, yet the following experiment was more satisfactory.

I exposed 1440 gr. of this spirit to marine air for 5 days, the thermometer being at  $50^{\circ}$  or under; it then weighed 1562 gr. and consequently imbibed 122 gr. of marine air; its specific gravity was then 1.253, which agrees exactly with what it should be by calculation. Being now satisfied I had discovered the proportion of acid and water in spirit of salt, I was impatient to find it in other acids also; and for that purpose I took 180 gr. of very strong oil of tartar per deliquium, but of whose specific gravity I can find no note, and found it to be saturated by 180 gr. of spirit of salt, whose specific gravity was 1.225. Now, by calculation it appears, that 180 gr. of this spirit contains 48.7 gr. of acid, and 131.3 of water; and hence I drew up the following table.

Marine acid.	Water.	Specific Gravity.
Parts.	Parts.	
	50	1.497
	60	1.431
	70	1.381
	80	1.341
	90	1.308
	100	1.282
	110	1.259
	120	1.246
	130	1.223
	140	1.209
	150	1.196
	160	1.185
	170	1.175
	180	1.166
	190	1.158
	200	1.151
	210	1.144
	220	1.138
48.7	230	1.132
	240	1.127
	250	1.122
	260	1.118
	270	1.114
	280	1.110
	290	1.106
	300	1.103
	310	1.100
	320	1.097
	330	1.091
	340	1.089
	350	1.086
	360	1.084
	370	1.082
	380	1.080
	390	1.078
	400	1.076
	410	1.074

The specific gravity of the strongest spirit of salt, made in the usual way, is, according to Mr. Baume, 1.187, and according to Mr. Bergman, 1.190; but we read in the Paris Memoirs for the year 1700, p. 191, that Mr. Homberg passed a spirit whose specific gravity was 1.300; and that made by Dr. Priestley (vol. 3, p. 275) must have been about 1.500. Hence we see that spirit of salt, whose specific gravity is 1.261 or less, has little or no attraction with water, and therefore attracts none from air, and on that account does not heat a thermometer whose ball is dipped in it as spirit of vitriol and spirit of nitre do, as has lately been observed by the Friendly Society of Berlin.

This table is not exactly accurate, as I had not in this first experiment found the point of saturation so nicely as was requisite. However, I have not corrected it, as the error is but small, and the proportion may at any time be found by calculation; at least when the specific gravity of this spirit does not exceed 1.253. Whether the mathematical specific gravity and that by observation differ in the higher degrees of specific gravity, I have not examined; but the table is formed on the supposition that they do not.

Common spirit of salt is always adulterated with vitriolic acid, and therefore not fit for these trials. Intending to determine by this experiment the proportion of acid, water, and fixed alkali in digestive salt, as it is called, I took 100 gr. of a solution of a tolerably pure vegetable alkali that had been 3 times calcined to whiteness, the specific gravity of which solution was 1.097. I also diluted the spirit of salt with different portions of water; the specific gravity of one sort was 1.115, and of another 1.098. I then found that the above quantity of the solution of a vegetable alkali required for its saturation 27 gr. of that spirit of salt whose specific gravity was 1.098, and 23.35 gr. of that spirit of salt whose specific gravity was 1.115. Now, 27 gr. of spirit of salt, whose specific gravity is 1.098, contain 3.55 gr. of marine acid, as appears by calculation. As the principle on which this calculation, by which the proportion of



substances in alloy is found, may not be generally known, I shall here mention them in the words of Mr. Cotes.

“The data requisite are the specific gravities of the mixture and of the two ingredients. . . . Then, as the difference of the specific gravities of the mixture and the lighter ingredient is to the difference of the specific gravities of the mixture and the heavier ingredient, so is the magnitude of the heavier to the magnitude of the lighter ingredient. Then, as the magnitude of the heavier multiplied into its specific gravity, is to the magnitude of the lighter multiplied into its specific gravity, so is the weight of the heavier to the weight of the lighter. Then, as the sum of these weights is to the given weight of either ingredient, so is the weight given, to the weight of the ingredient sought.” Thus, in this case,  $1.098 - 1.000 = .098$  is the magnitude of the heavier ingredient, viz. the marine acid; and  $.098 \times 3.100 = 0.3038$  the weight of the marine acid; and, on the other hand,  $3.100 - 1.098 = 2.002$  the magnitude of the water, also  $2.002 \times 1.000 = 2.002$  its weight; the sum of these weights is 2.3058: then, if 2.3058 parts of spirit of salt contain 0.3038 parts acid, 27 gr. of this spirit of salt will contain 3.55 acid. In the same manner it will be found, that 23.35 gr. of spirit of salt, whose specific gravity was 1.115, contained 3.55 gr. acid.

The point of saturation was pretty accurately found by putting the glass cylinder which contained the alkaline solution on the scale of a very sensible balance, and at the same time weighing the acid liquor in another pair of scales, when the loss of weight indicated the escape of nearly equal quantities of the fixed air contained in the solution; then the acid was gradually added, by dipping a glass rod into it, to the top of which a small drop of acid adhered: with this the solution was stirred, and very small drops taken up and laid on bits of paper stained blue with radish juice. As soon as the paper was in the least reddened, the operation was completed, so that there was always a very small excess of acid, for which half a grain was constantly allowed; but no allowance was made for the fixed air, which always remains in the solution; but as, on this account, only a small quantity of the alkaline solution was used, this proportion of fixed air must have been inconsiderable. If an ounce of the solution had been employed, this inappreciable portion of fixed air would be sufficient to cause a sensible error: for I judged of the quantity of fixed air lost by the difference between the weight added to the 100 gr. and the actual weight of the compound. When this difference amounted to 2.2 gr. I then judged the whole of the fixed air expelled, and found it to be so, as 100 gr. of this alkaline solution, being evaporated to dryness in a heat of  $300^{\circ}$ , left a residuum which amounted to  $10\frac{1}{2}$  gr.; which  $10\frac{1}{2}$  gr. contained 2.2 gr. of fixed air, as will hereafter be seen.

Hence 8.3 gr. of pure vegetable fixed alkali, free from fixed air and water, or



10.5 of mild fixed alkali, were saturated by 3.55 gr. of pure marine acid, and consequently the resulting neutral salt should, if it contained no water, weigh 11.85 gr.; but the salts resulting from this union (the solution being evaporated to perfect dryness in a heat of  $160^{\circ}$  kept up for 4 hours) weighed at a medium 12.66 gr. Of this weight, 11.85 gr. were acid and alkali; therefore the remainder, viz. 0.81 gr. were water; therefore 100 gr. of perfectly dry digestive salt contain 28 gr. acid, 6.55 water, and 65.4 of fixed alkali.

I was then curious to compare my experiments with those made by others, but could not find any made with sufficient precision except those of Mr. Homberg in the Paris Memoirs for 1699. However, as to spirit of salt I did not think proper to compare them, as he mentions that his could dissolve gold, and therefore was probably impure.

*Of spirit of nitre.*—The common reddish brown or greenish spirit of nitre containing, besides acid and water, a certain portion of phlogiston; and being also mixed with some portion of the acid of sea salt, I judged it unfit for these trials; and therefore used only the dephlogisticated sort, which is quite colourless, and resembles pure water in its appearance. This pure acid cannot be made to exist in the form of air, as Dr. Priestley has shown; for when it is deprived of water and phlogiston, and furnished with a due proportion of elementary fire, it ceases to have the properties of an acid, and becomes dephlogisticated air: I could not therefore determine its proportion in spirit of nitre, as I had done that of the marine acid, but was obliged to use another method. 1st. To 1963.25 gr. of this spirit of nitre, whose specific gravity was 1.419, I gradually added 179.5 gr. of distilled water: and when it cooled I found the specific gravity of this mixture 1.389. 2dly. To 1984.5 gr. of this I again added 178.75 gr. of water; its specific gravity was then 1.362.

I then took 100 gr. of a solution of fixed vegetable alkali, whose specific gravity was 1.097, the same as I had before used in the trials with spirit of salt, and found this quantity of alkali to be saturated by 11 gr. of the spirit of nitre, whose specific gravity was 1.419; and by 12 gr. of the spirit, whose specific gravity was 1.389; and by 13.08 of that whose specific gravity was 1.362. The quantities here mentioned were the mediums of 5 experiments. I found it necessary to dilute the nitrous acid with a small proportion of water, of which I kept an account. When I neglected this precaution, I found that part of the acid was phlogisticated, and went off with the fixed air. Note also, that after each affusion of acid, 10 minutes were allowed for the matters to unite; a precaution which I also found absolutely necessary.

Hence, on the supposition that a given quantity of fixed vegetable alkali is saturated by the same weight of both acids, we see that 11 gr. of spirit of nitre, whose specific gravity is 1.419, contain the same quantity of acid as 27 gr. of



spirit of salt, whose specific gravity is 1.098, that is, 3.55 gr.; the remainder of 11 gr. is therefore mere water, viz. 7.45 gr.; consequently, if the density of the acid and water had not been increased by their union, the specific gravity of the pure and mere nitrous acid should be 11.8729; for the specific gravity of this acid should be as its absolute weight divided by its loss of weight in water, and this loss should be as the total loss of these 11 gr. minus the loss of the aqueous part. Now the total loss  $= \frac{11}{1.419} = 7.749$ , and the loss of the aqueous part  $= 7.45$ , consequently the loss of the acid part is  $7.749 - 7.45 = 0.299$ , and therefore the specific gravity of the acid part, that is, of the pure nitrous acid, is  $\frac{3.55}{0.299} = 11.8729$ . But it is well known, that the density of the nitrous acid, as well as that of the vitriolic, is increased by its union with water; and therefore the loss above found is not the whole of its real loss in its natural state (if it could be so found) but partly the loss that arises from the density that accrues to it from its union with water; for since its density is increased by this union, its loss is less than it would be if the nitrous acid had only its own proper density, and consequently the specific gravity above found is greater than its real specific gravity.

To determine therefore the real specific gravity of this acid in its natural state, the quantity of accrued density must be found, and subtracted from the specific gravity of the spirit of nitre, whose true mathematical specific gravity will then appear. I endeavoured to effect this by mixing different portions of spirit of nitre and water, remarking the diminution of their joint volume below the sum of the spaces occupied by their separate volumes; but could never attain a sufficient degree of precision. The following method, though not exactly accurate, I found more satisfactory; 12 gr. of the spirit of nitre, whose specific gravity by observation was 1.389, contained, as I supposed from the former experiment, 3.55 gr. of acid, and 8.45 of water; then if the specific gravity of the pure nitrous acid were 11.872, the specific gravity of this compound of acid and water should be 1.371; for the loss of 3.55 gr. acid should be 0.299, and the loss of the water 8.45; the sum of the losses 8.749;  $\frac{12}{8.749} = 1.371$ ; but, as already said, the specific gravity by observation was 1.389, therefore the accrued density in this case was at least .018, the difference between 1.389 and 1.371. I say at least, for as the specific gravity 11.872 was certainly too high, the loss of 3.55 gr. acid was certainly too small; and if it were greater, the mathematical specific gravity 1.371 would have been still lower. However, .018 is certainly a near approximation to the degree of density that accrues to 3.55 gr. acid by their union with 7.45 gr. of water, and differs inconsiderably from the truth, as will appear by the sequel; therefore, subtracting this quantity from 1.419, we have nearly the mathematical specific gravity of that proportion of acid and water,



namely, 1.401. And since 11 gr. of this spirit of nitre contain 3.55 gr. of acid and 7.45 of water, its loss of weight should be  $\frac{11}{1.401} = 7.855$ ; and subtracting the loss of the aqueous part from this, the remainder 0.405 is the loss of the 3.55 gr. acid, and consequently the true specific gravity of the pure and mere nitrous acid is  $\frac{3.55}{0.405} = 8.7654$ . This being settled, the mathematical specific gravity, and true increase of density of the above mixtures, will be found. Thus the mathematical specific gravity of 12 gr. of that spirit of nitre, whose specific gravity by observation was 1.389, must be 1.355, supposing it to contain 3.55 gr. acid and 8.45 of water; for the loss of 3.55 gr. acid is  $\frac{3.55}{8.763} = 0.405$ , and the loss of water 8.45; the sum of these losses is 8.855. Then  $\frac{12}{8.855} = 1.355$ , and consequently the accrued density is  $1.389 - 1.355 = .034$ . In the same manner it will be found, that the mathematical specific gravity of 13.08 gr. of that spirit of nitre whose specific gravity by observation was 1.362, must be 1.315, and consequently its accrued density .047.

But the whole still rests on the supposition that each of these portions of spirit of nitre contain 3.55 gr. of acid. To verify this supposition, I could think of no better method than that of examining the mathematical specific gravities of the first mixture I had made of spirit of nitre and water in large quantities; for if the mathematical specific gravities of these agreed exactly with those of the quantities I had supposed in smaller portions of each, I could not but conclude, that the supposition of such proportions of acid and water, as I had determined in each, was just; and that this was the case will appear by the following calculations. 1st. When to 1963.25 gr. of spirit of nitre, whose specific gravity was 1.419, were added 179.5 gr. of water, the quantity of acid on the above supposition should be 634.53 gr.; for  $11 : 3.55 :: 1963.25 : 634.53$ ; the quantity of water in those 1963.25 gr. of spirit of nitre should then be 1328.72, and after adding 179.5 gr. of water, the whole quantity of acid and water should be 2142.75; the loss of acid was  $\frac{634.53}{8.7654} = 71.24$ , and the sum of the losses 1580.46: then the mathematical specific gravity should be  $\frac{2142.75}{1580.46} = 1.355$ , which is exactly the same as that which was found in 12 gr. of this spirit of nitre, on the supposition that they contained 3.55 gr. of acid.

Again: when to 1984.5 gr. of this mixture I added 178.75 gr. of water, the whole quantity of diluted spirit of nitre was 2163.25 gr. and the quantity of acid in 1984.5 gr. was 587.081 gr. for  $12 : 3.55 :: 1984.5 : 587.081$ ; the loss of this quantity of acid is 66.96 gr. and the sum of the losses of acid and water is 1643.129 gr.; and consequently the mathematical specific gravity should be  $\frac{2163.75}{1643.125} = 1.315$ , which is the same as that determined in 13.08 gr. of the same mixture.



By continuing these mixtures till I found the mathematical specific gravity and that by observation nearly to coincide, I was enabled to draw up the following table, in which if any errors be found, I hope they will be excused, from the impossibility of avoiding them where the weights must be found with such extreme precision: the two first series were only found by analogy.

Spirit of nitre.	Acid.	Water.	Accrued density.	Mathem. specific gravity.	Spec. gra- vity by observat.	Attract. of the acid to water.	Attract. of wat. to the acid.
Grs.	Grs.	Grs.					
9	— —	5.45	.600	1.537	1.537	— —	— —
10	— —	6.45	.009	1.458	1.467	.009	.054
11	— —	7.45	.018	1.401	1.419	.018	.045
12	— —	8.45	.034	1.355	1.389	.027	.036
13.08	— —	9.53	.047	1.315	1.362	.036	.027
14.15	— —	10.6	.051	1.286	1.337	.045	.018
15.23	— —	11.68	.054	1.260	1.314	.	.009
16.305	— —	12.755	.054	1.238	1.292	.054	.009
17.38	— —	13.83	.051	1.220	1.271		
18.445	— —	14.9	.047	1.205	1.252		
19.53	— —	15.98	.044	1.191	1.235		
20.605	— —	17.055	.042	1.180	1.222		
21.68	— —	18.13	.040	1.177	1.217		
22.755	— —	19.205	.038	1.160	1.198		
23.83	— —	20.28	.036	1.152	1.188		
24.905	— —	21.45	.033	1.144	1.177		
26.17	— —	22.62	.030	1.132	1.162		
27.34	3.55	23.79	.027	1.130	1.157		
28.51	— —	24.96	.026	1.124	1.150		
29.68	— —	26.13	.024	1.114	1.138		
30.85	— —	27.30	.022	1.113	1.135		
32.02	— —	28.47	.020	1.109	1.129		
33.09	— —	29.54	.018	1.102	1.120		
34.26	— —	30.71	.016	1.101	1.117		
35.43	— —	31.88	.014	1.097	1.111		
36.60	— —	33.05	.012	1.094	1.106		
37.77	— —	34.22	.010	1.090	1.100		
38.94	— —	35.39	.008	1.088	1.096		
40.11	— —	36.56	.006	1.085	1.091		
41.28	— —	37.73	.004	1.082	1.086		
42.45	— —	38.90	.002	1.080	1.082		

The intermediate specific gravities may be found by taking an arithmetical mean among the specific gravities by observation between which that sought lies, and noting how much it exceeds or falls short of such arithmetical mean; and then taking also an arithmetical mean among the mathematical specific gravities between which that sought for must lie, and a proportionate excess or defect. I have added a column of attraction of the nitrous acid to water, as far as it keeps pace with the increase of density, but no further, as I am unacquainted with the law of its further increase. The specific gravity of the strongest spirit of nitre yet made is, according to Mr. Baume, 1.500; and according to Mr. Berg-



man, 1.586. I next proceeded to examine the proportion of acid, water, and fixed alkali, in nitre, in the same manner as I had before done that in digestive salt, and found that 100 gr. of perfectly dry nitre, contain 28.48 gr. of acid, 5.2 of water, and 66.32 of fixed alkali.

I shall now compare the result of these experiments with those of Mr. Homberg. The specific gravity of the spirit of nitre which Mr. Homberg made use of, was 1.349; and of this, he says, 1 oz. 2 dr. and 36 gr., that is, 621 Troy, are requisite to saturate 1 French oz. (472.5 Troy) of dry salt of tartar; according to my computation, 613 gr. are sufficient; for this specific gravity lies between the tabular specific gravities by observation, 1.362 and 1.337, and is nearly an arithmetical mean between them. The corresponding mathematical specific gravity lies between the tabular quantities 1.315 and 1.286, and is nearly 1.300. Now the proportion of acid and water in this is 2.629 of acid, and 7.465 of water; for  $8.765 - 1.300 = 7.465$  water, and  $8.765 \times .300 = 2.629$  of acid; and the sum of both is 10.044. Now, since 10.5 gr. mild vegetable fixed alkali require 3.55 gr. of acid for their saturation, 472.5 will require 159.7; therefore if 10.044 gr. of nitre contain 2.629 gr. acid, the quantity of this spirit of nitre requisite to give 159.7, will be 613.2 nearly; and hence the difference between us is only about 8 gr.

2dly. Mr. Homberg says, he found his salt, when evaporated to dryness, to weigh 186 gr. more than before; whereas, by my experiment, it should weigh but 92.8 gr. more than at first. I shall mention the cause of this difference in treating of tartar vitriolate, for it cannot be entirely attributed to the difference of evaporation. 3dly. Mr. Homberg infers, that 1 oz. (that is, 472.5 Troy gr.) of this spirit of nitre contains 141 gr. Troy of real acid: by my computation it contains but 123.08 gr. of real acid. This difference evidently proceeds from his neglecting the quantity of water that certainly enters into the composition of nitre; for he proceeds on this analogy,  $621 : 186.6 :: 472.5 : 141$ .

The proportion of fixed alkali I have assigned to nitre is fully confirmed by a very curious experiment of Mr. Fontana's, inserted in Rozier's Journal for November 1778. This ingenious philosopher decomposed 2 oz. of nitre by distilling it in a strong heat for 18 hours. After the distillation there remained in the retort a substance purely alkaline, amounting to 10 French dr. and 12 gr. Now 2 French oz. = 944 gr. Troy, and the alkaline matter amounts to 607 gr. Troy; and, according to my computation, 944 gr. of nitre should contain 625 of alkali. So small a difference may fairly be attributed to the loss in transferring from one vessel to another, weighing, filtering, evaporating, &c.

Mr. Lavoisier, in the Paris Memoirs for the year 1776, has given us, after Dr. Priestley, the analysis of the nitrous acid. In 2 oz. French measure (= 945 gr. troy) of spirit of nitre, whose specific gravity was 1.3160, he dis-



solved 2 oz. and 1 dr. of mercury; the quantity of air obtained during the solution was 190 cubic inches French (= 202.55 English). This air was all nitrous. There remained a white mercurial salt, which, being distilled, afforded 12 cubic inches (= 12.785 English) of air mixed with red vapours, and which differed little from common air. There afterwards arose 224 cubic inches (= 238.56 English) of dephlogisticated air, during the production of which, the mercury was almost revived, there remaining but a few grains of a yellow sublimate. The 12 inches of air mixed with red vapours arose, he says, from a mixture of 36 cubic inches of nitrous air (= 38.34 English) and 14 of dephlogisticated air (14.91 English); and as the mercury was almost wholly revived, he concludes, that these airs arose from the nitrous acid, and formed it; and hence infers, that 16 oz. of this spirit of nitre (= 7560 gr. troy) contained 13 oz. 7 dr.  $36\frac{2}{3}$  gr. (that is, 6589 gr. troy) of water, and consequently only 971 gr. troy of real acid, and therefore 2 oz. of this spirit of nitre contained but 120 gr. troy of real acid: but, by my calculation, 2 oz. of this spirit of nitre contained 213 gr. acid; for its mathematical specific gravity is 1.265. The same weight of acid will also be found in it by computing the weight of the volumes of the different airs: he himself found it consist of, or at least to afford by its decomposition; for 202.55 cubic inches of nitrous air weigh, by Mr. Fontana's experiment, 80.8174 gr. troy, and 238.56 inches of dephlogisticated air weigh 100.1952 gr. troy, and adding to these the weight of 38.34 inches of nitrous air, and of 14.91 of dephlogisticated air, which made the 12 cubic inches of air mixed with red vapours, we shall find the whole weight of these airs to be 202.181 gr. the few grains wanting of 213 gr. may be accounted for from the absorption of the water in which he received the airs, and by allowing for that still remaining in the yellow sublimate.

*Of oil of vitriol.*—The oil of vitriol I made use of was not perfectly dephlogisticated; but though pale, yet a little inclined to red. It contained some whitish matter, as I perceived by its becoming milky on the affusion of pure distilled water. How far this may alter the result of the following experiments I have not tried; but believe it to be as pure as that which is commonly used in experiments, and therefore the fittest for my purpose.

To 2519.75 gr. of this oil of vitriol, whose specific gravity was 1.819, I gradually added 180 gr. of distilled water, and 6 hours after found its specific gravity to be 1.771. To this mixture I again added 178.75 gr. of water, and found its specific gravity, when cooled to the temperature of the atmosphere, to be 1.719: it was then milky. I then saturated the same quantity of the oil of tartar abovementioned, with each of these sorts of oil of vitriol in the manner already mentioned, and found the saturation to be effected (taking the medium of 5 experiments) by 6.5 gr. of that whose specific gravity was 1.819; by 6.96



gr. of that whose specific gravity was 1.771; and by 7.41 of that whose specific gravity was 1.719.

I was obliged to add a certain proportion of water to each of these sorts of oil of vitriol; for when they were not diluted, I perceived that part of the acid was phlogisticated, and went off with the fixed air; but knowing the quantity of water that was added, it was easy to find, by the rule of proportion, the quantity of each sort of oil of vitriol that was taken up by the alkali. Hence I supposed that each of these quantities of oil of vitriol, of different densities, contained 3.55 gr. of acid, as they saturated the same quantity of vegetable fixed alkali as 11 gr. of spirit of nitre, which contained that quantity of acid.

I then endeavoured to find the specific gravity of the pure vitriolic acid, in the same manner as I before had that of the nitrous, as it cannot be had in the shape of air unless united to such a quantity of phlogiston as quite alters its properties. The loss of 6.5 gr. of oil of vitriol, whose specific gravity is 1.819, is  $\frac{6.5}{1.819} = 3.572$ ; but as these 6.5 gr. contained, besides 3.55 gr. acid, 2.95 of water, the loss of this must be subtracted from the entire loss, and then the remainder 0.622 is the loss of the pure acid part, in that state of density to which it is reduced by its union with water. The specific gravity therefore of the pure vitriolic acid, in this state of density, is  $\frac{3.55}{0.622} = 5.707$ . But to find its natural specific gravity, we must find how much its density is increased by its union with this quantity of water: and in order to observe this, I proceeded as before with the nitrous acid. Thus, 6.96 gr. of oil of vitriol, whose specific gravity was 1.771, contained 3.55 gr. acid, and 3.41 of water; then its specific gravity by calculation should be 1.726, for the loss of 3.55 gr. acid is  $\frac{3.55}{5.707} = .0622$ ; the loss of 3.41 gr. water is 3.41; the sum of the losses 4.032. Then  $\frac{6.96}{4.032} = 1.726$ ; therefore the accrued density is  $1.771 - 1.726 = .045$ . Taking this therefore from 1.819, its mathematical specific gravity will be 1.774; then the loss of 6.5 gr. of oil of vitriol, whose specific gravity, by observation, is 1.819, will be found to be  $\frac{6.5}{1.774} = 3.664$ ; but of this, 2.95 gr. are the loss of the water it contains, and the remainder 0.714\* the loss of the mere acid part. Then  $\frac{3.55}{0.715} = 4.9649$  is nearly the true specific gravity of the pure vitriolic acid. I then found the true increase of density arising from the union of the vitriolic acid and water in the foregoing mixtures, and observed, that in oil of vitriol, whose specific gravity was 1.771, it was 0.84, and in that whose specific gravity was 1.719, it was 0.100.

\* By mistake, the following calculations were made on the supposition that the loss was 0.715; the difference being immaterial, the calculations were not repeated.—Orig.



To obtain a synthetical proof of these deductions, I compared them with the specific gravities of the first mixtures I had made: for if these deductions were true, the mathematical specific gravities, and the accrued densities, added to each other, should amount to the same quantity, as the specific gravities by observation; and this I found to happen very nearly: for in the first experiment, where 2519.75 gr. of oil of vitriol, whose specific gravity was 1.819, were mixed with 180 gr. of water, that oil of vitriol contained by my calculation 1376.171 gr. of acid and 1143.597 gr. of water, besides the 180 gr. of water that were added to it, the loss of the acid was  $\frac{1376.171}{4.964} = 277.22$ . The whole quantity of oil of vitriol was 2699.75 gr.; then the sum of the losses was 1600.81; and therefore the mathematical specific gravity  $\frac{2699.75}{1600.81} = 1.686$ ; to which adding 0.84, the degree of accrued density, the specific gravity by observation should be 1.770, which wants less than 1000th part in 2700 of being just. Again: in the mixture whose specific gravity was 1.719, the sum of the losses was 1779.549, and the weight of the whole 2878.4; the mathematical specific gravity should be  $\frac{2878.400}{1779.549} = 1.617$ ; to which adding 0.100, the specific gravity by observation should be 1.717, which is nearly the truth.

By continuing these mixtures till the specific gravities by calculation and observation nearly coincided, I formed the following table. The extra-tabular proportions are to be sought in the manner already shown; the first two series were formed by analogy.

Oil and spirit of vitriol.	Acid.	Water.	Accrued density.	Mathe- mat. spe- cific grav.	Specific gravity by observat.	Attract. of the acid to water.	Attract. of water to the acid.
Grs.	Grs.	Grs.					
5.58	— —	2.03	.000	2.032	2.032		
6.04	— —	2.49	.005	1.884	1.889	.005	0.140
6.5	— —	2.95	.045	1.774	1.819	.045	0.149
6.96	— —	3.41	.084	1.687	1.771	.084	0.139
7.41	— —	3.86	0.100	1.619	1.719	0.100	0.137
7.87	— —	4.32	0.112	1.563	1.675	0.112	0.129
8.33	— —	4.78	0.122	1.515	1.637	0.122	0.122
8.79	— —	5.24	0.129	1.476	1.605	0.129	0.112
9.25	— —	5.70	0.137	1.441	1.578	0.137	0.100
9.71	— —	6.16	0.139	1.412	1.551	0.139	.084
10.17	— —	6.62	0.140	1.385	1.525	0.149	.045
10.63	— —	7.08	0.139	1.363	1.502	0.140	.005
11.09	— —	7.54	0.132	1.343	1.475		
11.55	— —	8.00	0.127	1.325	1.452		
12.01	— —	8.46	0.120	1.308	1.428		
12.47	— —	8.92	0.113	1.294	1.407		
12.93	— —	9.38	0.106	1.280	1.386		
13.39	— —	9.84	0.100	1.268	1.368		
13.85	— —	10.30	0.094	1.257	1.351		
14.31	— —	10.76	0.088	1.247	1.335		
14.77	— —	11.22	.083	1.237	1.320		
15.23	— —	11.68	.078	1.228	1.306		
15.69	3.35	12.14	.074	1.220	1.294		
16.15	— —	12.60	.070	1.212	1.282		
16.61	— —	13.06	.066	1.205	1.271		
17.07	— —	13.52	.062	1.199	1.261		
17.53	— —	13.98	.059	1.191	1.250		
17.99	— —	14.44	.056	1.187	1.243		
18.45	— —	14.90	.053	1.181	1.234		
18.91	— —	15.36	.050	1.176	1.226		
19.37	— —	15.82	.047	1.167	1.214		
19.83	— —	16.28	.044	1.166	1.210		
20.29	— —	16.74	.040	1.162	1.203		
20.75	— —	17.20	.038	1.158	1.196		
21.21	— —	17.66	.035	1.154	1.189		
21.67	— —	18.12	.032	1.150	1.182		
22.13	— —	18.58	.029	1.146	1.175		
22.59	— —	19.04	.026	1.143	1.169		
23.05	— —	19.50	.023	1.140	1.163		
23.51	— —	19.96	.020	1.137	1.157		
23.97	— —	20.42	.018	1.134	1.152		
24.43	— —	20.88	.016	1.131	1.147		
24.89	— —	21.34	.014	1.128	1.142		
25.35	— —	21.80	.012	1.125	1.137		
25.81	— —	22.26	.010	1.123	1.133		
26.27	— —	22.72	.008	1.120	1.128		
26.73	— —	23.18	.006	1.118	1.124		
27.19	— —	23.64	.004	1.116	1.120		
27.65	— —	24.10	.002	1.114	1.116		

The specific gravity of the most concentrated oil of vitriol yet made is, according to Mr. Beaume and Bergman, 2.125.

I ascertained the proportion of acid water and fixed alkali in tartar vitriolate, as



before, in nitre and digestive salt. I found the salts, resulting from the saturation of the same oil of tartar, with portions of oil of vitriol of different specific gravities, to weigh, at a medium, 12.45 gr. Of this weight, only 11.85 gr. were alkali and acid, the remainder therefore was water, viz. 0.6 of a grain; consequently 100 gr. of perfectly dry tartar vitriolate contain 28.51 gr. acid, 4.82 of water, and 66.67 of fixed vegetable alkali. In drying this salt I used a heat of  $240^{\circ}$  to expel the adhering acid more thoroughly, and I kept it in that heat a quarter of an hour.

According to Mr. Homberg, 1 French oz. (or 472.5 gr. troy) of dry salt of tartar required 297.5 gr. troy of oil of vitriol, whose specific gravity was 1.674, to saturate it; but, by my calculation, this quantity of fixed alkali would require 325 gr.: a difference which, considering our different methods of determining the specific gravity of liquids (his method, viz. that by mensuration, giving it always less than mine) the different desiccation of our alkalis, &c. may pass for inconsiderable.

The resulting salt weighed, according to Mr. Homberg, 182 gr. Troy above the original weight of the fixed alkali; but by my experiment it should weigh but 87.7 gr. more; for  $10.5 : 12.45 :: 472.5 : 560.2$ . It is hard to say how Mr. Homberg could find this great excess of weight both in nitre and tartar vitriolate, unless he meant by the original weight of the salt of tartar the weight of the mere alkaline part, distinct from the fixed air it contained: and indeed one would be apt to think he did make this distinction; for in that case the excess of weight will be very nearly such as he determined it: for  $10.5 : 8.3 :: 472.5 : 373.3$ . Now the whole weight of his nitre was 560.2, as above shown; then  $560.2 - 373.3 = 186.9$ , which is only 4 gr. more than he determined it.

Hence he inferred, that 1 oz. (472.5 gr. Troy) of this oil of vitriol contains 291.7 gr. of acid. By my computation it contains but 213.3; but it must be considered that he made no allowance for the water contained in tartar vitriolate, and imagined the whole of the increase of weight proceeded from the acid that is united in it to the fixed alkali. Now the aqueous part in 560 gr. of tartar vitriolate amounts to 37 gr. the remaining difference may be attributed to the different degrees of desiccation, &c.

*Of the acetous acid.*—I have made no experiment on this acid; but, by calculating from the experiment of Mr. Homberg, I find the specific gravity of the pure acetous acid, free from superfluous water, should be 2.130. It is probable that its affinity to water is not strong enough to cause any irregular increase in its density, at least that can be expressed by 3 decimals; and hence its proportion of acid and water may always be calculated from its specific gravity and absolute weight. 100 parts of foliated tartar, or, as it should rather be called, acetous tartar, contain well dried 32 of fixed alkali, 19 of acid, and 49 parts of



water. The specific gravity of the strongest concentrated vinegar yet made, is 1.069. It is more difficult to find the point of saturation with the vegetable than with the mineral acids; because they contain a mucilage that prevents their immediate union with alkalis; and hence they are commonly used in too great quantity. They should be used moderately hot, and sufficient time should be allowed them to unite.

From these experiments it follows: 1st. That fixed vegetable alkalis take up an equal quantity of the 3 mineral acids, and probably of all pure acids; for we have seen that 8.3 grains of pure vegetable alkali, that is, free from fixed air, take up 3.55 gr. of each of these acids; and consequently 100 parts of caustic fixed alkali would require 42.4 parts of acid to saturate them. Now Mr. Bergman has found, that 100 parts of caustic fixed vegetable alkali take up 47 parts of the aerial acid, which, considering his alkali might contain some water, differs but little from my calculation. It should therefore seem, that alkalis have a certain determinate capacity of uniting to acids, that is, to a given weight of acids; and that this capacity is equally satiated by that given weight of any pure acid indiscriminately. This weight is about 2.35 of the weight of the vegetable alkali. 2dly. That the three mineral acids, and probably all pure acids, take up 2.253 times their own weight of pure vegetable alkali, that is, are saturated by that quantity.

3dly. That the density accruing to compound substances, from the union of their component parts, and exceeding its mathematical ratio, increases from a minimum, when the quantity of one of them is very small in proportion to that of the other; to a maximum, when their quantities differ less; but that the attraction, on the contrary, of that part which is in the smallest quantity to that which is in the greater, is at its maximum when the accrued density is at its minimum, but not reciprocally; and hence the point of saturation is probably the maximum of density and the minimum of sensible attraction of one of the parts. Hence no decomposition operated by means of a substance that has a greater affinity with one part of a compound than with the other, and than these parts have to each other, can be complete, unless the minimum affinity of this 3d substance be greater than the maximum affinity of the parts already united. Hence few decompositions are complete unless a double affinity intervenes; and hence the last portion of the separated substance adheres so obstinately to that to which it was first united, as all chemists have observed. Thus, though acids have a greater affinity to phlogiston than the earths of the different metals have to it, yet they can never totally dephlogisticate these earths, but only to a certain degree; so though atmospheric air, and particularly dephlogisticated air, attracts phlogiston more strongly than the nitrous acid does; yet not even dephlogisticated air can deprive the nitrous acid totally of its phlogiston, as is evident



from the red colour of the nitrous acid when nitrous air and dephlogisticated air are mixed together. Hence also mercury precipitated from its solution in any acid, even by fixed alkalis, constantly retains a portion of the acid to which it was originally united, as Mr. Bayen has shown; so also does the earth of alum, when precipitated in the same manner from its solution; and thus several anomalous decompositions may be explained. Indeed, I have reason to doubt whether mercury does not attract acids more strongly than alkalis attract them.

4thly. That concentrated acids are, in some measure phlogisticated, and evaporate by union with fixed alkalis. 5thly. That knowing the quantity of fixed alkali in oil of tartar, we may determine the quantity of real pure acid in any other acid substance that is difficultly decomposed, as the sedative acid, and those of vegetables and animals; for 10.5 gr. of the mild alkali will always be saturated by 3.55 gr. of real acid: and reciprocally, the quantity of acid in any acid liquor being known, the quantity of real alkali in any vegetable alkaline liquor may be found.

*Of the specific gravity of fixed air in its fixed state.*—Being desirous to know the specific gravity of some substances which are difficultly procured, or at least preserved for any time, free from fixed air, such as fixed and volatile alkalis, I was induced to seek the specific gravity of the former in its fixed state, as of an element necessary to the calculation of the latter; it being very evident that its density, in its fixed state, must be very different from that which it possesses in its fluid elastic state. I therefore took a piece of white marble, of the purest kind, which weighed 440.25 gr. and weighing it in water, found it to lose 162 gr.; its specific gravity was therefore 2.7175. Of this marble, reduced to a fine powder, I put 180 gr. into a phial, and expelling the fixed air by the dilute vitriolic acid and heat, I found its quantity amount to 105.28 cubic inches; the thermometer being at  $65^{\circ}$ , and the barometer between 29 and 30 inches; this bulk of air would, at  $55^{\circ}$  of Fahrenheit, occupy but 102.4 cubic inches; at which temperature, according to the experiment of Mr. Fontana, a cubic inch of fixed air, the barometer being at  $29^{\frac{1}{2}}$ , would weigh  $\text{C } 57$  of a grain; therefore the weight of the whole quantity of fixed air amounted to 58.368 gr. which is nearly  $\frac{1}{7}$  of the weight of the marble. At this rate, 100 gr. of the marble contained 32.42 of fixed air.

To determine the proportion of water and calcareous earth, and also the specific gravity of this latter, I put 3009.25 gr. of the same marble finely powdered into a crucible, loosely covered; the crucible and its contents, before calcination, weighed 8394 gr. and after remaining 14 hours in a white heat I found it to weigh 7067.5 gr. The weight of the crucible alone was 5384.75 gr.; therefore the weight of the lime singly was 1682.75 gr. The marble then lost by calcination 1326.5 gr.; 180 gr. of the marble should then lose 79.343

gr. and 100 gr. should lose 44.08 ; but of these 44.08, 32.42 were fixed air, as is already seen, therefore the remainder, that is, 11.66 gr. were water, and the quantity of pure calcareous earth in 100 gr. of the marble was 55.92 gr.

I next proceeded to discover the specific gravity of the lime. Into a brass box, which weighed 607.65 gr. and in the bottom of which a small hole was drilled, I stuffed as much as possible of the finely powdered lime, and then screwed the cover on, and weighed it both in air and water. When immersed in this latter, a considerable quantity of common air was expelled ; when this ceased, I weighed it. The result of this experiment was as annexed :

	Grains.
Weight of the box in air ..	607.65
Its loss of weight in water..	73.75
Weight of the box and lime in air .....	1043.5
Weight of lime singly in air.	435.85
Loss of weight of the box and lime in water .....	256.5
Loss of weight of lime singly	182.3

Hence, dividing the absolute weight of the lime by its loss in water, its specific gravity was found to be 2.3908.

From these data I deduced the specific gravity of fixed air in its fixed state ; for 100 gr. of marble consist of 55.92 of earth, 32.42 of fixed air, and 11.66 of water ; and the specific gravity of the marble is 2.717. Now the specific gravity of the fixed air, in its fixed state, is as its absolute weight divided by its loss of weight in water ; and its loss of weight in water is as the loss of 100 gr. of marble minus the losses of the pure calcareous earth and of the water.

$$\text{Loss of 100 gr. of marble} = \frac{100}{2.717} = 36.8 \text{ gr.}$$

$$\text{Loss of 55.92 gr. calcareous earth} = \frac{55.92}{2.390} = 23.39 \text{ gr.}$$

$$\begin{array}{rcl} \text{Loss of 11.66 gr. water} & = & 11.66 \\ \text{Sum} & & 35.05 \end{array}$$

Then the loss of the fixed air  $36.8 - 35.05 = 1.75$  ; consequently, its specific gravity is  $\frac{32.42}{1.75} = 18.52$  ; by which it appears to be the heaviest of all acids, or even of all bodies yet known, gold and platina excepted.

*Of fixed vegetable alkali.*—As the manner of conducting the experiments made on this salt was nearly the same as that used in the foregoing, except that to find its specific gravity I weighed it in æther instead of water, I shall content myself, to avoid the repetition of tedious calculation, with relating the result of these experiments. 1st. I found that 100 gr. of this alkali contain about 6.7 gr. of earth, which, according to Mr. Bergman, is siliceous : this earth passes the filter with it when the alkali is not saturated with fixed air, so that it seems to be held in solution as in liquor silicum. 2dly. I found, that the quantity of fixed air in oil of tartar and dry vegetable fixed alkali, is various at various times and in various parcels of the same salt ; but that at a medium in the purer alkalis it may be rated at 21 gr. in 100 ; and hence the quantity of this alkali in any solution of it may be very nearly guessed at, by adding a known weight of a



dilute acid to a given weight of such solution, and then weighing it again; for as 21 is to 100, so is the weight lost, to the weight of mild alkali in such solution.

The specific gravity of mild and perfectly dry 4 times calcined fixed alkali, free from siliceous earth, and containing 21 per cent. of fixed air, I found to be 5.0527. When it contains more fixed air, its specific gravity is probably higher, except it were not perfectly dry: whence I inferred the specific gravity of this alkali, when caustic and free from water, to be 4.234.

From the weight of the aerial acid, in its fixed state, it happens, that fixed alkalis, when united to it, are specifically heavier than when united either to the vitriolic or nitrous acids. Thus Mr. R. Watson, in the Phil. Trans. for the year 1770, p. 337, found the specific gravity of dry salt of tartar, including siliceous earth, to be 2.761: whereas the specific gravity of tartar vitriolate was only 2.636, and that of nitre 1.933. The reason why nitre is so much lighter than tartar vitriolate, is, because it contains much more water, and its union with the alkali is less intimate.

Impure vegetable fixed alkalis, such as pearl ash, pot ashes, &c. contain more fixed air, as appears by the experiments of Dr. Lewis. Pearl ash, according to Mr. Cavendish, contains 28.4 or 28.7 per cent. of fixed air. Hence, in lyes of equal specific gravity with those of a purer alkali, the quantity of saline matter will be more probably in the ratio of 28.4 or 28.7 to 21: but this surplus weight is only fixed air.

*III. Account of the violent Storm of Lightning at East-Bourn, in Sussex, Sept. 17, 1780. Communicated by Owen Salusbury Brereton, Esq., F. R., and A. S. p. 42.*

I am desired by my friend and neighbour James Adair, Esq., of Soho-square, to communicate an account of the dreadful accident which happened to him and his family at East-Bourn, in the county of Sussex, at 9 o'clock in the morning, on Sunday the 17th of September last, (1780). He rented a house which stood by itself, built of various sorts of stone, 3 stories high, and facing the sea, which was nearly south-east of it. The morning was very stormy, with rain, thunder, and lightning; and just at 9 o'clock a horrid black cloud appeared, out of which Mr. Adair saw several balls of fire drop into the sea successively, as he was approaching the window in a one-pair of stairs room; and very soon after, as he was standing at it with his hands clasped, and extended open against the middle of the frame, a most violent flash of fire forced his hands asunder, and threw him several yards on the floor on his back, with both his legs upright in the air, which remained long so fixed. He was very sensible of his situation all the time, but could not open his eyes nor speak; nor had he

the least power of motion of any of his limbs for a long time. On help coming in, and examining his clothes, which were blue cloth, his right sleeve, both of coat and waistcoat, and also shirt, were all torn on the inside of the arm entirely open, as if by a dog, from the shoulder to the wrist; the right side of the breeches was torn in the same manner, and part of each of the brass buttons melted.

He had in his fob a gold watch with a steel chain; the button which opens it, and 3 other places of the case were melted. The pendant to which the chain is fixed was almost melted through, and much of the steel chain is incorporated with it, as is reciprocally some gold on that part of the steel which was within the fob. The going of the watch had stopped instantaneously, occasioned as at first appeared by the small pendulum spiral steel spring having been lengthened; not that it was absolutely so, but relatively, respecting the scapement of the watch, the several inner turns being brought closer together. His right arm, right side, and thigh, were miserably scorched, and the flesh torn: the foot of the stocking of his right leg and his shoe were torn in several places between the buckle and the toe-end of the shoe, and one of his toes split almost to the bone; but the buckle, which was a broad silver one, was not the least hurt nor even marked, and remained buckled as before. His sleeve-button of gold, in which was plaited hair covered with crystal, was broken from its link, and neither hair nor crystal have been found. A key and a penknife in his right-side breeches pocket have several marks of fusion on them.

The frame of the window, on which Mr. Adair was leaning, was little damaged; but every pane of glass so completely smashed, it could scarcely be perceived it had ever any glass in it. The room was stuccoed and papered, and between the windows hung a large pier glass, which, with much of the stucco, was shivered to pieces, and strewed over the floor. A door opposite the window was shattered to pieces, and the posts of a bed in a room behind, and all the bell-wires were destroyed. Under the dining-room Mr. Adair was in, on the parlour floor, were his coachman, butler, and footman. The coachman was going to open a glass-door to go towards the sea, and was struck dead. His body was totally black. His clothes, and the caul of his wig, and cravat, were much torn; but no particular flesh wound was found. The enamelled face of his silver watch was broken to pieces, and the links of its steel chain fastened together.

The footman was dressing his hair near a window, when he was thrown dead on the ground. He appeared much scorched, bruised, and black. He had a very large wound in his side, which penetrated near his heart; but very little, if any, blood came from it. His buck-skin breeches were much torn, and the steel of a metal knee-buckle driven through them. The window sash was driven



into the room, and a stone, about 8 inches square, forced out of the wall into the middle of the room, not far from the body. The butler was a yard or two behind the coachman, and going out with a telescope in his hand, which was forced in pieces from him, his hat and wig were thrown to some distance, and he perceived a violent pressure on his skull and on his back, but was no otherwise hurt. He had a silver watch with a silver chain, which received no damage. In the room over Mr. Adair's, a young lady was dressing, and her maid attending. They were both driven to a distant part of the room, and rendered insensible for some time, but not hurt. The posts of the bed she had just left were all shivered to pieces, and the bell wires destroyed, and the chimney thrown down on the roof.

Though the bodies of the two servants lay unburied from Sunday till Tuesday, all their limbs were as entirely flexible as those of a living person. Multitudes on the shore before the house saw the meteor dart in a right line over their heads, and break against the front of the house in different directions, and all agreed that the form and flame was exactly like that of an immense sky-rocket.

*IV. An Account of the Harmattan, a singular African Wind. By Matthew Dobson, M. D., F. R. S. p. 46.*

The harmattan is a periodical wind, which blows from the interior parts of Africa towards the Atlantic Ocean, and possesses such extraordinary properties, as to merit the attention of the naturalist, making a curious and important article in the history and theory of the winds. It is from the materials furnished by Mr. Norris, that the following account is drawn up.

On that part of the coast of Africa which lies between Cape Verd and Cape Lopez, an easterly wind prevails during December, January, and February, which by the Fantees, a nation on the Gold Coast, is called the harmattan. Cape Verd is in  $15^{\circ}$  N. latitude, and Cape Lopez in  $1^{\circ}$  S. latitude, and the coast between these two capes runs, in an oblique direction, nearly W. S. W. to E. S. E. forming a range of upwards of 2100 miles. At the Isles de Los, which are a little to the northward of Sierra Leone, and to the southward of Cape Verd, it blows from the E. S. E. on the Gold Coast from the N. E. and at Cape Lopez and the river Gabon, from the N. N. E. This wind is, by the French and Portugeze who frequent the Gold Coast, called the N. E. wind, the quarter from which it blows. The English, who sometimes borrow words and phrases from the Fantee language, which is less guttural and more harmonious than that of their neighbours, adopt the Fantee word harmattan. The harmattan comes on indiscriminately, at any hour of the day, at any time of the tide, or at any period of the moon, and continues sometimes only a day or two, sometimes 5 or 6 days, and it has been known to last 15 or 16 days. There are generally 3



or 4 returns of it every season. It blows with a moderate force, not quite so strong as the sea breeze, which every day sets in during the fair season from the w. w. s. w. and s. w.; but somewhat stronger than the land wind at night from the n. and n. n. w.

1. A fog or haze is one of the peculiarities which always accompanies the harmattan. The gloom occasioned by this fog is so great, as sometimes to make even near objects obscure. The English fort at Whydah stands about the midway between the French and Portuguese forts, and not quite a quarter of a mile from either, yet very often from it neither of the other forts can be discovered. The sun, concealed the greatest part of the day, appears only for a few hours about noon, and is then of a mild red, exciting no painful sensation on the eye. The particles which constitute the fog are deposited on the grass, the leaves of trees, and even on the skin of the negroes, so as to make them appear whitish. They do not flow far over the surface of the sea: at 2 or 3 miles distance from the shore the fog is not so thick as on the beach; and at 4 or 5 leagues distance it is entirely lost, though the harmattan itself is plainly felt for 10 or 12 leagues, and blows fresh enough to alter the course of the current.

2. Extreme dryness makes another extraordinary property of this wind. No dew falls during the continuance of the harmattan; nor is there the least appearance of moisture in the atmosphere. Vegetables of every kind are very much injured; all tender plants, and most of the productions of the garden, are destroyed; the grass withers, and becomes dry like hay; vigorous ever-greens likewise feel its pernicious influence; the branches of the lemon, orange, and lime trees droop, the leaves become flaccid, wither, and, if the harmattan continues to blow for 10 or 12 days, are so parched as to be easily rubbed to dust between the fingers: the fruit of these trees, deprived of its nourishment, and stunted in its growth, only appears to ripen, for it becomes yellow and dry, without acquiring half the usual size. The natives take this opportunity, of the extreme dryness of the grass and young trees, to set fire to them, especially near their roads, not only to keep the roads open to travellers, but to destroy the shelter which long grass, and thickets of young trees, would afford to skulking parties of their enemies. A fire thus lighted flies with such rapidity as to endanger those who travel: in that situation a common method of escape is, on discovering a fire to windward, to set the grass on fire to leeward, and then follow your own fire. There are other extraordinary effects produced by the extreme dryness of the harmattan. The covers of books, even closely shut up in a trunk, and lying among clothes, are bent as if they had been exposed to the fire. Household furniture is also much damaged: the pannels of doors and of wainscot split, and any veneered work flies to pieces. The joints of a well-



laid floor of seasoned wood opened sufficiently to lay one's finger in them; but become as close as before on the ceasing of the harmattan. The seams also in the sides and decks of ships are much injured, and the ships become very leaky, though the planks are 2 or 3 inches in thickness. Iron-bound casks require the hoops to be frequently driven tighter; and a cask of rum or brandy, with wooden hoops, can scarcely be preserved; for, unless a person attends to keep it moistened, the hoops fly off.

The parching effects of this wind are likewise evident on the external parts of the body. The eyes, nostrils, lips, and palate, are rendered dry and uneasy, and drink is often required, not so much to quench thirst, as to remove a painful aridity in the fauces. The lips and nose become sore, and even chapped; and though the air be cool, yet there is a troublesome sensation of prickling heat on the skin. If the harmattan continues 4 or 5 days, the scarf skin peels off, first from the hands and face, and afterwards from the other parts of the body, if it continues a day or two longer. Mr. Norris, who frequently visited the coast of Africa, observed, that when sweat was excited by exercise on those parts which were covered by his clothes from the weather, it was peculiarly acrid, and tasted, on applying his tongue to his arm, something like spirit of hart's-horn diluted with water.

As the state of salt of tartar placed in the open air, and the quantity evaporated from a given surface of water, are obvious proofs of the comparative moisture or dryness of the atmosphere, Mr. Norris put the harmattan to each of these tests; and particularly to moisten salt of tartar ad deliquium, and exposed it to the night air during the time that the harmattan was blowing. The following is the account of the result of these experiments. Salt of tartar will not only remain dry during the night as well as in the day; but, when liquefied so as to run on a tile, and exposed to the harmattan, becomes perfectly dry in 2 or 3 hours; and, exposed in like manner to the night air, will be dry before morning.

It appears, from experiments made by Mr. Norris, that if the evaporation of the whole year be supposed to go on in the same proportion with what occurred during a short and very moderate return of the harmattan, the annual harmattan evaporation would be 133 inches; and if the calculation was made in proportion to the evaporation which occurs during a longer visit from the harmattan, and a more forcible breeze, the annual harmattan evaporation would be much more considerable. If the annual evaporation be in like manner calculated, in proportion to the evaporation which took place subsequent to and preceding the harmattan, the annual evaporation at Whydah on the Gold Coast, would be 64 inches, and he had found the annual evaporation at Liverpool to be 36 inches.



These three therefore are in the following proportion; harmattan 133 inches, Whydah 64 inches, and Liverpool 36 inches.

3. Salubrity forms a third peculiarity of the harmattan. Though this wind is so very prejudicial to vegetable life, and occasions such disagreeable parching effects on the human species, yet it is highly conducive to health. Those labouring under fluxes and intermitting fevers generally recover in an harmattan. Those weakened by fevers, and sinking under evacuations for the cure of them, particularly bleeding, which is often injudiciously repeated, have their lives saved, and vigour restored, in spite of the doctor. It stops the progress of epidemics: the small-pox, remittent fevers, &c. not only disappear, but those labouring under these diseases, when an harmattan comes on, are almost certain of a speedy recovery. Infection appears not then to be easily communicated even by art. In the year 1770 there were on board the *Unity*, at Whydah, above 300 slaves; the small-pox broke out among them, and it was determined to inoculate; those who were inoculated before the harmattan came on, got very well through the disease. About 70 were inoculated a day or two after the harmattan set in; but not one of them had either sickness or eruption. It was imagined, that the infection was effectually dispersed, and the ship clear of the disorder; but in a very few weeks it began to appear among these 70. About 50 of them were inoculated the second time; the others had the disease in the natural way: an harmattan came on, and they all recovered, except one girl, who had an ugly ulcer on the inoculated part, and died some time afterwards of a locked jaw. Mr. Norris dissents from Dr. Lind, who speaks of the harmattan as "fatal and malignant; that its noxious vapours are destructive to blacks as well as whites; and that the mortality which it occasions is in proportion to the density and duration of the fog." The baneful effects here pointed out proceed from the periodical rains which fall in March, April, &c. and which are ushered in by the tornados, or strong gusts of wind from the N. E. and E. N. E. accompanied with violent thunder and lightning, and very heavy showers. The earth, drenched by these showers and acted on with an intense solar heat as soon as the storm is over, sends forth such noisome vapours as strike the nostrils with a most offensive stench, and occasion bilious vomitings, fluxes, and putrid fevers. Besides these vapours, which are annual, there appears to be a collection of still more pestiferous matter, confined for a longer time, and issuing from the earth after an interval of 5, 6, or 7 years. There may indeed be instances in which the harmattan comes loaded with the effluvia of a putrid marsh; and if there are any such situations, the nature of the wind may be so changed as to become even noxious.

It appears that, except a few rivers and some lakes, the country about and



beyond Whydah is covered for 400 miles back with verdure, open plains of grass, clumps of trees, and some woods of no considerable extent. The surface is sandy, and below that a rich reddish earth; it rises with a gentle ascent for 150 miles from the sea before there is the appearance of a hill, without affording a stone of the size of a walnut. Beyond these hills there is no account of any great ranges of mountains. With respect to the origin of this wind, Mr. Norris says, "the harmattan, according to Dr. Lind, arises from the conflux of several rivers about Benin; but when I was on a visit to the King of Dahomey, 120 miles north, or inland from the fort at Whydah, I there felt the harmattan blowing from the N. E. stronger than I have at any other time, though Benin then bore from me S. E." On this head Mr. Norris makes the following conjecture: "The intersection of 3 lines, viz. an east line drawn from Cape Verd, a north-east one from the centre of the Gold Coast, and a north line from Cape Lopez, would point out a probable source of this extraordinary wind." Three lines, drawn according to the direction of Mr. Norris, towards the points of the compass from which the harmattan blows on Cape Verd, the Gold Coast, and Cape Lopez, converge to a part of Africa about the 15th degree of N. latitude, and the 25th degree of E. longitude, which is that part of Africa where, according to Ptolemy, the mountains of Caphas are situated. From these mountains, according to the same authority, the river Daradus arose, supposed by some to be now the river Senegal. It may be conjectured, that the disagreeable Levant wind of the Mediterranean proceeds from the same part of the continent of Africa; for it prevails during the same season of the year, and may derive its qualities from the surface over which it passes.

The last article of information with which I have been favoured by Mr. Norris, is an account of the manner in which the Fantee nation divide their year. Aherramantah, or the harmattan, from the 1st of December to the middle of February, about 10 weeks. Quakorah, a wind up the coast, from S. S. W. to S. S. E. from the middle of February to the first week in March, about 3 weeks. Pempina, or tornado season, part of March, all April, and the greatest part of May, about 12 weeks. Abrenama, or the old man's and woman's children, that is, the Pleiades, the rainy season, the latter end of May, all June, and to about the 20th of July, 8 weeks. Atukogan, or 5 stars, that is, Orion, high wind and squally, the rains very heavy, to the middle of August, 3 weeks. Worrobakorou, or one star, the ceasing of the rains, about 3 weeks. Mawurrah, the name of a certain star; close, foggy weather and no breeze, the first 3 weeks in September. Boutch, no land breeze in this season, the wind blows fresh down the coast, about 6 weeks. Autiophi, or the croziers; tornados and southerly wind, with some rain, generally called the latter rains, about 4



weeks, to the beginning of December, when the Aherramantah season again commences.

*V. A New Method of applying the Screw. By Mr. Wm. Hunter, Surg. p. 58.*

The method is somewhat similar to Nonius's division of the circle. In certain cases it may be attended with some advantages to a greater degree than by those commonly practised. Let AB (fig. 2, pl. 1) be a plate of metal in which the screw CD plays, having a number of threads in an inch equal to  $a$ . Within the screw CD there is a female screw, by which is received the smaller screw DE of  $a + 1$  threads in an inch. This screw is retained from moving round along with the screw CD by means of the apparatus at AFG B. Now, if the handle CKL be turned  $a$  times round the screw, CD will advance upwards an inch, and if we suppose the screw DE to move round along with CD, the point E will also advance an inch. If we now turn the screw DE  $a$  times backward, the point E will move downwards  $\frac{a}{a+1}$  of an inch, and the result of both motions will be to lift the point E upward  $(1 - \frac{a}{a+1} =) \frac{1}{a+1}$  of an inch. But if, while the screw CD is turned  $a$  times round, DE be kept from moving, the effect will be the same as if it had moved  $a$  times round with CD and been  $a$  times turned back, that is, it will advance  $\frac{1}{a+1}$  of an inch. At one turn therefore of the handle CKL it will move upwards  $(\frac{1}{a+1} \times \frac{1}{a} =) \frac{1}{a^2+a}$  of an inch. If then we suppose the handle CKL to be  $b$  inches long, the power gained by the machine will be as  $(a^2 + a) \times 6.2832 b$  to unity.

To illustrate this by a particular example, let the screw CD have 10 threads in an inch, and DE 11; then, while the handle CKL is turned 10 times round, the point D will rise 1 inch above its former situation. But at 10 turns it can only pass over 10 threads of the screw DE, and consequently it will advance on that screw  $\frac{10}{11}$  of an inch. The point E therefore must rise  $\frac{1}{11}$  of an inch, that the point D may have room to rise a complete inch above its former place: therefore, at one turn of the handle, the point E will rise  $\frac{1}{110}$  of an inch; and if the handle be supposed half a foot long, the power, to produce an equilibrium, must be to the weight, as 1 to  $110 \times 6.2832 \times 6 = 4146.912$ , which is the very number expressed by the general theorem, viz.  $(a^2 + a) \times 6.2832 b$ , calling  $a = 10$  and  $b = 6$ .

Now let us compare, according to the rules before laid down, this method of using the screw with the common one. And first, in order to have the same power by means of the common screw that is exerted by this machine, it must have a number of threads in an inch equal to  $a^2 + a$ , which would render it too



weak to resist any considerable violence. For example, if  $dc$  have 5 threads in an inch, and  $de$  6, and if the handle  $ckl$  be a foot in length, the power gained by the engine will be nearly as  $(a^2 + a) \times 6b = 2160$  to 1; whereas, to have the same force by means of the common screw, it must have 30 threads in an inch, and so must yield under a resistance which the other screw would overcome without any difficulty. On this principle, the screw may be applied with advantage in presses of different kinds, by fixing one of the plates of the press to the end of the screw at  $E$ .

If the screw  $de$  be intended to carry an index which must turn round at the same time that it rises upwards, the common screw is preferable; for though I can see a method by which the machine before described may be made to answer this purpose, I am almost afraid to propose it. I mean, that within the screw  $de$  another still smaller should be made to play, and be connected with the screw  $cd$ , so as to move round along with it. It must have  $a^2 + a + 1$  threads in an inch, and they must be in the contrary direction to those of  $cd$ , so that when they are both turned together, and  $cd$  moves upward, this other one may move downward. At one turn of the handle this will move upward  $\frac{1}{a^2 + a} \times \frac{1}{a^2 + a + 1} = \frac{1}{a^4 + 2a^3 + 2a^2 + a}$  of an inch, and at the same time will move round in a circular direction. For example, let  $cd$  have 5 threads ( $= a$ ) in an inch,  $de$  6 ( $= a + 1$ ), and a third screw within  $de$ , but connected with  $cd$  so as to partake of its motion, 31 ( $= a^2 + a + 1$ ). At one turn of the handle, this screw will rise upwards  $\frac{1}{5} \times \frac{1}{6} \times \frac{1}{31} = \frac{1}{930}$  of an inch; but this appears too complicated for use, and the least inaccuracy in the construction would hinder it from moving.

But, on the other hand, if while the point  $E$  rises it is of consequence that it be kept from going round, the machine under consideration will best answer this purpose. On this principle it may be useful in several respects: for instance, let  $A$  (fig. 3) represent a magnifying lens, and let it be moveable on the screw  $bc$  of 16 threads in an inch, which turns within the larger screw  $cd$  of 15 threads in an inch, and that again moves within the plate  $EF$  in the end of the cylinder  $GF$ .\* To use the instrument, fix the object to be magnified on the pin  $GL$ , and then turn the lens  $A$  on the screw  $bc$ , till it be nearly at the proper distance from the pin, and opposite to it. You may then adjust the distance more accurately by turning the screw  $dc$ , at each turn of which the lens will recede from, or approach to, the pin  $\frac{1}{240}$  of an inch. This it will do and not turn aside, but still remain opposite to the pin  $LG$ . A double microscope might be fitted on in

\* The screw  $bc$  is restrained from moving along with  $cd$  by the small pillar  $HK$ , which slides backwards and forwards in a groove in the cylinder  $GF$ .—Orig.



the place of the lens A. The whole instrument may be furnished with a handle, as at M; or, if larger, it may have 3 feet to stand on a table.

On the last principle it must be owned, the common screw has the advantage, as 2 screws will produce more friction than one; and besides, in the compound engine there is an additional friction from the piece FG (fig. 2) on the pillars between which it moves.

Another case in which this machine may be employed, is in the micrometer. Thus, let the screw AB (fig. 4) of 50 threads in an inch be turned round by the index c, which moves on the graduated circle ECD in the direction CD. Within the screw AB is the smaller one AF of 51 threads in an inch, retained from moving round by the bar GFH. The piece AF is continued to K, where it forms a fine point. To use the instrument, let it be adjusted to the telescope or microscope by which you are to view a star, or some small object, and let the point K appear just to touch one edge of the object. Then turn the index c, and the point K will advance upwards till it appears to cover the other edge of the object, and thus you can determine its size. The point K will advance at each complete turn of the index  $\frac{1}{2550}$  of an inch; and if the circle be divided into 80 equal parts, 1 of which, if it is an inch in diameter, will be very observable, while the index moves over one of these, the point K will advance  $\frac{1}{204000}$  of an inch.

Thus, for example, suppose I am to measure the diameter of a nervous fibre in the medullary substance of the brain, I make the point K appear close to one edge, and turn the index till the same point pass over the fibre, and appear to touch the other edge: I then look on the graduated circle ECD, and perceive that the index c has passed over, suppose, 23.2 divisions. Hence I conclude the diameter of the fibre to be  $23.2 \times \frac{1}{204000} = \frac{1}{8790}$  of an inch, which is nearly the size as found by the accurate observations of Dr. Monro. There should be a Nonius's scale on the index which will measure to  $\frac{1}{10}$  of a division.

As the index c must continue close to the plate ECD, while at the same time it turns round the screw AB, which is continually rising, it must be made as in fig. 5, where a, b, are 2 small pieces which play in a groove in the screw AB (fig. 4). while the groove CD (fig. 5) in the index is filled up by a protuberance of the plate ECD (fig. 4); the piece below the groove cd (fig. 5) being sunk into that plate. The whole machinery may be inclosed in a cylinder of brass reaching from B to L (fig. 4), so that the point of the screw KL may be without it, and the sides of the cylinder may be open at ECD.

It is further to be observed, that what has been said goes on the supposition that the point K, in the micrometer, is equally magnified with the object we are to measure. But, if this point be placed in the focus of the eye-glass of a double microscope; when it moves it will pass over, not the object itself, but its image,



magnified by the object-glass. In this case, if the object-glass magnify the diameter 10 times, while the index passes over 1 division, the point  $k$  will pass over the image of an object, the diameter of which is  $\frac{1}{2040000}$  of an inch. As in this mode of application the point  $k$  must fall between the object and eye-glass, the screws may be contained within the fulcrum by which the microscope is supported.

The machine (fig. 2) may be applied as a jack to raise great weights a little way from the ground, by substituting 2 cross hand-spikes for the handle  $ckl$ ; or a vertical handle may be employed in the following manner. Let  $A$  (fig. 6) be a pinion turned by the handle  $AB$ , which suppose a foot in length. Let the pinion  $A$  have 4 teeth; and move the wheel  $cd$  of 16 teeth. The screw  $EF$  of 4 threads in an inch is fixed in this wheel, and turns round along with it. Within it plays the screw  $FG$  of 5 threads in an inch, and which we suppose prevented from following the motion of  $EF$ : it terminates in such a shoulder as that represented at  $G$ , and being continued to  $H$  ends in a foot as in the figure. The whole is inclosed in a strong frame. The pinion  $A$  must be connected in such a manner with the wheel  $cd$ , as to rise within the frame along with it, which may easily be done by making its axis play in a piece of wood or metal, which is connected by the end to the screw  $EF$ . Or, if this should be deemed inconvenient, as the rising of the pinion must raise the handle  $AB$ , the wheel  $cd$  may be hindered from rising, and at the same time turn the screw  $EF$ , by a contrivance similar to that used with the index  $c$  (fig. 4) in the micrometer. In either case, the axis of the pinion should be continued through the opposite side of the frame, and armed with a heavy fly to regulate the motion. When the machine is to be applied to use, the bottom of the frame resting on the ground, if the body to be lifted be already as high as the top  $G$ , that top is applied below it; but if it be close to the ground, we put below it the foot  $H$ ; then, if the handle  $AB$  be turned once round, the wheel  $cd$  and screw  $EF$  will turn  $\frac{1}{4}$  part round, and the point  $F$  will rise ( $\frac{1}{4} \times \frac{1}{4} =$ )  $\frac{1}{16}$  of an inch. The point  $G$  or  $H$  will therefore be lifted upwards ( $\frac{1}{16} \times \frac{1}{5} =$ )  $\frac{1}{80}$  of an inch. But the end  $B$  of the handle  $AB$  has described above 6 feet; therefore the velocity of the point  $G$  is to that of the point  $B$ , as 1 to ( $72 \times 80 =$ ) 5760. Therefore, if we suppose a man to act at the handle with a force equal to 30 lbs. he may keep in equilibrio a weight of 172800 lbs. But a subduction of perhaps more than  $\frac{1}{2}$  of this must be made, that he may raise the weight, as the friction of the engine will be considerable. Suppose it to be  $\frac{1}{3}$ , the effect still remains equal to 57600 lbs. or 25 tons, 14 cwts. and 32 lbs.

It will easily appear, that this method of applying the screw may take place in many other engines, particularly where great accuracy is required; or where we want a motion to be performed with great power, while at the same time it need



not have any large compass. The few examples given above may serve as a specimen.

*VI. An Account of the Turkey. By Thomas Pennant, Esq., F. R. S. p. 67.*

**TURKEY.**—Bill convex, short and strong. Head and neck covered with a naked tuberose flesh, with a long fleshy appendage hanging from the base of the upper mandible. On the breast a long tuft of coarse black hairs.

**WILD TURKEY.**—Josselyn's Voy. 99. Rarities 8. Clayton's Virgin. Lawson, 149. Catesby Topp. 44.—Le coque d'Inde, Belon 248.—Gallo-pavo, Gesner. Av. 481. Icon. 56.—Gallo-pavo, Aldrov. Av. 11. 18.—Gallo-pavo, the Turkey, A. 3.—Gallo-pavo sylvestris Novæ Angliæ, a New England wild Turkey, Raii Synopsis Avium 51.—Meleagris Gallo-pavo. M. capite caruncula frontali gula-rique, maris pectore barbato, Lin. Syst. 268.—Le Dindon de Buffon III. Brisson. 1, 158, tab. 16. Pl. Enl. 97.

*Description.*—T. with the characters described in the definition of the genus. The plumage, dark glossed with variable copper colour, and green. Coverts of the wings and the quill feathers barred with black and white. Tail consists of 2 orders. The upper or shorter very elegant, the ground colour a bright bay; the middle feather marked with numerous bars of shining black and green. The greatest part of the exterior feathers of the same ground with the others marked with 3 broad bands of mallard green, placed remote from each other. The two next are coloured like those of the middle; but the end is plain and crossed with a single bar, like the exterior. The longer or lower order are of a rusty white colour, mottled with black; and crossed with numerous narrow-waved lines of the same colour, and near the end with a broad band.

Wild turkeys preserve a sameness of colouring; the tame, as usual with domestic animals, vary. It is needless to point out the differences in so well known a bird: the black approaches nearest to the original stock. This variety I have seen nearly in a state of nature in Richmond and other parks. A most beautiful kind has of late been introduced into England, of a snowy whiteness, finely contrasting with its red head. These, I think, came out of Holland, probably bred from an accidental white pair; and from them preserved pure from any dark or variegated birds.

The sizes of the wild turkeys have been differently represented. Some writers assert that there have been instances of their weighing 60 pounds; but I find none who, speaking from their own knowledge, can prove their weight to be above 40. Josselyn says, that he has eaten part of a cock, which after it was plucked, and the entrails taken out, weighed 30. Lawson, whose authority is unquestionable, saw half a turkey serve 8 hungry men for 2 meals; and says, that he had seen others which he believed weighed 40 pounds. Catesby tells us,



that out of the many hundreds which he had handled, very few exceeded 30 pounds; each of these speak of their being double that size merely from the reports of others.

The manners of these birds are as singular as their figure. Their attitudes in the season of courtship are very striking. The males fling their heads and neck backwards, bristle up their feathers, drop their wings to the ground, strut and pace most ridiculously; wheel round the females with their wings rustling along the earth, at the same time emitting a strange sound through their nostrils not unlike the grurr of a great spinning wheel. On being interrupted they fly into great rages, and change their notes into a loud and guttural gobble, and then return to dalliance. The sound of the female is plaintive and melancholy. The passions of the males are very strongly expressed by the change of colours in the fleshy substance of the head and neck, which alters to red, white, blue, and yellowish, as they happen to be affected. The sight of any thing red excites their choler greatly. They are polygamous, one cock serving many hens. They lay in the spring, and produce a great number of eggs. They will persist in laying for a great while. They retire to some obscure place to sit, the cock through rage at the loss of his mate being very apt to break the eggs. The females are very affectionate to their young, and make great moan on the loss of them. They sit on their eggs with such perseverance, that if they are not taken away when addle, the hens will almost perish with hunger before they will quit the nest. Turkeys greatly delight in the seeds of nettles; but those of the purple-fox glove prove fatal to them. Turkeys are very stupid birds, quarrelsome, and cowardly. It is diverting to see a whole flock attack the common cock, who will, for a long time, keep a great number at bay. They are very swift runners in the tame as well as the wild state: they are but indifferent flyers. They love to perch on trees, and gain the height they wish by rising from bough to bough. In a wild state they get to the very summit of the loftiest trees, even so high as to be beyond the reach of the musquet.

In the state of nature they go in flocks even of 500, feed much on the small red acorns, and grow so fat in March that they cannot fly more than 3 or 4 hundred yards, and are soon run down by a horseman. In the unfrequented parts bordering on the Mississippi, they are so tame as to be shot with even a pistol. They frequent the great swamps of their native country, and leave them at sun-rising to repair to the dry woods in search of acorns and berries; and before sun-set retire to the swamps to roost.

The flesh of the wild turkey is said to be superior in goodness to the tame, but redder. Eggs of the former have been taken from the nest, and hatched under tame turkeys. The young will still prove wild, perch separate, yet mix and breed together in the season. The Indians sometimes use the breed pro-



duced from the wild as decoy birds to seduce those in a state of nature within their reach. When disturbed, they do not take wing, but run out of sight. It is usual to chace them with dogs, when they will fly and perch on the next tree. They are so stupid or so insensible of danger, as not to fly on being shot at; but the survivors remain unmoved at the death of their companions. Wild turkies are now become most excessively rare in the inhabited parts of America, and are only found in numbers in the distant and most unfrequented spots. The Indians make a most elegant clothing of the feathers. They twist the inner webs into a strong double thread of hemp, or inner bark of the mulberry tree, and work it like matting; it appears very rich and glossy, and as fine as a silk shag. They also make fans of the tail; and the French of Louisiana were wont to make umbrellas by the junction of 4 of the tails.

Turkies are natives only of America, or the New World, and of course unknown to the ancients. Since both these positions have been denied by some of the most eminent naturalists of the 16th century, I beg leave to lay open, in as few words as possible, the cause of their error. Belon, the earliest of those writers who are of opinion that these birds were natives of the old world, founds his notion on the description of the Guinea fowl, the *Meleagrides* of Strabo, Athenæus, Pliny, and others of the ancients. I rest the refutation on the excellent account given by Athenæus, taken from Clytus Milesius, a disciple of Aristotle, which can suit no other than that fowl. "They want," says he, "natural affection towards their young; their head is naked, and on the top is a hard round body like a peg or nail: from their cheeks hangs a red piece of flesh like a beard. It has no wattles like the common poultry. The feathers are black, spotted with white. They have no spurs; and both sexes are so like as not to be distinguished by the sight." Varro and Pliny take notice of the spotted plumage and the gibbous substance on the head. Athenæus is more minute, and contradicts every character of the turkey, whose females are remarkable for their natural affection, and differ materially in form from the males, whose heads are destitute of the callous substance and whose heels, in the males, are armed with spurs. Aldrovandus, who died in 1605, draws his arguments from the same source as Belon; I therefore pass him by, and take notice of the greatest of our naturalists Gesner, who falls into a mistake of another kind, and wishes the turkey to be thought a native of India. He quotes *Ælian* for that purpose, who tells us, "That in India are very large poultry not with combs, but with various coloured crests interwoven like flowers, with broad tails neither bending nor displayed in a circular form, which they draw along the ground as peacocks do when they do not erect them; and that the feathers are partly of a gold colour, partly blue, and of an emerald colour." This in all probability was the same bird with the peacock pheasant of



Mr. Edwards, *Le Paon de Tibet* of M. Brisson, and the *Pavo bicalcaratus* of Linneus. I have seen this bird living. It has a crest, but not so conspicuous as that described by Ælian; but it has those striking colours in form of eyes, neither does it erect its tail like the peacock, but trails it like the pheasant. The *catreus* of Strabo seems to be the same bird. He describes it as uncommonly beautiful and spotted, and very like a peacock. The former author gives a more minute account of this species, and under the same name. He borrows it from Clitarchus, an attendant of Alexander the Great in all his conquests. It is evident from his description, that it was of this kind; and it is likewise probable, that it was the same with his large Indian poultry before cited. He celebrates it also for its fine note; but allowance must be made for the credulity of Ælian. The *catreus*, or peacock pheasant, is a native of Tibet, and in all probability of the north of India, where Clitarchus might have observed it; for the march of Alexander was through that part which borders on Tibet, and is now known by the name of *Penj-ab*, or five rivers.

I shall now collect from authors the several parts of the world where turkies are unknown in the state of nature. Europe has no share in the question; it being generally agreed that they are exotic in respect to that continent. Neither are they found in any part of Asia Minor, or the Asiatic Turkey, notwithstanding ignorance of their true origin first caused them to be named from that empire. About Aleppo, capital of Syria, they are only met with, domesticated like other poultry. In Armenia they are unknown, as well as in Persia; having been brought from Venice by some Armenian merchants into that empire, where they are still so scarce as to be preserved among other rare fowl in the royal menagery. Du Halde acquaints us, that they are not natives of China; but were introduced there from other countries. He errs from misinformation in saying that they are common in India. I will not quote Gemelli Careri, to prove that they are not found in the Philippine Islands, because that gentleman, with his pen travelled round the world in his easy chair, during a very long indisposition and confinement in his native country. But Dampier bears witness that none are found in Mindanao.

The hot climate of Africa barely suffers these birds to exist in that vast continent, except under the care of mankind. Very few are found in Guinea, except in the hands of the Europeans, the negroes declining to breed any on account of the great heats. Prosper Alpinus satisfies us, that they are not found either in Nubia or in Egypt. He describes the *Meleagrides* of the ancients, and only proves that the Guinea hens were brought out of Nubia, and sold at a great price at Cairo; but is totally silent about the turkey of the moderns.

Let me here observe, that the Guinea hens have long been imported into Britain. They were cultivated in our farm-yards; for I discover in 1277,



in the Grainge of Clifton, in the parish of Ambrosden, in Buckinghamshire, among other articles, 6 Mutilones and 6 Africanæ fœminæ, for this fowl was familiarly known by the names of Afra Avis and Gallina Africana and Numida. It was introduced into Italy from Africa, and from Rome into our country. They were neglected here by reason of their tenderness and difficulty of rearing. We do not find them in the bills of fare of our ancient feasts; neither do we find the turkey: which last argument amounts to almost a certainty, that such a hardy and princely bird had not found its way to us. The other likewise was then known by its classical name; for that judicious writer Doctor Caius describes, in the beginning of the reign of Elizabeth, the Guinea fowl, for the benefit of his friend Gesner, under the name of Meleagris, bestowed on it by Aristotle.

Having denied, on the very best authorities, that the turkey ever existed as a native of the old world, I must now bring my proofs of its being only a native of the new, and of the period in which it first made its appearance in Europe. The first precise description of these birds is given by Oviedo, who in 1525 drew up a summary of his greater work, the History of the Indies, for the use of his monarch Charles v. This learned man had visited the West Indies and its islands in person, and payed particular regard to the natural history. It appears from him, that the turkey was in his days an inhabitant of the greater islands, and of the main-land. He speaks of them as peacocks; for being a new bird to him, he adopts that name from the resemblance he thought they bore to the former. "But," says he, "the neck is bare of feathers, but covered with a skin which they change after their phantasie into diverse colours. They have a horn as it were on their front, and hairs on the breast." He describes other birds which he also calls peacocks. They are of the gallinaceous genus, and known by the name of Curassao birds, the male of which is black, the female ferruginous. The next who speaks of them as natives of the main-land of the warmer parts of America, is Francisco Fernandez, sent there by Philip II., to whom he was physician. This naturalist observed them in Mexico. We find by him, that the Indian name of the male was huexolotl, of the female cihuatotolin. He gives them the title of Gallus Indicus and Gallo-pavo. The Indians as well as Spaniards, domesticated these useful birds. He speaks of the size by comparison, saying, that the wild were twice the magnitude of the tame; and that they were shot with arrows or guns. I cannot learn the time when Fernandez wrote. It must be between the years 1555 and 1598, the period of Philip's reign. Pedro de Ciesa mentions turkies on the Isthmus of Darien. Lery, a Portugeze author, asserts, that they are found in Brazil, and gives them an Indian name; but since I can discover no traces of them in that diligent and excellent naturalist Marcgrave, who resided long in that country, I must deny my assent.



But the former is confirmed by that able and honest navigator Dampier, who saw them frequently, as well wild as tame, in the province of Yucatan, now reckoned part of the kingdom of Mexico.

In North America they were observed by the very first discoverers. When René de Laudonniere, patronized by Admiral Coligni, attempted to form a settlement near the place where Charlestown now stands, he met with them on his first landing in 1564, and by his historian has represented them with great fidelity in the 5th plate of the recital of his voyage: from his time the witnesses to their being natives of the continent are innumerable. They have been seen in flocks of hundreds in all parts from Louisiana even to Canada; but at this time are extremely rare in a wild state, except in the more distant parts, where they are still found in vast abundance.

It was from Mexico or Yucatan that they were first introduced into Europe; for it is certain, that they were imported into England as early as the year 1524, the 15th of Henry VIII. We probably received them from Spain, with which we had great intercourse till about that time. They were most successfully cultivated in our kingdom from that period; insomuch, that they became common in every farm-yard, and became even a dish in our rural feasts by the year 1585; for we may certainly depend on the word of old Tusser, in his Account of the Christmas Husbandlie Fare, in the Five Hundred Points of good Husbandrie, p. 57.

Beefe, mutton, and porke, shred pies of the best,  
Fig, veale, goose, and capon, and turkie well drest,  
Cheese, apples, and nuts, jolie carols to heare,  
As then in the countrie, is counted good cheare.

But at this very time they were so rare in France, that we are told, that the very first which was eaten in that kingdom appeared at the nuptial feast of Charles IX., in 1750.

To this account I beg leave to mention the very extraordinary appearance on the thigh of a turkey, bred in my poultry-yard, and which was killed a few years ago for the table. The servant in plucking it was very unexpectedly wounded in the hand. On examination, the cause appeared so singular, that the bird was brought to me. I discovered, that from the thigh-bone issued a short upright process, and to that grew a large and strong toe, with a sharp and crooked claw, exactly resembling that of a rapacious bird.

*VII. Of a Nebula in Coma Berenices. By Edward Pigot, Esq. p. 82.*

On the 23d of March, 1779, Mr. P. discovered a nebula in the constellation of Coma Berenices, previously he presumes unnoticed; at least not mentioned in M. de Lalande's Astronomy, nor in M. Messier's ample Catalogue of nebulous

Stars. He observed it in an achromatic transit instrument, 3 feet long, and deduced its mean R. A., by comparing it to several stars, having made the necessary corrections for aberration and nutation, the result of all being  $191^{\circ} 28' 38''$ . Its declination north was  $22^{\circ} 53' \frac{1}{4}$ . The diameter of this nebula about  $2'$  of a degree.

*VIII. Double Stars discovered in 1779, at Frampton-house, Glamorganshire.*

*By Nathan. Pigott, Esq., F.R.S., Foreign Member of the Academies of Brussels and Caen, and Correspondent of the Royal Academy of Sciences at Paris. p. 84.*

Inclosed are the determinations of the places of 3 double stars, which I discovered this summer (1779): at least I presume they have not been observed before, because I do not find them inserted in Dr. Bradley's catalogue, published in the Nautical Almanac 1773, or in the Connoissance des Temps, or in other catalogues in my possession.  $\gamma$  Delphini indeed, is in M. de la Caille's catalogue; but not as a double star. The instrument he used was not probably powerful enough for that purpose. In the two-feet telescope of my quadrant it appears only as a single star. These stars were observed by me in a three-feet achromatic telescope of a transit instrument, with an object-glass near 2 inches diameter. The R. A. are nicely determined by several observations, which always agree with each to a fraction of a second in time. The declinations were deduced from the difference of altitudes between the double stars and the known stars to which they were compared.

*September 5, 1779.*

App. R. A.	App. declination.
307 $^{\circ}$ 21' 5" ... $\alpha$ Delphini, 3d mag. ....	15 $^{\circ}$ 8' 53"N.
309    6 30 ... 2d or brightest of $\gamma$ Delphini 4. ....	15 20 40 N.
0    9 $\frac{1}{2}$ ... diff. R. A. of the 2 stars in $\gamma$ Delphini.	

Note, both the stars in  $\gamma$  Delphini have the same, or nearly the same, declination. The 1st is of the 6th, the 2d of the 4th mag.

*September 19.*

319   59 27 + $\beta$ Aquarii, 3d mag. ....	6 31 34 s.
318    3 21 ... preceding double star, 5th mag. ....	7 40 34 s.
0 11 — diff. R. A. between 1st and 2d of the double stars.	

Note, the 1st seemed of the 5th, the 2d of the 7th mag. The 1st is perhaps 6" or 8" s. of the following one.

337   36 55 + $\zeta$ Pegasi, 3d mag. ....	9 41 24 N.
346   53 36 $\frac{1}{2}$ ... double star, 8th, 9th mag. ....	3 59 17 N.

Note, both the stars of this double star have the same, or nearly the same R. A.; their difference in declination is 15" or perhaps 20".



*IX. An Account of the Ganges and Burrampooter Rivers. By James Rennell, Esq., F. R. S. p. 87.*

The \* Ganges and † Burrampooter rivers, with their numerous branches and adjuncts, intersect the country of Bengal in such a variety of directions, as to form the most complete and easy inland navigation that can be conceived. So equally and admirably diffused are those natural canals, over a country that approaches nearly to a perfect plane, that, after excepting the lands contiguous to Burdwan, Birboom, &c. which altogether do not constitute a 6th part of Bengal, we may fairly pronounce, that every other part of the country has, even in the dry season, some navigable stream within 25 miles at farthest, and more commonly within a 3d part of that distance. It is supposed, that this inland navigation gives constant employment to 30,000 boatmen; for all the salt, and a large proportion of the food consumed by 10 millions of people, are conveyed by water within the kingdom of Bengal and its dependencies. To these must be added, the transport of the commercial exports and imports, probably to the amount of 2 millions sterling per ann.; the interchange of manufactures and products throughout the whole country; the fisheries; and the article of travelling.

These rivers exactly resemble each other in length of course; in bulk, till they approach the sea; in the smoothness and colour of their waters; in the appearance of their borders and islands; and finally, in the height to which their floods rise with the periodical rains. Of the two, the Burrampooter is the larger; but the difference is not obvious to the eye. It is now well known that they derive their sources from the vast mountains of Thibet‡; whence they proceed in opposite directions; the Ganges seeking the plains of Hindostan, by the west; and the Burrampooter by the east; both pursuing the early part of their course through rugged vallies and defiles. The Ganges, after wandering about 750 miles through these mountainous regions, issues forth a deity to the superstitious, yet gladdened, inhabitant of Hindostan. From Hurdwar, or Hurdoar, in latitude 30°, where it gushes through an opening in the mountains, it flows with a smooth navigable stream through delightful plains during the remainder of its course to the sea, which is about 1350 miles, diffusing plenty immediately by means of its living

\* The proper name of this river in the language of Hindostan, is Pudda or Padda. It is also named Burra-Gonga, or the Great River; and Gonga, the river, by way of eminence; and from this doubtless the European names of the river are derived.—Orig.

† The orthography of this word, as given here, is according to the common pronunciation in Bengal; but it is said to be written in the Sancrit language, Brahma-pootar, which signifies the son of Brahma.—Orig.

‡ These are among the highest of the mountains of the old hemisphere. Their height may in some measure be guessed, by the circumstance of their rising considerably above the horizon, when viewed from the plains of Bengal, at the distance of 150 miles.—Orig.



productions; and secondarily by enriching the adjacent lands, and affording an easy means of transport for the productions of its borders.

In its course through the plains, it receives eleven rivers, some of which are equal to the Rhine, and none smaller than the Thames, besides as many others of less note. It is owing to this vast influx of streams, that the Ganges exceeds the Nile so greatly in point of magnitude, while the latter exceeds it in length of course by one-third. Indeed the Ganges is inferior, in this last respect, to many of the northern rivers of Asia; though probably it discharges as much or more water than any of them, because those rivers do not lie within the limits of the periodical rains.\*

The bed of the Ganges is very unequal in point of width. From its first arrival in the plains at Hurdwar, to the conflux of the Jumnah, the first river of note that joins it, its bed is generally from a mile to a mile and a quarter wide; and, compared with the latter part of its course, tolerably straight. Hence, downward, its course becomes more winding, and its bed consequently wider, till, having alternately received the waters of the Gogra, Soane, and Gunduck, besides many smaller streams, its bed has attained its full width; though, during the remaining 600 miles of its course, it receives many other principal streams. Within this space it is, in the narrowest parts of its bed, half a mile wide, and in the widest, 3 miles; and that, in places where no islands intervene. The stream within this bed is always either increasing or decreasing, according to the season. When at its lowest, which happens in April, the principal channel varies from 400 yards to a mile and a quarter; but is commonly about 3 quarters of a mile. It is fordable in some places above the conflux of the Jumnah, but the navigation is never interrupted. Below that, the channel is of considerable depth, for the additional streams bring a greater accession of depth than width. At 500 miles from the sea, the channel is 30 feet deep when the river is at its lowest; and it continues at least this depth to the sea, where the sudden expansion of the stream deprives it of the force necessary to sweep away the bars of sand and mud thrown across it by the strong southerly winds; so that the principal branch of the Ganges cannot be entered by large vessels. About 220 miles from the sea, but 300 reckoning the windings of the river, commences the head of the Delta of the Ganges, which is considerably more than twice the area of that of the Nile. The two westernmost branches, named the Cossimbuzar and

\* The proportional lengths of course of some of the most noted rivers in the world are shown nearly in the following numbers:

*European rivers:* Thames 1; Rhine  $5\frac{1}{4}$ ; Danube 7; Wolga  $9\frac{1}{2}$ .—*Asiatic rivers:* Indus  $5\frac{1}{2}$ ; Euphrates  $8\frac{1}{2}$ ; Ganges  $9\frac{1}{2}$ ; Burrampooter  $9\frac{1}{2}$ ; Nou Kian, or Ava river  $9\frac{1}{2}$ ; Jennisea 10; Oby  $10\frac{1}{2}$ ; Amoor 11; Lena  $11\frac{1}{2}$ ; Hoanho (of China)  $13\frac{1}{2}$ ; Kian Keu (of ditto)  $15\frac{1}{2}$ .—*African river:* Nile  $12\frac{1}{2}$ .—*American rivers:* Mississippi 8; Amazons  $15\frac{3}{4}$ .—Orig.



Jellinghy rivers, unite and form what is afterwards named the Hoogly river, which is the port of Calcutta, and the only branch of the Ganges that is commonly navigated by ships. The Cossimbuzar river is almost dry from October to May; and the Jellinghy river is in some years unnavigable during 2 or 3 of the driest months; so that the only subordinate branch of the Ganges, that is at all times navigable, is the Chundnah river, which separates at Moddapour, and terminates in the Hooringotta.

That part of the Delta bordering on the sea, is composed of a labyrinth of rivers and creeks, all of which are salt, except those that immediately communicate with the principal arm of the Ganges. This tract, known by the name of the Woods, or Sunderbunds, is in extent equal to the principality of Wales; and is so completely enveloped in woods, and infested with tigers, that if any attempts have ever been made to clear it, they have hitherto miscarried. Its numerous canals are so disposed as to form a complete inland navigation throughout and across the lower part of the Delta, without either the delay of going round the head of it, or the hazard of putting to sea. Here salt, in quantities equal to the whole consumption of Bengal, and its dependencies, is made and transported with equal facility: and here also is found an inexhaustible store of timber for boat-building. The breadth of the lower part of this Delta is upwards of 180 miles; to which, if we add that of the two branches of the river that bound it, we shall have about 200 miles for the distance to which the Ganges expands its branches at its junction with the sea.

It has been observed before, that the course of this river, from Hurdwar to the sea, is through a uniform plain, or at least what appears such to the eye: for, the declivity is much too small to be perceptible. A section of the ground, parallel to one of its branches, 60 miles in length, was taken by order of Mr. Hastings; and it was found to have about 9 inches descent in each mile, reckoning in a straight line, and making allowance for the curvature of the earth. But the windings of the river were so great, as to reduce the declivity on which the water ran, to less than 4 inches per mile.\* The medium rate of motion of the Ganges is less than 3 miles an hour in the dry months. In the wet season, and during the draining off of the waters from the inundated lands, the current runs from 5 to 6 miles an hour; but there are instances of its running 7, and even 8 miles, in particular situations, and under certain circumstances.

Commonly there is found on one side of the river an almost perpendicular bank, more or less elevated above the stream, according to the season, and with

\* M. de Condamine found the descent of the river Amazons, in a straight course of about 1860 miles, to be 1020 English feet, or  $6\frac{2}{3}$  inches in a mile. If we allow for the windings, it comes out nearly the same as the Ganges (which winds about  $1\frac{1}{2}$  mile in 3, taking its whole course through the plains), namely, about 4 inches in a mile.—Orig.



deep water near it: and on the opposite side a bank, shelving away so gradually as to occasion shallow water at some distance from the margin. This is more particularly the case in the most winding parts of the river, because the very operation of winding produces the steep and shelving banks: for the current is always strongest on the external side of the curve formed by the serpentine course of the river; and its continual action on the banks either undermines them, or washes them down. In places where the current is remarkably rapid, or the soil uncommonly loose, such tracts of land are swept away in the course of one season, as would astonish those who have not been eye-witnesses to the magnitude and force of the mighty streams occasioned by the periodical rains of the tropical regions. This necessarily produces a gradual change in the course of the river; what is lost on one side being gained on the other, by the mere operation of the stream: for the fallen pieces of the bank dissolve quickly into muddy sand, which is hurried away by the current along the border of the channel to the point whence the river turns off to form the next reach, where the stream becoming weak, it finds a resting place, and helps to form a shelving bank, which commences at the point, and extends downwards, along the side of the succeeding reach.

It is evident that the repeated additions made to the shelving bank above-mentioned, become in time an encroachment on the channel of the river; and this is again counterbalanced by the depredations made on the opposite steep bank, the fragments of which, either bring about a repetition of the circumstances above recited, or form a bank or shallow in the midst of the channel. Thus a steep and a shelving bank are alternately formed in the crooked parts of the river (the steep one being the indented side, and the shelving one the projecting); and thus a continual fluctuation of course is induced in all the winding parts of the river; each meander having a perpetual tendency to deviate more and more from the line of the general course of the river, by eating deeper in the bays, and at the same time adding to the points, till either the opposite bays meet, or the stream breaks through the narrow isthmus, and restores a temporary straightness to the channel.

Several of the windings of the Ganges and its branches are fast approaching to this state; and in others, it actually exists at present. The experience of these changes should operate against attempting canals of any length, in the higher parts of the country; and I much doubt, if any in the lower parts would long continue navigable. During 11 years of my residence in Bengal, the outlet or head of the Jellinghy river was gradually removed  $\frac{3}{4}$  of a mile farther down: and by 2 surveys of a part of the adjacent bank of the Ganges, taken about the distance of 9 years from each other, it appeared that the breadth of an English mile and a half had been taken away. This is however the most rapid



change that I have noticed; a mile in 10 or 12 years being the usual rate of encroachment, in places where the current strikes with the greatest force, namely, where 2 adjoining reaches approach nearest to a right angle. In such situations it not unfrequently excavates gulfs of considerable length within the bank. These gulfs are in the direction of the strongest parts of the stream; and are, in fact, the young shoots, as it were, which in time strike out and become branches of the river; for we generally find them at those turnings that have the smallest angles.

There are not wanting instances of a total change of course in some of the Bengal rivers. The Cosa River, equal to the Rhine, once ran by Purneah, and joined the Ganges opposite Rajemal. Its junction is now 45 miles higher up. Gour, the ancient capital of Bengal, stood on the banks of the Ganges. Appearances favour very strongly the opinion, that the Ganges had its former bed in the tract now occupied by the lakes and morasses between Nattore and Jaffiergunge, striking out of its present course at Bauleah, and passing by Pootyah. With an equal degree of probability, favoured by tradition, we may trace its supposed course by Dacca, to a junction with the Burrampooter or Megna near Fringybazar; where the accumulation of two such mighty streams probably scooped out the present amazing bed of the Megna.\*

In tracing the sea coast of the Delta, we find no less than 8 openings; each of which, without hesitation, one pronounces to have been in its time the principal mouth of the Ganges. Nor is the occasional deviation of the principal branch, probably the only cause of fluctuation in the dimensions of the Delta. One observes that the Deltas of capital rivers, the tropical ones particularly, encroach on the sea. Now, is not this owing to the mud and sand brought down by the rivers, and gradually deposited, from the remotest ages down to the present time? The rivers are loaded with mud and sand at their entrance into the sea; and the sea recovers its transparency at the distance of 20 leagues from the coast; which can only arise from the waters having precipitated their earthy particles within that space. The sand and mud banks at this time, extend 20 miles off some of the islands in the mouths of the Ganges and Burrampooter; and in many places rise within a few feet of the surface. Some future generation will probably see these banks rise above water, and succeeding ones possess and cultivate them! Next to earthquakes, perhaps the floods of the tropical rivers produce the quickest alterations in the face of our globe. Extensive islands are formed in the channel of the Ganges, during a period far short of that of a man's life; so that the whole process lies within the compass of his observation.

\* Megna and Burrampooter are names belonging to the same river in different parts of its course. The Megna falls into the Burrampooter; and, though a much smaller river, communicates its name to the other during the rest of its course.—Orig.



Some of these islands, 4 or 5 miles in extent, are formed at the angular turnings of the river, and were originally large sand banks thrown up round the points, but afterwards insulated by breaches of the river. Others are formed in the straight parts of the river, and in the middle of the stream; and owe their origin to some obstruction lurking at the bottom. Whether this be the fragments of the river bank; or a large tree swept down from it; or a sunken boat; it is sufficient for a foundation; and a heap of sand is quickly collected below it. This accumulates amazingly fast: in the course of a few years it peeps above water, and having now usurped a considerable portion of the channel, the river borrows on each side to supply the deficiency in its bed; and in such parts of the river we always find steep banks on both sides.\* Each periodical flood brings an addition of matter to this growing island; increasing it in height as well as extension, until its top is perfectly on a level with the banks that include it: and at that period of its growth it has mould enough on it for the purposes of cultivation, which is owing to the mud left on it when the waters subside, and is indeed a part of the economy which nature observes in fertilizing the lands in general.

While the river is forming new islands in one part, it is sweeping away old ones in other parts. In the progress of this destructive operation, we have opportunities of observing, by means of the sections of the falling bank, the regular distribution of the several strata of sand and earths, lying above each other in the order in which they decrease in gravity. As they can only owe this disposition to the agency of the stream that deposited them, it would appear that these substances are suspended at different heights in the stream, according to their respective gravities. We never find a stratum of earth under one of sand; for the muddy particles float nearest the surface.† I have counted 7 distinct strata in a section of one of these islands. Indeed, not only the islands, but most of the river banks wear the same appearance: for as the river is always changing its present bed, and verging towards the site of some former one now obliterated, this must necessarily be the case. As a strong presumptive proof of the wanderings of the Ganges from the one side of the Delta to the other, there is no appearance of virgin earth between the Tiperah hills on the east, and the province of Bardwan on the west; nor on the north till we arrive at Dacca and Bauleah. In all the sections of the numerous creeks and rivers in the Delta, nothing appears but sand and black mould in regular strata, till we arrive at the

\* This evidently points out the means for preventing encroachments on a river bank in the straight parts of its course, viz. to remove the shallows in the middle of its channel.—Orig.

† A glass of water taken out of the Ganges, when at its height, yields about 1 part in 4 of mud. No wonder then that the subsiding waters should quickly form a stratum of earth; or that the Delta should encroach on the sea!—Orig.



clay that forms the lower part of their beds. There is not any substance so coarse as gravel either in the Delta or nearer the sea than 400 miles, where a rocky point, a part of the base of the neighbouring hills, projects into the river: but out of the vicinity of the great rivers the soil is either red, yellow, or of a deep brown.

The annual swelling and overflowing of the Ganges appears to owe its increase as much to the rain water that falls in the mountains contiguous to its source, and to the sources of the great northern rivers that fall into it, as to that which falls in the plains of Hindoostan; for it rises  $15\frac{1}{2}$  feet out of 32, the sum total of its rising, by the latter end of June: and it is well known, that the rainy season does not begin in most of the flat countries till about that time. In the mountains it begins early in April; and by the latter end of that month, when the rain-water has reached Bengal, the rivers begin to rise, but by very slow degrees; for the increase is only about an inch per day for the first fortnight. It then gradually augments to 2 and 3 inches before any quantity of rain falls in the flat countries; and when the rain becomes general, the increase on a medium is 5 inches per day. By the latter end of July all the lower parts of Bengal, contiguous to the Ganges and Burrampooter, are overflowed, and form an inundation of more than 100 miles in width; nothing appearing but villages and trees, excepting very rarely the top of an elevated spot, the artificial mound of some deserted village, appearing like an island.

The inundations in Bengal differ from those in Egypt in this particular, that the Nile owes its floods entirely to the rain-water that falls in the mountains near its source; but the inundations in Bengal are as much occasioned by the rain that falls there, as by the waters of the Ganges; and as a proof of it, the lands in general are overflowed to a considerable height long before the bed of the river is filled. It must be remarked, that the ground adjacent to the river bank, to the extent of some miles, is considerably higher than the rest of the country,\* and serves to separate the waters of the inundation from those of the river till it overflows. This high ground is in some seasons covered a foot or more; but the height of the inundation within varies of course according to the irregularities of the ground, and is in some places 12 feet. Even when the inundation becomes general, the river still shows itself, as well by the grass and reeds on its banks, as by its rapid and muddy stream; for the water of the inundation acquires a blackish hue, by having been so long stagnant among grass and other vegetables: nor does it ever lose this tinge, which is a proof of the predominancy of the

\* This property of the bank is well accounted for by Count Buffon, who imputes it to the precipitation of mud made by the waters of the river, when it overflows. The inundation, says he, purifies itself as it flows over the plain; so that the precipitation must be greatest on the parts nearest to the margin of the river.—Orig.



rain water over that of the river ; as the slow rate of motion of the inundation, which does not exceed half a mile per hour, is of the remarkable flatness of the country.

There are particular tracts of land, which, from the nature of their culture, and species of productions, require less moisture than others ; and yet, by the lowness of their situation, would remain too long inundated, were they not guarded by dikes or dams, from so copious an inundation as would otherwise happen from the great elevation of the surface of the river above them. These dikes are kept up at an enormous expense ; and yet do not always succeed, for want of tenacity in the soil of which they are composed.

During the swoln state of the river, the tide totally loses its effect of counter-acting the stream ; and in a great measure that of ebbing and flowing, except very near the sea. It is not uncommon for a strong wind, that blows up the river for any continuance, to swell the waters 2 feet above the ordinary level at that season : and such accidents have occasioned the loss of whole crops of rice.\* A very tragical event happened at Luckipour in 1763, situated above 50 miles from the sea, by a strong gale of wind conspiring with a high spring tide, at a season when the periodical flood was within a foot and half of its highest pitch. It is said that the waters rose 6 feet above the ordinary level. Certain it is, that the inhabitants of a considerable district, with their houses and cattle, were totally swept away ; and, to aggravate their distress, it happened in a part of the country which scarce produces a single tree for a drowning man to escape to.

Embarkations of every kind traverse the inundation : those bound upwards, availing themselves of a direct course and still water, at a season when every stream rushes like a torrent. The wind too, which at this season blows regularly from the south-east, favours their progress ; insomuch that a voyage which takes up 9 or 10 days by the course of the river when confined within its banks, is now effected in 6. Husbandry and grazing are both suspended ; and the peasant traverses in his boat, those fields which in another season he was wont to plow ; happy that the elevated site of the river banks place the herbage they contain, within his reach, otherwise his cattle must perish.

The following is a table of the gradual increase of the Ganges and its branches, according to observations made at Jellinghy and Dacca.

In May it rose, at Jellinghy. . .	6 ft. 0 inc.	At Dacca. . . . .	2 ft. 4 inc.
June. . . . .	9 6	. . . . .	4 6
July . . . . .	12 6	. . . . .	5 6
In the first half of August. . .	4 0	. . . . .	1 11
In all . . . . .	32 0	. . . . .	14 3

\* The rice I speak of is of a particular kind ; for the growth of its stalk keeps pace with the increase of the flood at ordinary times, but is destroyed by a too sudden rise of the water. The harvest is often reaped in boats. There is also a kind of grass which overtops the flood in the same manner, and at a small distance has the appearance of a field of the richest verdure.—Orig.



These observations were made in a season, when the waters rose rather higher than usual ; so that we may take 31 feet for the medium of the increase.

The inundation is nearly at a stand for some days preceding the middle of August, when it begins to run off; for though great quantities of rain fall in the flat countries, during August and September, yet, by a partial cessation of the rains in the mountains, there happens a deficiency in the supplies necessary to keep up the inundation. The quantity of the daily decrease of the river is nearly in the following proportion: during the latter half of August, and all September, from 3 to 4 inches; from September to the end of November it gradually lessens from 3 inches to an inch and a half; and from November to the latter end of April, it is only half an inch per day at a medium. These proportions must be understood to relate to such parts of the river as are removed from the influence of the tides; of which more will be said by and by. The decrease of the inundation does not always keep pace with that of the river, by reason of the height of the banks; but after the beginning of October, when the rain has nearly ceased, the remainder of the inundation goes off quickly by evaporation, leaving the lands highly manured, and in a state fit to receive the seed, after the simple operation of plowing.

There is a circumstance attending the increase of the Ganges, little known or attended to; because few people have made experiments on the heights to which the periodical flood rises in different places. The circumstance alluded to, is, the difference of the quantity of the increase, as expressed in the foregoing tables, in places more or less remote from the sea. It is a fact, confirmed by repeated experiments, that from about the place where the tide commences, to the sea, the height of the periodical increase diminishes gradually, till it totally disappears at the point of confluence. Indeed, this is perfectly conformable to the known laws of fluids: the ocean preserves the same level at all seasons, under similar circumstances of tide, and necessarily influences the level of all the waters that communicate with it, unless precipitated in the form of a cataract. Could we suppose, for a moment, that the increased column of water, of 31 feet perpendicular, was continued all the way to the sea, by some preternatural agency: whenever that agency was removed, the head of the column would diffuse itself over the ocean, and the remaining parts would follow, from as far back as the influence of the ocean extended; forming a slope, whose perpendicular height would be 31 feet. This is the precise state in which we find it. At the point of junction with the sea, the height is the same in both seasons at equal times of the tides. At Luckipour there is a difference of about 6 feet between the heights in the different seasons; at Dacca, and places adjacent, 14; and near Custee, 31 feet. Here then is a regular slope; for the distances between the places bear a proportion to the respective heights. This slope must add to



the rapidity of the stream; for, supposing the descent to have been originally 4 inches per mile, this will increase it to about  $5\frac{1}{4}$ . Custee is about 240 miles from the sea, by the course of the river; and the surface of the river there, during the dry season, is about 80 feet above the level of the sea at high water. Thus far does the ocean manifest its dominion in both seasons: in the one by the ebbing and flowing of its tides; and in the other by depressing the periodical flood, till its surface coincides as nearly with its own, as the descent of the channel of the river will admit.

Similar circumstances take place in the Jellinghy, Hoogly, and Burrampooter rivers; and probably in all others that are subject either to periodical or occasional swellings. Not only does the flood diminish near the sea, but the river banks diminish in the same proportion; so that in the dry season the height of the periodical flood may be known by that of the bank. If it be objected to the above solution, that the lowness of the banks in places near the sea, is the true reason why the floods do not attain so considerable a height, as in places farther removed from it, and where the banks are high; for that the river, wanting a bank to confine it, diffuses itself over the surface of the country: in answer to this, it may be observed, that it is proved by experiment, that at any given time, the quantity of the increase in different places bears a just proportion to the sum total of the increase in each place respectively: or, in other words, that when the river has risen 3 feet at Dacca, where the whole rising is about 14 feet; it will have risen upwards of  $6\frac{1}{2}$  feet at Custee, where it rises 31 feet in all.

The quantity of water discharged by the Ganges, in one second of time, during the dry season, is 80,000 cubic feet; but in the place where the experiment was made, the river, when full, has thrice the volume of water in it; and its motion is also accelerated in the proportion of 5 to 3: so that the quantity discharged in a second at that season is 405,000 cubic feet. If we take the medium the whole year through, it will be nearly 80,000 cubic feet in a second.

The Burrampooter, which has its source from the opposite side of the same mountains that give rise to the Ganges, first takes its course eastward, or directly opposite to that of the Ganges, through the country of Thibet, where it is named Sanpoo or Zanciu, which bears the same interpretation as the Gonga of Hindoostan, namely, the River. Its course through Thibet, as given by Father Du Halde, and formed into a map by Mr. D'Anville, though sufficiently exact for the purposes of general geography, is not particular enough to ascertain the precise length of its course. After winding with a rapid current through Thibet, it washes the border of the territory of Lassa, in which is the residence of the grand Lama, and then deviating from an east to a south-east course, it approaches within 220 miles of Yunan, the westernmost province of China. Here



it appears, as if undetermined whether to attempt a passage to the sea by the Gulf of Siam, or by that of Bengal; but seemingly determining on the latter, it turns suddenly to the west through Assam, and enters Bengal on the north-east. I have not been able to learn the exact place where it changes its name; but as the people of Assam call it Burrampoot, it would appear that it takes this name on its entering Assam. After its entry into Bengal, it makes a circuit round the western point of the Garrow Mountains; and then, altering its course to south, it meets the Ganges about 40 miles from the sea.

Father Du Halde expresses his doubts concerning the course that the Sanpoo takes after leaving Thibet, and only supposes generally that it falls into the gulf of Bengal. M. D'Anville, his geographer, with great reason supposed the Sanpoo and Ava River to be the same: and in this he was justified by the information which his materials afforded him: for the Burrampooter was represented to him, as one of the inferior streams that contributed its waters to the Ganges, and not as its equal or superior; and this was sufficient to direct his researches, after the mouth of the Sanpoo River, to some other quarter. The Ava River, as well from its bulk, as the bent of its course for some hundred miles above its mouth, appeared to him to be a continuation of the river in question: and it was accordingly described as such in his maps, the authority of which was justly esteemed as decisive; and till the year 1765, the Burrampooter, as a capital river, was unknown in Europe.

On tracing this river in 1765, I was no less surprized, at finding it rather larger than the Ganges, than at its course previous to its entering Bengal. This I found to be from the east; though all the former accounts represented it as from the north: and this unexpected discovery soon led to inquiries, which furnished an account of its general course to within 100 miles of the place where Du Halde left the Sanpoo. I could no longer doubt that the Burrampooter and Sanpoo were one and the same river: and to this was added the positive assurances of the Assamers, "That their river came from the north-west, through the Bootan mountains." And to place it beyond a doubt, that the Sanpoo River is not the same with the river of Ava, but that this last is the great Nou Kian of Yunan; I have in my possession a manuscript draught of the Ava River, to within 150 miles of the place where Du Halde leaves the Nou Kian, in its course towards Ava; together with very authentic information that this river (named Irabattey by the people of Ava) is navigable from the city of Ava into the province of Yunan in China.

The Burrampooter, during a course of 400 miles through Bengal, bears so intimate a resemblance to the Ganges, except in one particular, that one description may serve for both. The exception I mean is, that during the last 60 miles before its junction with the Ganges, it forms a stream which is regularly from 4



to 5 miles wide, and but for its freshness might pass for an arm of the sea. Common description fails in an attempt to convey an adequate idea of the grandeur of this magnificent object.

I have already endeavoured to account for the singular breadth of the Megna, by supposing that the Ganges once joined it where the Issamutty now does; and that their joint waters scooped out its present bed. The present junction of these two mighty rivers below Luckipour, produces a body of running fresh water, hardly to be equalled in the old hemisphere, and, perhaps, not exceeded in the new. It now forms a gulf interspersed with islands, some of which rival, in size and fertility, our Isle of Wight. The water at ordinary times is hardly brackish at the extremities of these islands; and, in the rainy season, the sea, or at least the surface of it, is perfectly fresh to the distance of many leagues out.

The Bore (which is known to be a sudden and abrupt influx of the tide into a river or narrow strait) prevails in the principal branches of the Ganges, and in the Megna; but the Hoogly River, and the passages between the islands and sands situated in the gulf, formed by the confluence of the Ganges and Megna, are more subject to it than the other rivers. This may be owing partly to their having greater embouchures in proportion to their channels, than the others have, by which means a larger proportion of tide is forced through a passage comparatively smaller; and partly to there being no capital openings near them, to draw off any considerable portion of the accumulating tide. In the Hoogly or Calcutta River, the Bore commences at Hoogly Point, the place where the river first contracts itself, and is perceptible above Hoogly Town; and so quick is its motion, that it hardly employs 4 hours in travelling from one to the other, though the distance is near 70 miles. At Calcutta it sometimes occasions an instantaneous rise of 5 feet: and both here, and in every other part of its track, the boats, on its approach, immediately quit the shore, and make for safety to the middle of the river.

In the channels, between the islands in the mouth of the Megna, &c. the height of the Bore is said to exceed 12 feet; and is so terrific in its appearance, and dangerous in its consequences, that no boat will venture to pass at spring tide. After the tide is fairly past the islands, no vestige of a Bore is seen, which may be owing to the great width of the Megna, in comparison with the passages between the islands; but its effects are visible enough by the sudden rising of the tides.

*X. Astronomical Observations on the Rotation of the Planets round their Axes, made with a View to determine whether the Earth's Diurnal Motion is perfectly Equable. By Mr. William Herschel, of Bath. p. 115.*

The various motions of the planet we inhabit; the annual revolution in its



orbit; the diurnal rotation round its axis; the menstrual motion round the common centre of gravity of the moon and earth; the precession of the equinoctial points; the diminution of the obliquity of the ecliptic; the nutation of the earth's axis: in short, every one of the motions that arise from the actions of the sun, moon, and planets, combined with the spheroidical figure of the earth, and the projectile and rotatory motions first impressed on it, have all been considered by astronomers, and their real and apparent inequalities investigated. And to the great honour of modern astronomers it must be confessed, that no science has ever made such considerable strides towards perfection in so short a time as astronomy has done since the invention of the telescope.

There is one of the motions of the earth however which, it seems, has hitherto escaped the scrutiny of observers: viz. the diurnal rotation round its axis. The principal reason why this has not been looked into, is probably the difficulty of finding a proper standard to measure it by; since it is itself used as the standard by which we measure all the other motions. We have indeed no cause to suspect any very material periodical irregularity, either diurnal, menstrual, or annual; for the great perfection of our present time-pieces would have discovered any considerable deviation from that equability which we have hitherto ascribed to the diurnal motion of the earth. And yet, it is not perhaps altogether impossible but that inequalities may exist in this motion, which, in an age when observations are carried to such a degree of refinement, may be of some consequence.

To show how far time-keepers, though ever so perfect, are from being a proper, or at least a sufficient standard, to examine the diurnal motion of the earth by, it may be asked, whether it is probable, that any clock would have discovered to us the aberration of the fixed stars? And yet that aberration produces a change in longitude, and of consequence in right ascension, which causes an annual irregularity in a star's coming to the meridian, which a time-piece, were it a sufficient standard, would soon have discovered, and which we might have attributed to an inequality of the earth's diurnal motion, had we not been acquainted with its real cause. And if we were to find out any apparent irregularity, acceleration, or retardation, should we not much rather suspect the clock than the diurnal motion? We may therefore venture to say, that the aberration of the fixed stars, though attended with the above-mentioned consequence, would for ever have remained a secret to us, had it not been found out by other methods than time-keepers.

Now, if time-pieces fail us in this critical case, where we stand in the greatest need of their assistance, it is almost in vain to expect any help from another quarter; for what mechanical movement on earth, or motion of the heavens, is there that can measure out such equal portions of time as we require to compare the diurnal motion of the earth to? However, to proceed, since we have



already great proofs that the diurnal motion of the earth is, if not perfectly equable, at least more so than any other motion we are acquainted with, it will not appear absurd to suppose the diurnal rotation of the other planets to be so likewise. This suggested the thoughts of estimating the diurnal motion of one planet very exactly by that of another, making each the standard of the other. In this manner we may obtain a comparative view, by which future astronomers, if they shall hereafter be inclined to pursue the subject, may be enabled to make some estimate of the general equability of the rotatory motions of the planets. For if in length of time they should perceive some small retardation in the diurnal motion of a planet, occasioned by some resistance of a very subtle medium in which the heavenly bodies perhaps move; or, on the other hand, if there should be found an acceleration from some cause or other, they might then ascribe the alteration either to the diurnal motion of the earth, or to the gyration of the other planet, according as circumstances, or observed phenomena, should make one or the other of these opinions most probable.

Now, this method of comparing together different rotations of several planets, simple as it may appear, was not without some difficulties. In the first place it was evident, that the common account of their diurnal motions, (Keill, Ast. Lect. 5) which makes that of Jupiter  $9^h 56^m$ , of Mars  $24^h 40^m$ , how true soever it may be in a general way, was much too inaccurate for this critical purpose. The gyration of Venus was still less to be depended on, being only noted to the hour, without the minutes: it became therefore necessary to proceed to observations of a more determinate kind. From what had already been seen of the rotation of the planets, Mr. H. concluded, that Mars on several accounts would be the most eligible planet for his purpose: for the spots on Jupiter change so often, that it is not easy, if at all possible, to ascertain the identity of the same appearance, for any considerable length of time. Nor do the dark spots only change their place, which may be supposed to be large black congeries of vapours and clouds swimming in the atmosphere of Jupiter; but also the bright spots, though they may adhere firmly to the body of Jupiter, may undergo some apparent change of situation, by being differently covered or uncovered on one side or the other, by alterations in the belts. For Mr. H. had observed the revolution of a very bright spot, not suspected of any change of situation, to be first, by one set of observations, at the rate of  $9^h 51^m 45^s.6$ ; and afterwards, by another set immediately following, at the rate of  $9^h 50^m 48^s$ .

As the principal belts on Jupiter are equatorial, and as we have certain constant winds on our planet, especially near the equator, that regularly, for certain periods, blow the same way, it is easily supposed, that they may form equatorial belts by gathering together the vapours which swim in our atmosphere, and carrying them about in the same direction. This will, by analogy, account for all



the irregularities of Jupiter's revolutions, deduced from spots on his disc that may have changed their situation; for if we suppose the rotation of Jupiter, according to Cassini, to be  $9^h 56^m$ , then some spots that Mr. H. had observed must have been carried through about  $60^\circ$  of Jupiter's equator in 22 of his revolutions or days. This would certainly be a very great velocity in the clouds, which is however not unparalleled by what has happened in our own atmosphere.

But to return: on the planet Mars we see spots of a different nature; their constant and determined shape, as well as remarkable colour, show them to be permanent and fastened to the body of the planet. These will give the revolution of his equator to a great certainty, and by a great number of revolutions, to a very great exactness also. Supposing then, that by a method hereafter described, we can determine whether a spot on the disc of Mars is, or is not, in the line which joins the centre of the earth and the centre of that planet, to half an hour's time with certainty, probably 10 or 12 minutes will be found sufficient for that purpose, in this case we shall in 30 days have the revolution true to a minute; and, by continuing these observations for 3 months, we shall have it to  $20^s$ . When we are so far certain, we can easily arrive to a much greater degree of exactness; for as we now can no longer mistake a whole revolution, if we take the time of any particular spot being in the line which joins the centres of the planets during one opposition of Mars, and take the same again at or near the next opposition, we shall have an interval of about 780 days, which will give the diurnal motion of that planet true to about  $2^s$ . The next opposition will give it to 1, and so forth; by which means, and by taking a proper number of such periods, we may determine the rotation of Mars to as great an exactness as we shall think necessary for the purpose of our comparative view. Had such observations as these been made 2000, or perhaps only so many hundred years ago, we might now, by repeating them, most probably become acquainted with some curious minute changes of the solar system that have hitherto passed unnoticed.

There is a certain circumstance which would almost create a suspicion that there has been some retardation in the diurnal motion of the earth. The difference between the equatorial and polar diameters of the earth, by actual measurement, has been found to be about 36 English miles and  $\frac{2}{10}$ ; but, by a calculation wherein the present rotation is made use of, it will only amount to about 33 miles and  $\frac{8}{10}$ ; from which it should seem probable, that when the earth assumed the present form, the diurnal rotation was somewhat quicker than it is at present, by which means the centrifugal force bore a greater proportion to the force of gravity, to which it is contrary, and thus occasioned a higher elevation of the equatorial parts. But Mr. H. would not lay much stress on this argument; for, in the calculation it has been supposed, that the earth is nearly of an equal density at the surface and towards the centre, which it seems is not agreeable to



some late curious experiments and calculations that have been made on the attraction of a mountain,\* the result of which ought now to be taken into consideration, and the calculation repeated. If all the data could be exactly depended on, it would be practicable enough from the laws of gravity, and the present rotation and given form of the earth, to find the centrifugal force required to produce that form, and thence to show what must have been its diurnal motion when it assumed the same. However, these are researches that in the present situation Mr. H. neither had opportunity nor perhaps ability enough to investigate properly; and which therefore he hoped some of our excellent mathematicians will think worth while to look into.

Mr. H. now relates his observations on Jupiter and Mars. The telescopes used were of his own construction; and were, a 20-feet Newtonian reflector, a 10-feet reflector of the same form, and a 7-feet reflector, already mentioned in his paper on the mountains of the moon. His time he gained by equal altitudes taken with a brass quadrant of 2-feet radius, carrying a telescope which magnifies about 40 times: for the correction of altitudes taken of the sun, he used De Lalande's tables. He kept the time by two very good pieces; one having a deal pendulum-rod, the other a compounded one of brass and iron, both having a proper contrivance not to stop when winding up. The rate of going of his clocks he determined by the transit of stars.

*Observations on Jupiter in the year 1778.*—Feb. 24, clock 1<sup>m</sup> 10<sup>s</sup> too soon. About 9 o'clock saw a bright belt on one part of Jupiter's disc. About 10 o'clock it was advanced as far as the centre. At 11<sup>h</sup> the white belt still more advanced. At 11<sup>h</sup> 25<sup>m</sup> it approached towards the edge of the disc; and at 12<sup>h</sup> was extended all over it.

Feb. 25, 8<sup>h</sup>, the same bright belt observed yesterday extended all over. At 8<sup>h</sup> 45<sup>m</sup> it was divided by a darkish spot, situated at some distance from the centre. At 9<sup>h</sup> 5<sup>m</sup> the small dark division was advanced a little farther than the centre. At 9<sup>h</sup> 23<sup>m</sup> the spot visibly advanced a considerable deal farther.

March 2, 8<sup>h</sup> 2<sup>m</sup>, the darkish spot, with some alteration in its shape, was in the middle of the disc.

March 3, 10<sup>h</sup> 34<sup>m</sup>, the bright belt on the south of the equator was in the middle: that is, if a line be drawn perpendicular to the equatorial belt, and through the centre, the end of the equatorial belt touches it. At 13<sup>h</sup> 49<sup>m</sup> the darkish spot, in which there had been some alteration, seemed to be in the centre.

March 14, the clock altered to true equated time; but the rate of going not

\* See Mr. Hutton's Account of the Calculations made from the Survey and Measures taken at Schihallien, in order to ascertain the mean Density of the Earth. Phil. Trans., 1778, Abridg. vol. xiv. p. 420.—Orig.



changed, being well regulated. At  $7^h 35^m$  the spot was in the centre, but did not seem quite to fill the white belt; nor was it so large and distinct as it was before.

April 7,  $9^h 31^m$ , 3 dark spots in the equatorial belt nearly in the centre.

April 12,  $7^h 50^m$ , the 3 dark spots in the centre. The southernmost of the 3 is nearly quite vanished; the other 2 are also much fainter. They are, however, distinct enough to be known.

*Observations on Jupiter in 1779.*—April 14, clock  $52^s$  too late. At  $8^h 48^m$  a remarkable bright spot in the equatorial belt towards the north is in the centre. At  $8^h 58^m$ , the spot a little past the centre.

April 19, clock true mean time. At  $7^h 10^m$ , a bright spot just in the centre, which, from its shape, seems to be the same that was there April 14th. At  $7^h 20^m$ , the spot visibly past the centre.

April 23, clock shows true time. At  $9^h 38^m$ , the same bright spot in the centre. At  $9^h 43^m$ , it was past the centre.

Comparing together the observations made in the year 1778, Feb. 24 and March 3, we obtain an interval of 7 days 34 minutes, which being divided by 17 revolutions made by Jupiter on his axis, we have the time of 1 synodical revolution equal to  $9^h 54^m 56^s.4$ .

The dark spot on Feb. 25 was observed some time before, and also just after it was past the centre; therefore supposing it to be in the centre about  $8^h 58^m$ , we have

1°. From Feb.  $25^d 8^h 58^m$  to March  $2^d 8^h 2^m = 4^d 23^h 4^m$ , which divided by 12 rev. gives 1 revol. =  $9^h 55^m 20^s$ .

2°. From Feb.  $25^d 8^h 58^m$  to March  $3^d 13^h 49^m = 6^d 4^h 51^m = 15$  revol. which gives 1 rev. =  $9^h 55^m 24^s$ .

3°. From Feb.  $25^d 8^h 58^m 0^s$  to March  $14^d 7^h 36^m 10^s$ , allowing  $1^m 10^s$  for the alt. of the clock, =  $16^d 22^h 38^m 10^s = 41$  revol. which gives 1 revol. =  $9^h 55^m 4^s.6$ .

4°. From March  $2^d 8^h 2^m$  to March  $3^d 13^h 49^m = 1^d 5^h 47^m = 3$  revol. which gives 1 revol. =  $9^h 55^m 40^s$ .

5°. From March  $2^d 8^h 2^m 0^s$  to March  $14^d 7^h 36^m 10^s = 11^d 23^h 34^m 10^s = 29$  revol. hence 1 revol. =  $9^h 54^m 58^s.2$ .

6°. From March  $3^d 13^h 49^m 0^s$  to  $14^d 7^h 36^m 10^s = 10^d 17^h 47^m 10^s = 26$  revol. hence 1 revol. =  $9^h 54^m 53^s.4$ .

7°. From April  $7^d 9^h 31^m$  to  $12^d 7^h 50^m = 4^d 22^h 29^m = 12$  revol. hence 1 revol. =  $9^h 51^m 35^s$ .

Again, comparing together the observations of 1779, which were made with the utmost attention to time, we have,

1°. From April 14<sup>d</sup> 8<sup>h</sup> 48<sup>m</sup> 52<sup>s</sup> to April 19<sup>d</sup> 7<sup>h</sup> 40<sup>m</sup> 0<sup>s</sup> = 4<sup>d</sup> 22<sup>h</sup> 21<sup>m</sup> 8<sup>s</sup> = 12 revol. hence 1 revol. = 9<sup>h</sup> 51<sup>m</sup> 45<sup>s</sup>.6.

2°. From April 19<sup>d</sup> 7<sup>h</sup> 40<sup>m</sup> to April 23<sup>d</sup> 9<sup>h</sup> 38<sup>m</sup> = 4<sup>d</sup> 2<sup>h</sup> 28<sup>m</sup> = 10 revol. hence 1 revol. = 9<sup>h</sup> 50<sup>m</sup> 48<sup>s</sup>.

And taking both together, from April 14<sup>d</sup> 8<sup>h</sup> 48<sup>m</sup> 52<sup>s</sup> to April 23<sup>d</sup> 9<sup>h</sup> 38<sup>m</sup> 0<sup>s</sup> = 9<sup>d</sup> 0<sup>h</sup> 49<sup>m</sup> 8<sup>s</sup> = 22 revol. hence 1 revol. = 9<sup>h</sup> 51<sup>m</sup> 19<sup>s</sup>.4.

These several results are so exceedingly various, that it is evident Jupiter is not a proper planet for the critical purpose of a comparative view of the diurnal motions; nor can this great variety proceed from any inaccuracy in the observations; for, Mr. H. thinks it is hardly possible to make a mistake in the situation of a spot that shall amount to so much as 5 minutes of time. The observation of April 23, 1779, was made with a view to ascertain this point, when it was found that 5 minutes of time made a sensible difference in the situation of a spot when near the centre.

By a comparison of the different periods it appears, that a spot which is carried about in the atmosphere of Jupiter generally suffers an acceleration, or, which is the same thing, performs its revolutions by degrees in less time than it did at first; which is agreeable enough to the theory of equatorial winds, since it may probably take up some time before a spot can acquire a sufficient velocity to go as fast as those winds may blow. And, by the bye, if Jupiter's spots should be observed in different parts of his year, and be found in some to be accelerated, in others to be retarded, it would almost amount to a demonstration of his monsoons and their periodical changes; but if his axis should not be inclined enough to his orbit, to occasion such a change, they may probably always blow in the same direction.

*Observations on Mars in the year 1777.*—Twenty-feet Newtonian reflector; power 300. April 8, 7<sup>h</sup> 30<sup>m</sup>, observed 2 spots on Mars, with a bright belt or partition between them. The belt not very well defined. At 9<sup>h</sup> 30<sup>m</sup>, the spots advanced, and more spotted parts visible. At 10<sup>h</sup> the revolution of Mars on were his axis very evident.

April 17, ten-feet Newtonian reflector; power about 211. At 7<sup>h</sup> 50<sup>m</sup>, two bright spots, so luminous that they seemed to project beyond the disc.

April 26, ten-feet reflector; power 211. At 9<sup>h</sup> 5<sup>m</sup>, the spots on the planet very faint.

*Observations on Mars in the year 1779.*—May 9th, clock 15<sup>s</sup> too fast; by equal altitudes on the 14th of April, and by the transit of a star, it was found to lose 1<sup>m</sup>.45 per day. At 11<sup>h</sup> 1<sup>m</sup> by the clock, a very remarkable dark spot not far from the centre. At 11<sup>h</sup> 30<sup>m</sup> the figures gone from the centre.

May 11, clock 12<sup>s</sup> too fast. At 10<sup>h</sup> 18<sup>m</sup>, the same spot that was visible May 9, is on the disc, the darkest place being entirely south-east of the centre. At



11<sup>h</sup> 43<sup>m</sup>, the darkest part is almost arrived at the centre. At 12<sup>h</sup> 17<sup>m</sup>, the dark spot is with its edge just near the centre.

May 13. Seven-foot reflector; power 222. Clock 9<sup>s</sup> too fast. At 11<sup>h</sup> 26<sup>m</sup>, Mars seems now to be in the same situation he was the 11th, at 10<sup>h</sup> 8<sup>m</sup>.

May 22, clock 4<sup>s</sup> too slow. At 12<sup>h</sup> 5<sup>m</sup>, the figure of May 11th not on the disc; but some other fainter spots are visible.

June 6, the clock loses 1<sup>s</sup>.9 per day. At 10<sup>h</sup> 10<sup>m</sup>, the same figure is on the disc of Mars which was there April 8, 1777, at 7<sup>h</sup> 30<sup>m</sup>.

June 15, clock 17<sup>m</sup> too slow. At 9<sup>h</sup> 45<sup>m</sup>, the same figure is on Mars that was there May 9, at 11<sup>h</sup> 1<sup>m</sup>; but it is more advanced. Mr. H. supposed it to be the same, and in the same situation, as April 17, 1777, at 7<sup>h</sup> 50<sup>m</sup>.

June 17, clock 20<sup>s</sup> slow. At 9<sup>h</sup> 12<sup>m</sup>, the dark spot on Mars is rather more advanced than it was May 11th, at 10<sup>h</sup> 18<sup>m</sup>. At 10<sup>h</sup> the spot visibly advanced.

June 19, clock 22<sup>s</sup> too slow by the transit of  $\delta$  Scorpii observed this evening. At 8<sup>h</sup> 40<sup>m</sup>, the figure on the disc of Mars appears now to be as it was April 26, 1777. At 11<sup>h</sup> 47<sup>m</sup>, the state of the air near the horizon is very unfavourable. With much difficulty it can but just be seen that the figure is not quite so far advanced as it was May 11th, at 11<sup>h</sup> 43<sup>m</sup>, but can certainly not be above 2 or 3 minutes from it.

Now to examine the result of the above-mentioned observations: comparing together the 2 following short intervals of the year 1779, we have,

From May 9<sup>d</sup> 11<sup>h</sup> 0<sup>m</sup> 45<sup>s</sup> to May 11<sup>d</sup> 12<sup>h</sup> 16<sup>m</sup> 48<sup>s</sup> = 2<sup>d</sup> 1<sup>h</sup> 16<sup>m</sup> 3<sup>s</sup> = 2 revol. hence 1 revol. = 24<sup>h</sup> 38<sup>m</sup> 1<sup>s</sup>.5

A 2d small interval. From May 11<sup>d</sup> 10<sup>h</sup> 17<sup>m</sup> 48<sup>s</sup> to 13<sup>d</sup> 11<sup>h</sup> 25<sup>m</sup> 51<sup>s</sup> = 2<sup>d</sup> 1<sup>h</sup> 8<sup>m</sup> 3<sup>s</sup> = 2 revol. hence 4 revol. = 24<sup>h</sup> 34<sup>m</sup> 1<sup>s</sup>.5.

Here we have 2 very short intervals that agree to 4<sup>m</sup>, which is more than we could have expected in such short periods of time. Comparing together observations that were made at a greater distance, we find, first monthly period, from May 11<sup>d</sup> 10<sup>h</sup> 17<sup>m</sup> 48<sup>s</sup> to June 17<sup>d</sup> 9<sup>h</sup> 9<sup>m</sup> 20<sup>s</sup> allowing 3<sup>m</sup>, because the observation says the spot was rather more advanced, = 36<sup>d</sup> 22<sup>h</sup> 51<sup>m</sup> 32<sup>s</sup> = 36 revol. hence 1 revol. = 24<sup>h</sup> 38<sup>m</sup> 5<sup>s</sup>.9.

Second monthly period, from May 11<sup>d</sup> 11<sup>h</sup> 42<sup>m</sup> 48<sup>s</sup> to June 19<sup>d</sup> 11<sup>h</sup> 50<sup>m</sup> 22<sup>s</sup>, allowing 3<sup>m</sup> for the time the spot would have taken to come to the place mentioned, = 39<sup>d</sup> 0<sup>h</sup> 7<sup>m</sup> 34<sup>s</sup> = 38 revol. hence 1 revol. = 24<sup>h</sup> 38<sup>m</sup> 5<sup>s</sup>.4.

Third monthly period, from May 13<sup>d</sup> 11<sup>h</sup> 25<sup>m</sup> 51<sup>s</sup> to June 17<sup>d</sup> 9<sup>h</sup> 9<sup>m</sup> 20<sup>s</sup> = 34<sup>d</sup> 21<sup>h</sup> 43<sup>m</sup> 29<sup>s</sup> = 34 revol. hence 1 revol. = 24<sup>h</sup> 38<sup>m</sup> 20<sup>s</sup>.3.

This last is perhaps as likely to be near the truth as any, since the same spot was here observed for the 3d time, and therefore its motion become more familiar.

Here we have 3 longer periods that agree to 15 seconds, which is quite suf-



ficient for extending the interval of time to those observations that were made in the year 1777. But as these are the synodical revolutions, it will be necessary first to reduce them to syderal rotations. In fig. 7, pl. 1, let us suppose the orbit of Mars,  $MABC$ , to be in the same plane with the orbit of the earth,  $EDFG$ ; and the axis of Mars to be perpendicular to his orbit. Let  $M$ ,  $E$ ,  $m$ ,  $e$ , be the situations of Mars and the earth on the 13th of May and 17th of June; then will the line  $EM$ , that connects the centres of Mars and the earth, point out the geocentric place of Mars on the 13th of May; and the line  $em$ , the geocentric place of the same planet on the 17th of June. Draw  $er$  and  $ms$  parallel to  $ER$ ; then will  $er$  point out the geocentric place of Mars on the 13th of May; and the angle  $sme$  is equal to the angle  $mer$ . Now, by an ephemeris the geocentric place of Mars, May 13, at  $11^h 26^m$ , was  $7^\circ 20' 59'' 21''$ ; and on the 17th of June, at  $9^h 9^m$ , it was  $7^\circ 12' 27'' 22''$ , by which we obtain the difference or angle  $rem = ems = 8^\circ 31' 59''$ .

Now a spot on Mars, situated in the direction  $ME$ , will have made a syderal revolution when it returns to the same, or a parallel direction  $ms$ . From which we gather, that the spot on the 17th of June, after coming to the line  $me$ , where it finishes the synodical revolution, will have to go through an arch of  $8^\circ 31' 59''$ , in order to arrive into the direction of the line  $ms$ , where it finishes the syderal rotation. The time it will take to go through this arch, at the syderal rate of  $24^h 39^m 20^s$  to 360 degrees, or  $4^s.109$  per minute of a degree, will be  $35^m 3^s.8$ ; this being divided by the number of revolutions 34, gives  $1^m 1^s.8$ ; which, added to  $24^h 38^m 20^s.3$ , gives  $24^h 39^m 22.1$  for the syderal revolution of Mars, as found by the 3d of the monthly periods. This quantity will help us to find a proper divisor for the 3 following long biennial periods.

It is to be observed, that Mars has been retrograde in the above example, for which reason the measure of the angle  $ems$  was to be added to the synodical revolution when we wanted to find the syderal rotation; but if he had been direct, or if his place had been more advanced in the ecliptic than that to which we compared it, as at  $\mu$ , then the line  $\mu\sigma$  parallel to  $EM$  would be the direction to which the spot should return, in order to accomplish a syderal revolution, and therefore the quantity of the angle  $\sigma\mu e = \mu er$ , or difference of the geocentric places ought to be subtracted from the synodical revolution to obtain the syderal one.

First biennial period from 1777, April  $8^d 7^h 30^m$  to 1779, June  $6^d 10^h 10^m = 789^d 2^h 40^m$ .

The geocentric places of Mars at those times were,  $6^\circ 6' 31' 26''$  and  $7^\circ 13' 48' 30''$ , their diff.  $1^\circ 7' 17' 4''$ , turned into time, at  $4^s.109$  per minute of a degree, and subtracted, because Mars is more advanced in the ecliptic, is  $789^d 2^h 40^m 0^s - 2^h 33^m 11^s.8 = 789^d 0^h 6^m 48^s.2 = 768$  revol. hence 1 revol. =  $24^h 39^m 23^s.03$ .



Second biennial period, 1777, April 17<sup>d</sup> 7<sup>h</sup> 50<sup>m</sup> 0<sup>s</sup> to 1779, June 15<sup>d</sup> 9<sup>h</sup> 45<sup>m</sup> 17<sup>s</sup> = 789<sup>d</sup> 1<sup>h</sup> 55<sup>m</sup> 17<sup>s</sup>. The geocentric places 6° 3<sup>d</sup> 31' 27" and 7° 12<sup>d</sup> 40' 23", the diff. 1° 9<sup>d</sup> 8' 56", turned into time 789<sup>d</sup> 1<sup>h</sup> 55<sup>m</sup> 17<sup>s</sup>, and subtracting 2<sup>h</sup> 40<sup>m</sup> 52<sup>s</sup>, leaves 788<sup>d</sup> 23<sup>h</sup> 14<sup>m</sup> 25<sup>s</sup> = 768 revol. hence 1 revol. = 24<sup>h</sup> 39<sup>m</sup> 18<sup>s</sup>.94.

Third biennial period, 1777, April 26<sup>d</sup> 9<sup>h</sup> 5<sup>m</sup> 0<sup>s</sup> to 1779, June 19<sup>d</sup> 8<sup>h</sup> 40<sup>m</sup> 22<sup>s</sup> = 783<sup>d</sup> 23<sup>h</sup> 35<sup>m</sup> 22<sup>s</sup>. The geocentric places 6° 1° 24' 36" and 7° 12° 31' 48", the diff. 1° 11° 7' 12" turned into time gives 783<sup>d</sup> 23<sup>h</sup> 35<sup>m</sup> 22<sup>s</sup>, and subtracted 2<sup>h</sup> 45<sup>m</sup> 15<sup>s</sup>.6 leaves 783<sup>d</sup> 20<sup>h</sup> 50<sup>m</sup> 6<sup>s</sup>.4 = 763 revol. hence 1 revol. = 24<sup>h</sup> 39<sup>m</sup> 23<sup>s</sup>.04.

As these 3 periods are supported by observations of equal validity, Mr. H. takes a mean of them all for the nearest approximation to the true sydereal revolution of Mars on his axis, which therefore is 24<sup>h</sup> 39<sup>m</sup> 21<sup>s</sup>.67.

It remains now only to see how far we may depend on this determination of Mars's diurnal rotation as coming near the truth; and looking over those causes which may possibly produce any errors, we find, first of all, that in the long biennial periods a mistake in the number of revolutions would produce a considerable deviation from truth. Secondly, in the observations of a spot which moves so slow, we are also liable to some considerable mistake in estimating the time when it comes to a certain place; and the more so, if that place is not the centre. Lastly, the time itself is liable to inaccuracy.

As to the 1st, it appears from the 3 monthly periods observed in the year 1779, when the proper allowances for the geocentric places are made, that the sydereal revolution of Mars cannot well be less than 24<sup>h</sup> 39<sup>m</sup> 5<sup>s</sup>, nor more than 24<sup>h</sup> 39<sup>m</sup> 22<sup>s</sup>; but if we should divide any one of the 3 biennial periods by a supposed number of revolutions, only one more or one less than we have done, the difference would be so considerable, that nothing but a mistake in every one of the 3 monthly periods, of at least one whole hour, could justify such a supposition; and that such a mistake in the situation of a spot on Mars cannot have been made in those observations, is evident enough from the exactness with which they were made, and from their agreement with each other.

The 2d cause of error, which is the uncertainty in assigning the exact time when a spot comes to the centre, is of some force. But it seems highly probable, from the manner in which the spots on Mars pass over the disc of that planet, that there can hardly be so great an error as 10<sup>m</sup> in an observation of any remarkable spot's coming to the centre. However, not being willing to trust more to the eye than ought to be done, Mr. H. had recourse to the following experiment. He drew several circles of 1 inch radius, taking care to make no visible impression of a centre; and placed in each a fine point at the several distances of .0424, .0636, .0848, in ten thousands of an inch from the real centre; some to the right, others to the left. These measures are the sines to radius 1, of 2° 26', 3° 39', and 4° 52'; which are the arches a spot on



Mars passes over in 10, 15, 20<sup>m</sup> minutes respectively. He exposed them to several persons unacquainted with his designs, and found, that not one of them made a single mistake in saying whether the point was, or was not, in the centre of the circle, and which way it deviated from it. As the direction of the motion of a spot on Mars is known, he thought the persons who were to judge of the place of the points were entitled to be acquainted with the line in which they were placed, which for that reason was always to the right and left only. The points that answer to the excentricity of 15' and 20' are indeed so visibly out of the centre, that we may safely say, that any mistake, in estimating the time of a spot on Mars coming to the centre, cannot well exceed a quarter of an hour at the outside.

As for the 3d and last occasion of error, the time itself, he thinks may be depended on to a few seconds; but the observations of the year 1777, indeed, are far from having the same advantage. He was not then provided with an altitude instrument, therefore set his clock by a good sun-dial, with the equation of time contained in the Nautical Almanac, and found it to agree generally to a minute or 2 with the time calculated for the eclipses of Jupiter's first satellite, as he deduced it for Bath from the Nautical Almanac. However, it was certainly liable to an error of several minutes; therefore, allowing no less than 10<sup>m</sup> for the clock in 1777, and 20<sup>m</sup> for an error in estimating the situation of a spot in 1779, it will both amount to half an hour; then, if we take a mean of the 3 numbers, by which we have divided the 3 biennial periods, we have  $766\frac{1}{3}$ ; and half an hour divided by  $766\frac{1}{3}$ , will therefore give us the quantity to which, it seems, can amount, all the uncertainty in the syderal diurnal rotation of Mars, which is 2<sup>s</sup>.34.

*XI. Of the Termites in Africa and other Hot Climates. By Mr. Henry Smeathman, of Clement's Inn. p. 139.*

The size and figure of the buildings of these insects have attracted the notice of many travellers, and yet the world has not hitherto been furnished with a tolerable description of them, though their contrivance and execution scarcely fall short of human ingenuity and prudence; but when we come to consider the wonderful economy of these insects, with the good order of their subterraneous cities, they will appear foremost on the list of the wonders of the creation, as most closely imitating mankind in provident industry and regular government.

These insects are known by various names: they belong to the termes of Linneus, and other systematical naturalists: by the English, in the windward parts of Africa, they are called Bugga Bugs; in the West Indies, Wood Lice, Wood Ants, or White Ants. By the French, at Senegal, Vague-Vagues; in the West Indies, Poux de Bois, or Fourmis Blanches. By the Bolms, or Sherbro people,



in Africa, Scantz. By the Portuguese in the Brazils, Coupée or Cutters, from their cutting things in pieces. By this latter name and that of Piercers or Eaters, and similar terms, they are distinguished in various parts of the tropical regions.

The termites are represented by Linneus as the greatest plagues of both Indies, and are indeed every way between the tropics so deemed, from the vast damages they cause, and the losses which are experienced in consequence of their eating and perforating wooden buildings, utensils, and furniture, with all kinds of household-stuff and merchandize, which are totally destroyed by them, if not timely prevented; for nothing less hard than metal or stone can escape their destructive jaws.

These insects have generally obtained the name of ants, it may be presumed, from the similarity in their manner of living, which is, in large communities which erect very extraordinary nests, for the most part on the surface of the ground, whence their excursions are made through subterraneous passages or covered galleries, which they build whenever necessity obliges, or plunder induces, them to march above ground, and at a great distance from their habitations carry on a business of depredation and destruction, scarcely credible but to those who have seen it. But though they live in communities, and are like the ants omnivorous; though like them at a certain period they are furnished with 4 wings, and emigrate or colonize at the same season; they are by no means the same kind of insects, nor does their form correspond with that of ants in any one state of their existence, which, like most other insects, is changed several times.

The termites resemble the ants also in their provident and diligent labour, but surpass them as well as the bees, wasps, beavers, and all other animals, in the arts of building, as much as the Europeans excel the least cultivated savages. It is more than probable they excel them as much in sagacity and the arts of government; it is certain that they show more substantial instances of their ingenuity and industry than any other animals; and do in fact lay up vast magazines of provisions and other stores; a degree of prudence which has of late years been denied, perhaps without reason, to the ants. Such however are the extraordinary circumstances attending their economy and sagacity, that it is difficult to determine, whether they are more worthy of the attention of the curious and intelligent part of mankind on these accounts, or from the ruinous consequences of their depredations, which have deservedly procured them the name of Fatalis or Destructor.

These communities consist of one male and one female, who are generally the common parents of the whole, or greater part, of the rest, and of 3 orders of insects, apparently of very different species, but really the same, which together compose great commonwealths, or rather monarchies, if we may be allowed the



term. Linneus, having seen or heard of only 2 of these orders, has classed the genus erroneously: for he has placed it among the aptera, or insects without wings; whereas the chief order, that is, the insect in its perfect state, having 4 wings without any sting, it belongs to the neuroptera; in which class it will constitute a new genus of many species.

The different species of this genus resemble each other in form, in their manner of living, and in their good and bad qualities; but differ as much as birds in the manner of building their habitations or nests, and in the choice of the materials of which they compose them. There are some species which build on the surface of the ground, or part above and part beneath, and 1 or 2 species, perhaps more, that build on the stems or branches of trees, sometimes aloft at a vast height.

Of every species there are 3 orders: 1st, the working insects, which Mr. S. calls labourers; next the fighting ones, or soldiers, which do no kind of labour; and lastly, the winged ones, or perfect insects, which are male and female, and capable of propagation. These might very appositely be called the nobility or gentry, for they neither labour, nor toil, nor fight, being quite incapable of either, and almost of self-defence. These only are capable of being elected kings or queens; and nature has so ordered it, that they emigrate within a few weeks after they are elevated to this state, and either establish new kingdoms, or perish within a day or two.

The *termes bellicosus*, being the largest species, is most remarkable and best known on the coast of Africa. It erects immense buildings of well-tempered clay or earth, which are contrived and finished with such art and ingenuity, that we are at a loss to say, whether they are most to be admired on that account, or for their enormous magnitude and solidity. It is from the two lower orders of this, or a similar species, that Linneus seems to have taken his description of the *termes fatalis*; and most of the accounts brought home from Africa or Asia, of the white ants, are also taken from a species that are so much alike in external habit and size, and build so much in their manner, that one may almost venture to pronounce them mere variations of the same species. The reason that the larger termites have been most remarked, is obvious; they not only build larger and more curious nests, but are also more numerous, and do infinitely more mischief to mankind. When these insects attack such things as we would not wish to have injured, we must consider them as most pernicious; but when they are employed in destroying decayed trees and substances which only encumber the surface of the earth, they may be justly supposed very useful. In this respect they resemble very much the common flies, which are regarded by mankind in general as noxious, and at best as useless beings in the creation; but this is certainly for want of consideration. There are not probably in all nature animals



of more importance, and it would not be difficult to prove, that we should feel the want of one or two species of large quadrupeds, much less than of one or two species of these despicable looking insects. Mankind in general are sensible that nothing is more disagreeable, or more pestiferous, than putrid substances; and it is apparent to all who have made observation, that those little insects contribute more to the quick dissolution and dispersion of putrescent matter than any other. They are so necessary in all hot climates, that even in the open fields a dead animal or small putrid substance cannot be laid on the ground 2 minutes before it will be covered with flies and their maggots, which instantly entering quickly devour one part, and perforating the rest in various directions, expose the whole to be much sooner dissipated by the elements. Thus it is with the termites; the rapid vegetation in hot climates, of which no idea can be formed by any thing to be seen in this, is equalled by as great a degree of destruction from natural as well as accidental causes.\* It seems, that when any thing whatever is arrived at its last degree of perfection, the Creator has decreed it shall be totally destroyed as soon as possible, that the face of nature may be speedily adorned with fresh productions in the bloom of spring or the pride of summer: so when trees, and even woods, are in part destroyed by tornados or fire, it is wonderful to observe, how many agents are employed in hastening the total dissolution of the rest; but in the hot climates there are none so expert, or who do their business so expeditiously and effectually, as these insects, which in a few weeks destroy and carry away the bodies of large trees, without leaving a particle behind, thus clearing the place for other vegetables, which soon fill up every vacancy; and in places, where 2 or 3 years before there has been a populous town, if the inhabitants, as is frequently the case, have chosen to abandon it, there shall be a very thick wood, and not the vestige of a post to be seen, unless the wood has been of a species which, from its hardness, is called iron wood.

The nests of this species, the *termes bellicosus*, are so numerous all over the island of Bananas, and the adjacent continent of Africa, that it is scarcely possible to stand on any open place, such as a rice plantation, or other clear spot, where one of these buildings is not to be seen within 50 paces, and frequently 2 or 3 are to be seen almost close to each other. In some parts near Senegal, as mentioned by Mons. Adanson, their number, magnitude, and closeness of situation, make them appear like the villages of the natives. These buildings are usually termed hills, by natives as well as strangers, from their outward appearance, which is that of little hills more or less conical, generally pretty much in the form of sugar loaves, and about 10 or 12 feet in perpendicular height above

\* The Guinea grass, which is so well known and so much esteemed by our planters in the West Indies, grows in Africa 13 feet high on an average, which height it attains in about 5 or 6 months; and the growth of many other plants is as quick.—Orig.



the common surface of the ground. These hills continue quite bare till they are 6 or 8 feet high; but in time the dead barren clay, of which they are composed, becomes fertilized by the genial power of the elements in these prolific climates, and the addition of vegetable salts and other matters brought by the wind; and in the 2d or 3d year, the hillock, if not over-shaded by trees, becomes, like the rest of the earth, almost covered with grass and other plants; and in the dry season, when the herbage is burnt up by the rays of the sun, it is not much unlike a very large hay-cock.

Every one of these buildings consists of 2 distinct parts, the exterior and the interior. The exterior is one large shell in the manner of a dome, large and strong enough to inclose and shelter the interior from the vicissitudes of the weather, and the inhabitants from the attacks of natural or accidental enemies. It is always therefore much stronger than the interior building, which is the habitable part, divided, with a wonderful kind of regularity and contrivance, into an amazing number of apartments for the residence of the king and queen, and the nursing of their numerous progeny; or for magazines, which are always found well filled with stores and provisions.

These hills make their first appearance above ground by a little turret or two in the shape of sugar loaves, which are run a foot high or more. Soon after, at some little distance, while the former are increasing in height and size, they raise others, and so go on increasing the number and widening them at the base, till their works below are covered with these turrets, which they always raise the highest and largest in the middle, and, by filling up the intervals between each turret, collect them as it were into one dome. They are not very curious or exact about these turrets, except in making them very solid and strong; and when by the junction of them the dome is completed, for which purpose the turrets answer as scaffolds, they take away the middle ones entirely, except the tops, which joined together make the crown of the cupola, and apply the clay to the building of the works within, or to erecting fresh turrets for the purpose of raising the hillock still higher; so that doubtless some part of the clay is used several times, like the boards and posts of a mason's scaffold.

When these hills are little more than half their height, it is always the practice of the wild bulls to stand as centinels on them, while the rest of the herd is ruminating below. They are sufficiently strong for that purpose, and at their full height answer excellently well as places to look out. Mr. S. has been with 4 men on the top of one of these hillocks. Whenever word was brought of a vessel in sight, they immediately ran to some bugga bug hill, as they are called, and clambered up to get a good view; for on the common surface it was seldom possible to see over the grass or plants, which, in spite of monthly brushings, generally prevented all horizontal views at any distance.



The outward shell or dome is not only of use to protect and support the interior buildings from external violence and the heavy rains, but to collect and preserve a regular degree of genial warmth and moisture, which seems very necessary for hatching the eggs and cherishing the young ones. The royal chamber, which Mr. S. so calls on account of its being adapted for, and occupied by, the king and queen, appears to be in the opinion of this little people of the most consequence, being always situated as near the centre of the interior building as possible, and generally about the height of the common surface of the ground, at a pace or two from the hillock. It is always nearly in the shape of half an egg or an obtuse oval within, and may be supposed to represent a long oven. In the infant state of the colony, it is but about an inch in length; but in time will be increased to 6 or 8 inches or more in the clear, being always in proportion to the size of the queen, who, increasing in bulk as in age, at length requires a chamber of such dimensions. Its floor is perfectly horizontal; and in large hillocks, sometimes more than an inch thick of solid clay. The roof also, which is one solid and well-turned oval arch, is generally of about the same solidity, but in some places it is not a quarter of an inch thick, viz. on the sides where it joins the floor, and where the doors or entrances are made level with it at nearly equal distances from each other. These entrances will not admit any animal larger than the soldiers or labourers; so that the king, and the queen, who is, at full size, a thousand times the weight of a king, can never possibly go out.

The royal chamber, if in a large hillock, is surrounded by an innumerable quantity of others of different sizes, shapes, and dimensions; but all of them arched in one way or another, sometimes circular, and sometimes elliptical or oval. These either open into each other, or communicate by passages as wide; and, being always empty, are evidently made for the soldiers and attendants, of whom it will soon appear great numbers are necessary, and of course always in waiting. These apartments are joined by the magazines and nurseries. The former are chambers of clay, and are always well filled with provisions, which to the naked eye seem to consist of the raspings of wood and plants which the termites destroy, but are found in the microscope to be principally the gums or inspissated juices of plants. These are thrown together in little masses, some of which are finer than others, and resemble the sugar about preserved fruits, others are like tears of gum, one quite transparent, another like amber, a 3d brown, and a 4th quite opaque, as we see often in parcels of ordinary gums. These magazines are intermixed with the nurseries, which are buildings totally different from the rest of the apartments: for these are composed entirely of wooden materials, seemingly joined together with gums. Mr. S. calls them the nurseries because they are invariably occupied by the eggs, and young ones, which appear



at first in the shape of labourers, but white as snow. These buildings are exceedingly compact, and divided into many very small irregular-shaped chambers, not one of which is to be found of half an inch in width. They are placed all round the royal apartments, and as near as possible to them.

When the nest is in the infant state, the nurseries are close to the royal chamber; but as in process of time the queen enlarges, it is necessary to enlarge the chamber for her accommodation; and as she then lays a greater number of eggs, and requires a greater number of attendants, so it is necessary to enlarge and increase the number of the adjacent apartments; for which purpose the small nurseries which are first built are taken to pieces, rebuilt a little farther off a size larger, and the number of them increased at the same time. Thus they continually enlarge their apartments, pull down, repair, or rebuild, according to their wants, with a degree of sagacity, regularity, and foresight, not even imitated by any other kind of animals or insects yet heard of.

There is one remarkable circumstance attending the nurseries. They are always slightly overgrown with mould, and plentifully sprinkled with small white globules about the size of a small pin's head. These at first Mr. S. took to be the eggs; but, on bringing them to the microscope, they evidently appeared to be a species of mushroom, in shape like our eatable mushroom in the young state in which it is pickled. They appear, when whole, white like snow a little thawed and then frozen again, and when bruised seem composed of an infinite number of pellucid particles, approaching to oval forms and difficult to separate; the mouldiness seems likewise to be the same kind of substance. The nurseries are inclosed in chambers of clay, like those which contain the provisions, but much larger. In the early state of the nest they are not larger than a hazelnut, but in great hills are often as large as a child's head of a year old.

The disposition of the interior parts of these hills is pretty much alike, except when some insurmountable obstacle prevents; for instance, when the king and queen have been first lodged near the foot of a rock or of a tree, they are certainly built out of the usual form, otherwise pretty nearly according to the following plan. The royal chamber is situated at about a level with the surface of the ground, at an equal distance from all the sides of the building, and directly under the apex of the hill. It is on all sides, both above and below, surrounded by what Mr. S. calls the royal apartments, which have only labourers and soldiers in them, and can be intended for no other purpose than for these to wait in, either to guard or serve their common father and mother, on whose safety depends the happiness, and, according to the negroes, even the existence of the whole community.

These apartments compose an intricate labyrinth, which extends a foot or more in diameter from the royal chamber on every side. Here the nurseries and ma-



gazines of provisions begin, and, being separated by small empty chambers and galleries, which go round them or communicate from one to the other, are continued on all sides to the outer shell, and reach up within it  $\frac{2}{3}$  or  $\frac{3}{4}$  of its height, leaving an open area in the middle under the dome, which very much resembles the nave of an old cathedral: this is surrounded by 3 or 4 very large gothic-shaped arches, which are sometimes 2 or 3 feet high next the front of the area, but diminish very rapidly as they recede from it, like the arches of aisles in perspectives, and are soon lost among the innumerable chambers and nurseries behind them. All these chambers, and the passages leading to and from them, being arched, they help to support each other; and while the interior large arches prevent them falling into the centre, and keep the area open, the exterior building supports them on the outside. There are, comparatively speaking, few openings into the great area, and they for the most part seem intended only to admit that genial warmth into the nurseries which the dome collects. The interior building or assemblage of nurseries, chambers, &c. has a flattish top or roof, without any perforation, which would keep the apartments below dry, in case through accident the dome should receive any injury and let in water; and it is never exactly flat and uniform, because the insects are always adding to it by building more chambers and nurseries: so that the divisions or columns between the future arched apartments resemble the pinnacles on the fronts of some old buildings, and demand particular notice, as affording one proof that for the most part the insects project their arches, and do not make them by evacuation.

The area has also a flattish floor, which lies over the royal chamber, but sometimes a good height above it, having nurseries and magazines between. It is likewise water-proof, and contrived, as far as we may guess, to let the water off, if it should get in, and run over by some short way into the subterraneous passages, which run under the lowest apartments in the hill in various directions, and are of an astonishing size, being wider than the bore of a great cannon. One, that Mr. S. measured, was perfectly cylindrical, and 13 inches in diameter. These subterraneous passages or galleries are lined very thick with the same kind of clay of which the hill is composed, and ascend the inside of the outward shell in a spiral manner, and winding round the whole building up to the top, intersect each other at different heights, opening either immediately in the dome in various places, and into the interior building, the new turrets, &c. or communicating with them by other galleries of different bores or diameters, either circular or oval.

From every part of these large galleries are various small pipes or galleries leading to different parts of the building. Under ground there are a great many that lead downward by sloping descents, 3 and 4 feet perpendicular among the gravel, whence the labouring termites cull the finer parts, which, being worked



up in their mouths to the consistence of mortar, becomes that solid clay or stone of which their hills and all their buildings, except their nurseries, are composed. Other galleries again ascend, and lead out horizontally on every side, and are carried under ground near to the surface a vast distance: for if you destroy all the nests within 100 yards of your house, the inhabitants of those which are left unmolested farther off, will still carry on their subterraneous galleries, and invade the goods and merchandizes contained in it by sap and mine, and do great mischief, if you are not very circumspect.

But to return to the cities whence these extraordinary expeditions and operations originate: it seems there is a degree of necessity for the galleries under the hills being thus large, being the great thoroughfares for all the labourers and soldiers, going forth or returning on any business whatever, whether fetching clay, wood, water, or provisions; and they are certainly well calculated for the purposes to which they are applied, by the spiral slope which is given them; for if they were perpendicular, the labourers would not be able to carry on their building with so much facility, as they ascend a perpendicular with great difficulty, and the soldiers can scarcely do it at all. It is on this account that sometimes a road like a ledge is made on the perpendicular side of any part of the building within their hill, which is flat on the upper surface, and half an inch wide, and ascends gradually like a staircase, or like those roads which are cut on the sides of hills and mountains, that would otherwise be inaccessible: by which, and similar contrivances, they travel with great facility to every interior part.

This too is probably the cause of their building a kind of bridge of one vast arch, which answers the purpose of a flight of stairs from the floor of the area to some opening on the side of one of the columns which support the great arches, which must shorten the distance exceedingly to those labourers who have the eggs to carry from the royal chamber to some of the upper nurseries, which in some hills would be 4 or 5 feet in the straightest line, and much more if carried through all the winding passages which lead through the inner chambers and apartments. Mr. S. had a memorandum of one of these bridges, half an inch broad, a quarter of an inch thick, and 10 inches long, making the side of an elliptic arch of proportionable size; so that it is wonderful it did not fall over or break by its own weight before they got it joined to the side of the column above. It was strengthened by a small arch at the bottom, and had a hollow or groove all the length of the upper surface, either made purposely for the inhabitants to travel over with more safety, or else, which is not improbable, worn so by frequent treading.

The nests before described are so remarkable on account of their size, that travellers have seldom, where they were to be seen, taken notice of any other; and have generally, when speaking of white ants, described them as inhabitants



of those hills. Those however which are built by the smaller species of those insects, are very numerous, and some of them exceedingly well worth our attention; one sort in particular, which from their form Mr. S. has named turret nests. These are a great deal less than the foregoing, and indeed much less in proportion to the size of the buildings; but their external form is more curious, and, their solidity considered, they are prodigious buildings for so small an animal.\*

These buildings are upright cylinders composed of a well-tempered black earth or clay, about  $\frac{3}{4}$  of a yard high, and covered with a roof of the same material in the shape of a cone, whose base extends over and hangs down 3 or 4 inches wider than the perpendicular sides of the cylinder; so that most of them resemble in shape the body of a round windmill; but some of the roofs have so little elevation in the middle, that they are pretty much in the shape of a full-grown mushroom.

After one of these turrets is finished, it is not altered or enlarged; but when no longer capable of containing the community, the foundation of another is laid within a few inches of it. Sometimes, though but rarely, the 2d is begun before the first is finished, and a 3d before they have completed the 2d: thus they will run up 5 or 6 of these turrets at the foot of a tree in the thick woods, and make a most singular group of buildings. The turrets are so strongly built, that in case of violence they will much sooner overset from the foundation, and tear up the gravel and solid earth, than break in the middle; and in that case the insects will frequently begin another turret and build it, as it were, through that which is fallen; for they will connect the cylinder below with the ground, and run up a new turret from its upper side, so that it will seem to rest on the horizontal cylinder only.

Mr. S. did not observe any thing else about these nests that was remarkable, except the quantity of the black brown clay, which is as dark coloured as rich vegetable mould; but burns to an exceeding fine and clear red brick. Within, the whole building is pretty equally divided into innumerable cells of irregular shapes; sometimes they are quadrangular or cubical, and sometimes pentagonal; but often the angles are so ill defined, that each half of a cell will be shaped like the inside of that shell which is called the sea-ear. Each shell has two or more entrances, and as there are no pipes or galleries, no variety of apartments, no well-turned arches, wooden nurseries, &c. &c. they do not by any means excite our admiration so much as the hill nests, which are indeed collections of wonders. There are two sizes of these turret nests, built by two different species of

\* If their height be estimated and computed by the size of the builders, and compared with ours on the like scale; each of them is 4 or 5 times the height of the monument, and a great many times its solid contents.—Orig.



termites. The larger species, the *termes atrox*, in its perfect state measures 1 inch and  $\frac{3}{10}$  from the extremities of the wings on the one side to the extremities on the other. The lesser species, *termes mordax*, measures only  $\frac{8}{10}$  of an inch from tip to tip.

The next kind of nests, built by another species of this genus, the *termes arborum*, have very little resemblance to the former in shape or substance. These are generally spherical or oval, and built in trees. Sometimes they are seated between the arms and the stems of trees, and very frequently may be seen surrounding the branch of a tree at the height of 70 or 80 feet; and, though but rarely, as large as a very great sugar cask. They are composed of small particles of wood and the various gums and juices of trees, perhaps combined with those of the animals, and worked by those little industrious creatures into a paste, and so moulded into innumerable little cells of very different and irregular forms, which afford no amusing variety and nothing curious, but the immense quantity of inhabitants, young and old, with which they are at all times crowded; on which account they are sought for in order to feed young fowls, and especially for the treating of turkies. These nests are very compact, and so strongly attached to the boughs on which they are fixed, that there is no detaching them but by cutting them in pieces, or sawing off the branch: and they will sustain the force of a tornado as long as the tree on which they are fixed. This species has the external habit, size, and almost the colour, of the *termes atrox*.

Some nests are built in those sandy plains called, after the Spaniards, Savannas, that resemble the hill nests first described. They are composed of a black mud, brought from a few inches below the white sand, and are built in the form of an imperfect cone, or bell-shaped, having their tops rounded. These nests are generally about 4 or 5 feet high. They seemed to be inhabited by nearly as large insects, differing very little except in colour, which is lighter than that of the *termites bellicosus*.

It has been before observed, that there are of every species of termites 3 orders; of these orders the working insects or labourers are always the most numerous; in the *termes bellicosus* there seems to be at the least 100 labourers to one of the fighting insects or soldiers. They are in this state about  $\frac{1}{4}$  of an inch long, and 25 of them weigh about a grain; so that they are not so large as some of our ants. From their external habit and fondness for wood, they have been very expressively called wood-lice by some people, and the whole genus has been known by that name, particularly among the French. They resemble them, it is true, very much at a distance, but they run as fast or faster than any other insects of their size, and are incessantly bustling about their affairs. The 2d order, or soldiers, have a very different form from the labourers, and have been by some authors supposed to be the males, and the former neuters; but they are,



in fact, the same insects as the foregoing, only they have undergone a change of form, and approached one degree nearer to the perfect state. They are now much larger, being half an inch long, and equal in bulk to 15 of the labourers. There is now likewise a most remarkable circumstance in the form of the head and mouth; for in the former state the mouth is evidently calculated for gnawing and holding bodies; but in this state, the jaws being shaped just like 2 very sharp awls a little jagged, they are incapable of any thing but piercing or wounding, for which purposes they are very effectual, being as hard as a crab's claw, and placed in a strong horny head, which is of a nut-brown colour, which seems to labour under great difficulty in carrying it: on which account perhaps the animal is incapable of climbing up in perpendicular surfaces.

The 3d order, or the insect in its perfect state, varies its form still more than ever. The head, thorax, and abdomen, differ almost entirely from the same parts in the labourers and soldiers; and, besides, the animal is now furnished with 4 fine large brownish, transparent, wings, with which it is at the time of emigration to wing its way in search of a new settlement. In short, it differs so much from its form and appearance in the other 2 states, that it has never been supposed to be the same animal, but by those who have seen it in the same nest; and some of these have distrusted the evidence of their senses. It was so long before Mr. S. met with them in the nests himself, that he doubted the information which was given by the natives, that they belonged to the same family. Indeed 20 nests may be opened without finding one winged ant; for those are to be found only just before the commencement of the rainy season, when they undergo the last change, which is preparative to their colonization. Add to this, they sometimes abandon an outward part of their building, the community being diminished by some accident. Sometimes, too, different species of the real ant (*formica*) possess themselves by force of a lodgement, and so are frequently dislodged from the same nest, and taken for the same kind of insects. This is often the case with the nests of the smaller species, which are often totally abandoned by the termites, and completely inhabited by different species of ants, cockroaches, scolopendræ, scorpions, and other vermin, fond of obscure retreats, that occupy different parts of their roomy buildings.

In the winged state they have also much altered their size as well as form. Their bodies now measure between 6 and 7 tenths of an inch in length, their wings being above  $2\frac{1}{2}$  inches from tip to tip, and they are equal in bulk to about 30 labourers, or 2 soldiers. They are now also furnished with 2 large eyes, one on each side of the head, and very conspicuous; if they have any before, they are not easily to be distinguished. Probably in the 1st 2 states, their eyes, if they have any, may be small like those of moles; for as they live like these animals always under-ground, they have as little occasion for these organs, and



it is not to be wondered at that we do not discover them; but the case is much altered when they arrive at the winged state, in which they are to roam, though but for a few hours, through the wide air, and explore new and distant regions. In this form the animal comes abroad during or soon after the first tornado, which at the latter end of the dry season proclaims the approach of the ensuing rains, seldom waiting for a 2d or 3d shower, if the 1st, as is generally the case, happens in the night, and brings much wet after it.

The quantities that are to be found the next morning all over the surface of the earth, but particularly on the waters, is astonishing; for their wings are only calculated to carry them a few hours, and after the rising of the sun not one in 1000 is to be found with 4 wings, unless the morning continues rainy, when here and there a solitary being is seen winging its way from one place to another, as if solicitous only to avoid its numerous enemies, particularly various species of ants, which are hunting on every spray, on every leaf, and in every possible place, for this unhappy race, of which probably not a pair in many millions get into a place of safety, to fulfil the great law of nature, and lay the foundation of a new community. Not only all kinds of ants, birds, and carnivorous reptiles, as well as insects, are on the hunt for them, but the inhabitants of many countries, and particularly of that part of Africa where Mr. S. was, eat them.\* On the following morning however they are to be seen

\* Mr. Konig, in an Essay on these Insects, read before the Society of Naturalists of Berlin, says, that, in some parts of the East Indies, the queens are given alive to old men for strengthening the back, and that the natives have a method of catching the winged insects, which he calls females, before the time of emigration. They make two holes in the nest; the one to windward, and the other to leeward. At the leeward opening they place the mouth of a pot, previously rubbed within with an aromatic herb called bergera, which is more valued there than the laurel in Europe. On the windward side they make a fire of stinking materials, which not only drives these insects into the pots, but frequently the hooded snakes also, on which account they are obliged to be cautious in removing them. By this method they catch great quantities, of which they make with flour a variety of pastry, which they can afford to sell very cheap to the poorer ranks of people. Mr. Konig adds, that this kind of food is very plentiful; the too great use of it brings on an epidemic colic and dysentery, which kills in two or three hours.

I have not, says Mr. S., found the Africans so ingenious in procuring or dressing them. They are content with a very small part of those which, at the time of swarming, or rather of emigration, fall into the neighbouring waters, which they skim off with calabashes, bring large kettles full of them to their habitations, and parch them in iron pots over a gentle fire, stirring them about as is usually done in roasting coffee. In that state, without sauce or any other addition, they serve them as delicious food; and they put them by hands-full into their mouths, as we do comfits. I have eaten them dressed this way several times, and think them both delicate, nourishing, and wholesome; they are something sweeter, but not so fat and cloying, as the caterpillar or maggot of the palm-tree snout-beetle, *curculio palmarum*, which is served up at all the luxurious tables of West Indian epicures, particularly of the French, as the greatest dainty of the western world.

According to the Baron de Geer, Mr. Sparrman says, that the Hottentots eat these insects, and



running on the ground in chace of each other ; sometimes with a wing or 2 still hanging to their bodies, which are not only useless, but seem rather cumbersome. The greater part have no wings, but they run exceedingly fast, the males after the females ; Mr. S. sometimes remarked 2 males after one female, contending with great eagerness who should win the prize, regardless of the innumerable dangers that surrounded them.

They are now become, from being one of the most active, industrious, and rapacious, from one of the most fierce and implacable little animals in the world, the most innocent, helpless, and cowardly ; never making the least resistance to the smallest ant. The ants are to be seen on every side in infinite numbers, of various species and sizes, dragging these annual victims of the laws of nature to their different nests. It is wonderful that a pair should ever escape so many dangers, and get into a place of security. Some however are so fortunate ; and being found by some of the labouring insects that are continually running about the surface of the ground under their covered galleries, are elected kings and queens of new states ; all those who are not so elected and preserved, certainly perish, and most probably in the course of the following day. The manner in which these labourers protect the happy pair from their innumerable enemies, not only on the day of the massacre of almost all their race, but for a long time after, will, Mr. S. hopes, justify him in the use of the term election. The little industrious creatures immediately inclose them in a small chamber of clay suitable to their size, into which at first they leave but one small entrance, large enough for themselves and the soldiers to go in and out, but much too little for either of the royal pair to make use of ; and when necessity obliges them to make more entrances, they are never larger ; so that, of course, the voluntary subjects charge themselves with the task of providing for the offspring of their sovereigns, as well as to work and to fight for them, till they shall have raised a progeny capable at least of dividing the task with them.

About this time a most extraordinary change begins to take place in the queen, to which Mr. S. knows nothing similar, except in the *pulex penetrans* of Linneus, the jigger of the West Indies, and in the different species of *coccus*, *cöchineal*. The abdomen of this female begins gradually to distend and enlarge

even grow fat on them ; but he does not say what methods they take to procure or dress them. And other writers mention their being an article of diet in different parts of South America.

Sir Hans Sloane says, the silk-cotton tree worm is esteemed by the Indians and negroes beyond marrow. This worm is no more than a large maggot, being the caterpillar of a large *Capricorn* beetle, or goat chafer: the larva of a pretty large *cerambix*, which is also brought from Africa, where I have eaten those worms roasted. This insect is most probably to be found in all countries where the silk-cotton tree (*bombax*) is indigenous. I have discoursed with several gentlemen on the taste of the white ants ; and we have always agreed, that they are most delicious. One gentleman compared them to sugared marrow, another to sugared cream and a paste of sweet almonds.—Orig.



to such an enormous size, that an old queen will have it increased so as to be 1500 or 2000 times the bulk of the rest of her body, and 20 or 30 thousand times the bulk of a labourer, as Mr. S. has found by carefully weighing and computing the different states. The skin between the segments of the abdomen distends in every direction; and at last the segments are removed to half an inch distance from each other, though at first the length of the whole abdomen is not half an inch. They preserve their dark brown colour, and the upper part of the abdomen is marked with a regular series of brown bars, from the thorax to the posterior part of the abdomen, while the intervals between them are covered with a thin, delicate, transparent skin, and appear of a fine cream colour, a little shaded by the dark colour of the intestines and watery fluid, seen here and there beneath. Mr. S. conjectures the animal is upward of 2 years old when the abdomen is increased to 3 inches in length; he had sometimes found them of near twice that size. The abdomen is now of an irregular oblong shape, being contracted by the muscles of every segment, and is become one vast matrix full of eggs, which make long circumvolutions through an innumerable quantity of very minute vessels, that circulate round the inside in a serpentine manner, which would exercise the ingenuity of a skilful anatomist to dissect and develope. This singular matrix is not more remarkable for its amazing extension and size, than for its peristaltic motion, which resembles the undulating of waves, and continues incessantly without any apparent effort of the animal; so that one part or other alternately is rising and sinking in perpetual succession, and the matrix seems never at rest, but is always protruding eggs to the amount, as he had frequently counted in old queens, of 60 in a minute, or 80 thousand and upward in 1 day of 24 hours.\*

These eggs are instantly taken from her body by her attendants, of whom there always are, in the royal chamber and the galleries adjacent, a sufficient number in waiting, and carried to the nurseries, which in a great nest may some of them be 4 or 5 feet distant in a straight line, and consequently much farther by their winding galleries. Here, after they are hatched, the young are attended and provided with every thing necessary, till they are able to shift for themselves, and take their share in the labours of the community. This then is an accurate description and account of the *termes bellicosus*, or species that builds the large nests in its different states.

\* Since the reading of this paper, Mr. John Hunter, so celebrated for his great skill and experience in comparative anatomy, has dissected 2 young queens. He finds the abdomen contains 2 ovaria, in each of which are many hundred ova-ducts, and in each of these ova-ducts a vast many eggs; so that there seems no doubt of the fact, as the matrix of a full-grown queen must be calculated for the production and yielding of a prodigious number of eggs. He has also dissected the kings; the result of these dissections, with some further particulars, will be related in another paper.



Those which build either the roofed turrets or the nests in the trees, seem in most instances to have a strong resemblance to them, both in their form and economy, going through the same changes from the egg to the winged state. The queens also increase to a great size when compared with the labourers; but very short of those queens before described. The largest are from about an inch to an inch and a half long, and not much thicker than a common quill. There is the same kind of peristaltic motion in the abdomen, but in a much smaller degree; and, as the animal is incapable of moving from her place, the eggs are doubtless carried to the different cells by the labourers, and reared with a care similar to that which is practised in the larger nests.

It is remarkable of all these different species, that the working and the fighting insects never expose themselves to the open air; but either travel under ground, or within such trees and substances as they destroy; except indeed when they cannot proceed by their latent passages, and find it convenient or necessary to search for plunder above ground. In that case they make pipes of that material with which they build their nests. The larger sort use the red clay; the turret builders use the black clay; and those which build in the trees employ the same ligneous substances of which their nests are composed. With the materials they completely line most of the roads leading from their nests into the various parts of the country, and travel out and home with the utmost security in all kinds of weather. If they meet a rock or any other obstruction, they will make their way over the surface; and for that purpose erect a covered way or arch, still of the same materials, continuing it with many windings and ramifications through large groves; having, where it is possible, subterranean pipes running parallel with them, into which they sink and save themselves, if their galleries above ground be destroyed by any violence, or the tread of men or animals alarms them. When we chance by accident to enter any solitary grove, where the ground is pretty well covered with their arched galleries, they give the alarm by loud hissings, which are heard distinctly at every step we take; soon after which we may examine their galleries in vain for the insects; we find only small holes, just large enough for them, by which they have made their escape into their subterraneous roads. These galleries are large enough for them to pass and repass so as to prevent any stoppages, though there are always numerous passengers, and shelter them equally from light and air, as well as from their enemies, of which the ants, being the most numerous, are the most formidable.

The termites, except their heads, are exceeding soft, and covered with a very thin and delicate skin; being blind, they are no match on open ground for the ants, who can see, and are all of them covered with a strong horny shell not easily pierced, and are of dispositions bold, active, and rapacious. Whenever the termites are dislodged from their covered ways, the various species of the



former, who probably are as numerous above ground as the latter are in their subterraneous passages, instantly seize and drag them away to their nests, to feed the young brood. The termites are therefore exceeding solicitous to preserve their covered ways in good repair; and if one of them be demolished, for a few inches in length, it is wonderful how soon they rebuild it. At first in their hurry they get into the open part an inch or two, but stop so suddenly that it is very evident they are surprized; for though some run straight on, and get under the arch as speedily as possible in the farther part, most of them run as fast back, and very few will venture through that part of the track which is left uncovered. In a few minutes they are seen rebuilding the arch, and by the next morning they will have restored their gallery for 3 or 4 yards in length, if so much has been ruined; and, on opening it again, will be found as numerous as ever, under it, passing both ways. If you continue to destroy it several times, they will at length seem to give up the point, and build another in a different direction; but, if the old one led to some favourite plunder, in a few days they will rebuild it again; and, unless you destroy their nest, never totally abandon their gallery.

The termites arborum, those which build in trees, frequently establish their nests within the roofs and other parts of houses, to which they do considerable damage, if not timely extirpated. The large species are not only much more destructive, but more difficult to be guarded against, since they make their approaches chiefly under ground, descending below the foundations of houses and stores at several feet from the surface, and rising again either in the floors, or entering at the bottoms of the posts, of which the sides of the buildings are composed, and bore quite through them, following the course of the fibres to the top, or making lateral perforations and cavities here and there as they proceed. While some are employed in gutting the posts, others ascend from them, entering a rafter or some other part of the roof. If they once find the thatch, which seems to be a favourite food, they soon bring up wet clay, and build their pipes or galleries through the roof in various directions, as long as it will support them; sometimes eating the palm-tree leaves and branches of which it is composed, and perhaps (for variety seems very pleasing to them) the rattan or other running plant which is used as a cord to tie the various parts of the roof together, and that to the posts which support it: thus, with the assistance of the rats, who during the rainy season are apt to shelter themselves there, and to burrow through it, they very soon ruin the house, by weakening the fastenings and exposing it to the wet. In the mean time the posts will be perforated in every direction as full of holes as that timber in the bottoms of ships which has been bored by the worms; the fibrous and knotty parts, which are the hardest, being left to the last.



In carrying on this business, they sometimes find, it seems, that the post has some weight to support; and then, if it is a convenient track to the roof, or is itself a kind of wood agreeable to them, they bring their mortar, and fill all or most of the cavities, leaving the necessary roads through it, and, as fast as they take away the wood, replace the vacancy with that material; which being worked together by them closer and more compactly than human strength or art could ram it, when the house is pulled to pieces, in order to examine if any of the posts are fit to be used again, those of the softer kinds are often found reduced almost to a shell, and all or a greater part transformed from wood to clay, as solid and as hard as many kinds of free-stone used for building in England. It is much the same when the termites *bellicosus* get into a chest or trunk containing clothes and other things; if the weight above is great, or they are afraid of ants or other enemies, and have time, they carry their pipes through, and replace a great part with clay,\* running their galleries in various directions. The tree termites indeed, when they get within a box, often make a nest there, and being once in possession destroy it at their leisure. They did so to the pyramidal box which contained Mr. S.'s compound microscope. It was of mahogany, and he had left it in the store of Governor Campbell, of Tobago, for a few months, while he made the tour of the Leeward Islands. On his return he found these insects had done much mischief in the store, and, among other things, had taken possession of the microscope, and eaten every thing about it except the glass or metal, and the board on which the pedestal is fixed, with the drawers under it, and the things inclosed. The cells were built all round the pedestal and the tube, and attached to it on every side. All the glasses which were covered with the wooden substance of their nests retained a cloud, of a gummy nature, on them, that was not easily got off, and the lacquer or burnish with which the brass work was covered was totally spoiled. Another party had taken a liking to the staves of a Madeira cask, and had let out almost a pipe of fine old wine. If the large species of Africa (the termites *bellicosus*) had been so long in the uninterrupted possession of such a store, they would not have left 20 pounds weight of wood remaining of the whole building, and all that it contained.\*

\* Captain Phillip of the navy, who was some time at the Brazils in the service of Portugal, gave Mr. S. the following relation. "An engineer, returned from surveying the country, left his trunk on a table: the next morning, not only all his clothes were destroyed by White Ants or Cutters, but his papers also; and the latter in such a manner, that there was not a bit left of an inch square. The black lead pencils were likewise so completely destroyed, that the smallest piece, even of the black lead, could not be found. The clothes were not entirely cut to pieces and carried away, but appeared as if moth-eaten, there being scarce a piece as large as a shilling that was free from small holes; and it was further remarkable, that some silver coin, which was in the trunk, had a number of black specks on it, caused by something so corrosive that they could not easily be rubbed off even with sand."—Orig.



These insects are not less expeditious in destroying the shelves, wainscotting, and other fixtures of a house, than the house itself. They are for ever piercing and boring in all directions, and sometimes go out of the broadside of one post into that of another joining to it; but they prefer and always destroy the softer substances the first, and are particularly fond of pine and fir boards, which they excavate and carry away with wonderful dispatch and astonishing cunning: for, except a shelf has something standing on it, as a book, or any thing else which may tempt them, they will not perforate the surface, but artfully preserve it quite whole, and eat away all the inside, except a few fibres which barely keep the two sides connected together; so that a piece of an inch board, which appears solid to the eye, will not weigh much more than two sheets of pasteboard of equal dimensions, after these animals have been a little while in possession of it.\* In short, the termites are so insidious in their attacks, that we cannot be too much on our guard against them: they will sometimes begin and raise their works, especially in new houses, through the floor. If you destroy the work so begun, and make a fire on the spot, the next night they will attempt to rise through another part; and, if they happen to emerge under a chest or trunk early in the night, will pierce the bottom, and destroy or spoil every thing in it before the morning. On these accounts we are careful to set all our chests and boxes on stones or bricks, so as to leave the bottoms of such furniture some inches above the ground; which not only prevents these insects finding them out so readily, but preserves the bottoms from a corrosive damp which would strike from the earth through, and rot every thing in them: a vast deal of vermin also would harbour under, such as cock-roaches, centipedes, millepedes, scorpions, ants, and various other noisome insects.

When the termites attack trees and branches in the open air, they sometimes vary their manner of doing it. If a stake in a hedge has not taken root and vegetated, it becomes their business to destroy it. If it has a good sound bark round it, they will enter at the bottom, and eat all but the bark, which will remain, and exhibit the appearance of a solid stick, which some vagrant colony of ants or other insects often shelter in till the winds disperse it; but if they cannot trust the bark, they cover the whole stick with their mortar, and it then looks as if it had been dipped into thick mud that had been dried on. Under this covering they work, leaving no more of the stick and bark than is barely sufficient to support it, and frequently not the smallest particle, so that on a very small tap with your walking stick, the whole stake, though apparently as thick as your arm, and 5 or 6 feet long, loses its form, and disappearing like a shadow,

\* "The white ants are transparent as glass, and bite so forcibly, that in the space of one night alone they can eat their way through a thick wooden chest of goods, and make it as full of holes, as if it had been shot through with hail-shot." *Bosman's Guinea*, p. 276, 277, 493.—Orig.



falls in small fragments at your feet. They generally enter the body of a large tree which has fallen through age, or been thrown down by violence, on the side next the ground, and eat away at their leisure within the bark, without giving themselves the trouble either to cover it on the outside, or to replace the wood which they have removed from within, being somehow sensible that there is no necessity for it. These excavated trees deceived Mr. S. some times in running: for, attempting to step 2 or 3 feet high, he might as well have attempted to step upon a cloud, and has come down with such unexpected violence, that, besides shaking his teeth and bones almost to dislocation, he has been precipitated, head foremost, among the neighbouring trees and bushes. Sometimes, though seldom, the animals are known to attack living trees; though probably not before symptoms of mortification have appeared at the roots, since it is evident that these insects are intended in the order of nature to hasten the dissolution of such trees and vegetables as have arrived at their greatest maturity and perfection, and which would, by a tedious decay, serve only to encumber the face of the earth. This purpose they answer so effectually, that nothing perishable escapes them, and it is almost impossible to leave any thing penetrable on the ground a long time in safety; for the odds are, that put it where you will abroad, they will find it out before the following morning, and its destruction follows very soon of course. In consequence of this disposition, the woods never remain long encumbered with the fallen trunks of trees or their branches; and thus it is that the total destruction of deserted towns is so effectually completed, that in 2 or 3 years a thick wood fills the space; and, unless iron-wood posts have been made use of, not the least vestige of a house is to be discovered.

The first object of admiration which strikes one, on opening their hills, is the behaviour of the soldiers. If you make a breach in a slight part of the building, and do it quickly with a strong hoe or pick-axe, in the space of a few seconds a soldier will run out, and walk about the breach, as if to try whether the enemy is gone, or to examine what is the cause of the attack. He will sometimes go in again, as if to give the alarm: but most frequently, in a short time, is followed by 2 or 3 others, who run as fast as they can, straggling after each other, and are soon followed by a large body, who rush out as fast as the breach will permit them, and so they proceed, the number increasing, as long as any person continues battering their building.\* It is not easy to describe the rage and fury

\* "They throw up little hills of 7 or 8 feet high, so very full of holes that they rather seem like honey-combs than burrows. These ant-hills are of a very small circumference in proportion to their height, being sharp at top, so that to judge by the looks of them one would think the wind could blow them down. I one day attempted to knock off the top of one of them with my cane, but the stroke had no other effect than to bring some thousands of the animals out of doors, to learn what was the matter: on which I took to my heels and ran away as fast as I could." *Smith's Voyage to Guinea.*—Orig.



they show. In their hurry they frequently miss their hold, and tumble down the sides of the hill, but recover themselves as quickly as possible; and, being blind, bite every thing they run against, and thus make a crackling noise, while some of them beat repeatedly with their forceps on the building, and make a small vibrating noise, something shriller and quicker than the ticking of a watch: Mr. S. could distinguish this noise at 3 or 4 feet distance, and it continued for a minute at a time, with short intervals. While the attack proceeds they are in the most violent bustle and agitation. If they get hold of any one, they will in an instant let out blood enough to weigh against their whole body; and if it be the leg they wound, you will see the stain on the stocking extend an inch in width. They make their hooked jaws meet at the first stroke, and never quit their hold, but suffer themselves to be pulled away leg by leg, and piece after piece, without the least attempt to escape. On the other hand, keep out of their way, and give them no interruption, and they will in less than half an hour retire into the nest, as if they supposed the wonderful monster that damaged their castle was gone beyond their reach. Before they are all got in you will see the labourers in motion, and hastening in various directions towards the breach: every one with a burthen of mortar in his mouth ready tempered. This they stick upon the breach as fast as they come up, and do it with so much dispatch and facility, that though there are thousands, and even millions of them, they never stop or embarrass each other; and you are most agreeably deceived when, after an apparent scene of hurry and confusion, a regular wall arises, gradually filling up the chasm. While they are thus employed, almost all the soldiers are retired quite out of sight, except here and there one, who saunters about among 6 hundred or a thousand of the labourers, but never touches the mortar either to lift or carry it; one, in particular, places himself close to the wall they are building. This soldier will turn himself leisurely on all sides, and every now and then, at intervals of a minute or two, lift up his head, and with his forceps beat upon the building, and make the vibrating noise before-mentioned; on which immediately a loud hiss, which appears to come from all the labourers, issues from within side the dome and all the subterraneous caverns and passages: that it does come from the labourers is very evident, for you will see them all hasten at every such signal, redouble their pace, and work as fast again.

As the most interesting experiments become dull by repetition or continuance, so the uniformity with which this business is carried on, though so very wonderful, at last satiates the mind. A renewal of the attack however instantly changes the scene, and gratifies our curiosity still more. At every stroke we hear a loud hiss; and on the first the labourers run into the many pipes and galleries with which the building is perforated, which they do so quickly that they seem to vanish, for in a few seconds all are gone, and the soldiers rush out



as numerous and as vindictive as before.\* On finding no enemy, they return again leisurely into the hill, and very soon after, the labourers appear loaded as at first, as active and as sedulous, with soldiers here and there among them, who act just in the same manner, one or other of them giving the signal to hasten the business. Thus the pleasure of seeing them come out to fight or to work alternately may be obtained as often as curiosity excites or time permits: and it will certainly be found, that the one order never attempts to fight, or the other to work, let the emergency be ever so great.

We meet great obstacles in examining the interior parts of these tumuli. In the first place, the works, for instance the apartments which surround the royal chamber and the nurseries, and indeed the whole internal fabric, are moist, and consequently the clay is very brittle: they have also so close a connexion, that they can only be seen as it were by piece-meal; for having a kind of geometrical dependance or abutment against each other, the breaking of one arch pulls down 2 or 3. To these obstacles must be added the obstinacy of the soldiers, who fight to the very last, disputing every inch of ground so well as often to drive away the negroes who are without shoes, and make white people bleed plentifully through their stockings. Neither can we let a building stand so as to get a view of the interior parts without interruption; for while the soldiers are defending the out-works, the labourers keep barricadoing all the way against us, stopping up the different galleries and passages which lead to the various apartments, particularly the royal chamber, all the entrances to which they fill up so artfully, as not to let it be distinguishable while it remains moist; and externally it has no other appearance than that of a shapeless lump of clay. It is however easily found, from its situation with respect to the other parts of the building, and by the crouds of labourers and soldiers which surround it, who show their loyalty and fidelity by dying under its walls. The royal chamber in a large nest is capacious enough to hold many hundreds of the attendants, besides the royal pair, and it is always found as full of them as it can hold. These faithful subjects never abandon their charge, even in the last distress; for whenever Mr. S. took out the royal chamber, and, as he often did, preserved it for some time in a large glass bowl, all the attendants continued running in one direction round the king and queen with the utmost solicitude, some of them stopping on every circuit at the head of the latter, as if to give her something. When they came to the extremity of the abdomen, they took the eggs from her, and carried them away, and piled them carefully together in some part of the chamber, or in the

\* By the soldiers being so ready to run out, on the repetition of the attack, it appears that they but just withdraw out of sight, to leave room for the labourers to proceed without interruption in repairing the breach. The sudden retreat of the labourers, in case of an alarm, is also a wonderful instance of good order and discipline.—Orig.



bowl under, or behind any pieces of broken clay which lay most convenient for the purpose.

Some of these little unhappy creatures would ramble from the chamber, as if to explore the cause of such a horrid ruin and catastrophe to their immense building, as it must appear to them; and, after fruitless endeavours to get over the side of the bowl, return and mix with the croud, that continue running round their common-parents to the last. Others, placing themselves along her side, get hold of the queen's vast matrix with their jaws, and pull with all their strength, so as visibly to lift up the part which they fix at; but, as Mr. S. never saw any effect from these attempts, he never could determine whether this pulling was with an intention to remove her body, or to stimulate her to move herself, or for any other purpose; but, after many ineffectual tugs, they would desist, and join in the croud running round, or assist some of those who are cutting off clay from the external parts of the chamber or some of the fragments, and moistening it with the juices of their bodies, to begin to work a thin arched shell over the body of the queen, as if to exclude the air, or to hide her from the observation of some enemy. These, if not interrupted, before the next morning, completely cover her, leaving room enough within for great numbers to run about her. Mr. S. does not mention the king in this case, because he is very small in proportion to the queen, not being larger than 30 of the labourers, so that he generally conceals himself under one side of the abdomen, except when he goes up to the queen's head, which he does now and then, but not so frequently as the rest.

If in your attack on the hill you stop short of the royal chamber, and cut down about half of the building, and leave open some thousands of galleries and chambers, they will all be shut up with thin sheets of clay before the next morning. If even the whole is pulled down, and the different buildings are thrown in a confused heap of ruins, provided the king and queen are not destroyed or taken away, every interstice between the ruins, at which either cold or wet can possibly enter, will be so covered as to exclude both, and, if the animals are left undisturbed, in about a year they will raise the building to near its pristine size and grandeur.

The marching termites are not less curious in their order, than those described before. This species seems much scarcer and larger than the *termes bellicosus*. Mr. S. could get no information relative to them from the black people, from which he conjectures they are little known to them: his seeing them was very accidental. One day having made an excursion with his gun up the river Camerankoes, on his return through the thick forest, while he was sauntering very silently in hopes of finding some sport, on a sudden he heard a loud hiss, which, on account of the many serpents in those countries, is a most alarming sound.



The next step caused a repetition of the noise, which he soon recognized, and was rather surprised at not seeing any covered ways or hills. The noise however led him a few paces from the path, where, to his great astonishment, he saw an army of termites coming out of a hole in the ground, which could not be above 4 or 5 inches wide. They came out in vast numbers, moving forward as fast seemingly as it was possible for them to march. In less than a yard from this place they divided into 2 streams or columns, composed chiefly of the first order, which he calls labourers, 12 or 15 a-breast, and crowded as close after each other as sheep in a drove, going straight forward without deviating to the right or left. Among these, here and there, one of the soldiers was to be seen, trudging along with them, in the same manner, neither stopping nor turning; and as he carried his enormous large head with apparent difficulty, he put Mr. S. in mind of a very large ox among a flock of sheep. While these were bustling along, a great many soldiers were to be seen spread about on both sides of the 2 lines of march, some a foot or two distant, standing still or sauntering about, as if on the look out lest some enemy should suddenly come on the labourers. But the most extraordinary part of this march was the conduct of some others of the soldiers, who having mounted the plants which grow thinly here and there in the thick shade, had placed themselves on the points of the leaves, which were elevated 10 or 15 inches above the ground, and hung over the army marching below. Every now and then one or other of them beat with his forceps on the leaf, and made the same sort of ticking noise so frequently observed to be made by the soldier who acts the part of a surveyor or super-intendant, when the labourers are at work repairing a breach made in one of the common hills of the termites bellicosi. This signal among the marching white ants produced a similar effect; for, whenever it was made, the whole army returned a hiss, and obeyed the signal by increasing their pace with the utmost hurry. The soldiers who had mounted aloft, and gave these signals, sat quite still during the intervals, except making now and then a slight turn of the head, and seemed as solicitous to keep their posts as regular centinels. The 2 columns of the army joined into one, about 12 or 15 paces from their separation, having in no part been above 3 yards asunder, and then descended into the earth by 2 or 3 holes. They continued marching by Mr. S. for above an hour that he stood admiring them, and seemed neither to increase nor diminish their numbers, the soldiers only excepted, who quitted the line of march, and placed themselves at different distances on each side of the 2 columns; for they appeared much more numerous before he quitted the spot. Not expecting to see any change in their march, and being pinched for time, the tide being nearly up, and his departure fixed at high water, he quitted the scene with some regret, as the observation of a day or two might have afforded him the opportunity of exploring the reason and necessity of their



marching with such expedition, as well as of discovering their chief settlement, which is probably built in the same manner as the large hills before described. If so, it may be larger and more curious, as these insects were at least one-third larger than the other species, and consequently their buildings must be more wonderful, if possible: thus much is certain, there must be some fixed place for their king and queen, and the young ones.

The economy of nature is wonderfully displayed in a comparative observation on the different species who are calculated to live under ground until they have wings, and this species which marches in great bodies in open day. The former, in the first 2 states, that is, of labourers and soldiers, have no eyes that Mr. S. could ever discover; but when they arrive at the winged or perfect state in which they are to appear abroad, though only for a few hours, and that chiefly in the night, they are furnished with 2 conspicuous and fine eyes: so the termes viarum, or marching bugga bugs, being intended to walk in the open air and light, are even in the first state furnished with eyes proportionably as fine as those which are given to the winged or perfect insects of the other species.

*Explanation of the Figures to Mr. Smeathman's Account of the Termites of Africa, &c.*

Pl. 1, fig. 8, the hill-nest raised by the termites bellicosus; aaa, turrets by which their hills are raised and enlarged.

Fig. 9, a section of fig. 8, as it would appear on being cut down through the middle from the top a foot lower than the surface of the ground; AA, a horizontal line from A on the left, and a perpendicular line from A at the bottom, will intersect each other at the royal chamber; the darker shades near it are the empty apartments and passages, which it seems are left so for the attendants on the king and queen, who, when old, may require near 100,000 to attend them every day; the parts which are the least shaded and dotted are the nurseries, surrounded, like the royal chamber, by empty passages on all sides for the more easy access to them with the eggs from the queen, the provision for the young, &c. The magazines of provisions are situated without any seeming order among the vacant passages which surround the nurseries; B, the top of the interior building, which often seems, from the arches carrying upward, to be adorned on the sides with pinnacles; c, the floor of the area or nave; DDD, the large galleries which ascend from under all the buildings spirally to the top; E, a bridge.

Fig. 10, the first appearance of a hill-nest by two turrets.

Fig. 11, a tree, with the nest of the termites arborum, and their covered way; FFFF, covered ways of the termites arborum.

Fig. 12, a section of the nest of the termites arborum.

Fig. 13, a nest of the termites bellicosus, with Europeans on it, seemingly observing a vessel at sea.

Fig. 14, a bull standing centinel on one of these nests, while the rest of the herd are ruminating below; GGG, the African palm-trees, from the nuts of which is made the oleum palmæ.

Fig. 15, a transverse section of a royal chamber; aa, the thin sides in which the entrances are made.

Fig. 16, a longitudinal section of a royal chamber; b, the entrances; A, the door shut up, as left by the labourers.

Fig. 17, a royal chamber fore-shortened.

Fig. 18, the same royal chamber represented as just opened, and discovering B, the queen, and her attendants running round her; bb, a line drawn from b to b will run along the range of doors or entrances; AAA, a line run from A to AA will cross the door, which remains closed as it was found.



The rest are represented as they appear since the mortar, with which they were stopped up, has been in part, or wholly picked out with a small instrument.

Fig. 19, a nursery. Fig. 20, a little nursery, with the eggs, the young ones, the mushrooms, mouldiness, &c. as just taken from the hill.

Fig. 21, the mushrooms magnified by a lens.

Pl. 2, fig. 1 and 2, the turret nests, with roofs of the termes mordax and a termes atrox as finished.

Fig. 3, a turret, with the roof begun. Fig. 4, a turret, raised only about half its height. Fig. 5, a turret, building on one which had been thrown down. Fig. 6, 6, a turret broken in two.

Fig. 7, a termes bellicosus. Fig. 8, a king. Fig. 9, a queen. Fig. 10, the head of a perfect insect magnified. Fig. 11, a face, with stemmata magnified. Fig. 12, a labourer. Fig. 13, a labourer magnified. Fig. 14, a soldier. Fig. 15, a soldier's forceps and part of his head magnified. Fig. 16, the termes mordax. Fig. 17, the face with the stemmata magnified. Fig. 18, a labourer. Fig. 19, a soldier. Fig. 20, the termes atrox. Fig. 21, the face and stemmata magnified. Fig. 22, a labourer. Fig. 23, a soldier. Fig. 24, idem. Fig. 25, the termes destructor. Fig. 26, the face and stemmata magnified. Fig. 27, the termes arborum. Fig. 28, the face and stemmata magnified. Fig. 29, a labourer. Fig. 30, a soldier. Fig. 31, a queen.

N. B. In the figures 11, 17, 21, 26, and 28, the two white spots between the edges are the stemmata.

*XII. An Account of several Earthquakes felt in Wales. By T. Pennant, Esq., F. R. S. p. 193.*

On Dec. 8, between 4 and 5 in the evening, we were alarmed with 2 shocks of an earthquake; a slight one, immediately followed by another very violent. It seemed to come from the north-east, and was preceded by the usual noise. Mr. P. could not trace it farther than Holywell. The earthquake preceding this was on the 29th of August last, about a quarter before 9 in the morning. Mr. P. was aware of it by a rumbling noise, not unlike the coming of a great waggon into the court-yard. Two shocks immediately followed, which were strong enough to terrify the people. They came from the north-east; were felt in Anglesea, at Caernarvon, Llanrwst, in the isle of Clwyd south of Denbigh, at his house, and in Holywell.

The next, in this retrograde way of enumerating these phenomena, was on Sept. 8, 1775, about a quarter before 10 at night: the noise was such as preceded the former, and the shock so violent as to shake the bottles and glasses on the table round which Mr. P. and some company were sitting. This seemed to come from the east. In the Gentleman's Magazine of that year, this shock, it was said, extended to Shropshire, and quite to Bath, and to Swansea in South Wales. The earliest earthquake Mr. P. remembered here was on the 10th of April 1750. It is recorded in the Philos. Trans.

Mr. P. resided near a mineral country, in a situation between lead mines and coal mines; in a sort of neutral tract, about a mile distant from the first, and half a mile from the last. On the strictest inquiry he could not discover that the miners or colliers were ever sensible of the shocks under ground; nor have

they ever perceived, when the shocks in question have happened, any falls of the loose and shattery strata, in which the last especially work; yet the earthquakes have had violence sufficient to terrify the inhabitants of the surface. Neither were these local; for, excepting the first, all may be traced to very remote parts. The weather was remarkably still at the time of every earthquake Mr. P. had felt.

*XIII. On the Roots of Equations, in an Extract of a Letter from the Rt. Hon. Philip Earl Stanhope, F. R. S., to Mr. Jas. Clow, Prof. of Philos., Glasgow. Dated Chevening, Feb. 16, 1777. p. 195.*

I have lately made some curious observations concerning the roots of adfected equations, part of which have occurred to Messieurs Daniel Bernoulli, Euler, De La Grange, Lambert, and others; but some of them, I believe, are quite new. I will give you one instance of a quadratic equation, as the simplest.

Let the quadratic equation  $11xx - 15x + 5 = 0$ , be proposed. I say then, that if two recurring series be formed from the fractions  $\frac{1+2z}{1-z-zz}$ ,  $\frac{2+3z}{1-z-zz}$ , which have a common denominator, and each series of co-efficients, continued both ways (that is, as well before, as after the first term), the fractions formed by dividing each term of the 1st series by the corresponding term of the 2d series, viz. &c.  $\frac{-11}{-14}$ ,  $\frac{+7}{+9}$ ,  $\frac{-4}{-5}$ ,  $\frac{+3}{+4}$ ,  $\frac{-1}{-1}$ ,  $\frac{-3}{-4}$ ,  $\frac{2}{3}$ ,  $\frac{1}{2}$ ,  $\left| \frac{3}{5}, \frac{4}{7}, \frac{7}{12}, \frac{11}{19}, \frac{18}{31}, \frac{29}{50}, \right.$  &c. will converge in the simplest manner possible; those before the bar, in a retrograde order to the greater root  $\frac{15+\sqrt{5}}{22}$ ; and those after the bar, in a direct order to the smallest root  $\frac{15-\sqrt{5}}{22}$ ; where it is to be observed, that the greater root is affirmative, notwithstanding the sign — being prefixed to some of the terms, because in each fraction the numerator and the denominator are affected by the same sign, whether + or —.

The chief improvement I have made, consists in approximating to two roots at once, by one and the same series, continued backwards as well as forwards. I have not time to enlarge on this subject at present; but the little I have said will be a specimen of the method to be used in higher equations.



*XIV. Extract of Two Meteorological Journals of the Weather, observed at Nain in 57° N. Lat. and at Okak in 57° 20' N. Lat. both on the Coast of Labrador. Communicated by Mr. De La Trobe. p. 197.*

1779	Thermometer at Okak.			Thermometer at Nain.			Barometer at Nain.		
	High.	Lowest	Mean.	High	Lowest	Mean.	High.	Lowest	Mean.
August..	78.0	37.0	52.0	78.0	38.0	52.0	28.5	27.8	28 1.5
Septemb.	74.0	32.0	45.0	74.0	35.0	45.4	28.6	27.5	28 1.4
October	45.0	11.0	32.3	47.0	14.0	33.6	28.6	27.4½	28 2.5
Novemb.	36.0	15.0	27.3	36.0	9.0	27.3	28.5	27.2	28 6.1.
Decemb.	31.0	-12.0	12.7	32.0	-13.0	11.1	28.3½	27.1½	28 3.0
1780									
January	35.0	-13.0	15.3	35.0	-16.0	13.4	28.2	26.6	27 6.8
February	33.0	-17.0	13.1	32.0	-23.0	9.7	28.5½	26.10	27 9.5
March..	45.0	-14.0	10.0	39.0	-18.0	10.0	28.8	27.3	28 1.9
April ...	57.0	9.0	29.5	45.0	4.0	28.2	28.8½	27.7	28 3.7
May ....	55.0	23.0	39.0	53.0	23.0	37.6	28.6½	27.9	28 3.1
June....	68.0	33.0	43.6	61.0	32.0	43.1	28.3	26.11	28 0.5
July ....	84.0	38.0	52.0	84.0	39.0	52.2	28.1½	27.5	28 1.3
Mean of all .....			31.0			30.3	French Inches ..		28 1.5

*Meteorological Journal kept at the House of the Royal Society. By Order of the President and Council. p. 199.*

The abstract of the whole of the year 1780 is as follows.

1780.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Inches.
January ...	47.0	20.0	31.9	40.5	24.5	33.6	30.35	28.59	29.77	0.692
February ..	53.5	20.0	37.8	49.0	31.0	38.4	30.52	29.08	29.91	0.858
March ....	59.0	34.5	51.4	56.5	36.0	49.9	30.45	29.38	29.91	1.189
April. ....	65.5	33.0	46.7	65.0	38.0	48.0	30.17	28.81	29.65	2.739
May .....	84.5	45.0	59.7	74.5	51.0	60.4	30.28	29.38	29.94	0.822
June .....	84.5	48.0	62.9	76.0	50.0	63.2	30.37	29.79	30.01	0.852
July .....	82.0	54.0	66.4	78.5	61.0	69.4	30.34	29.59	30.05	1.602
August ....	83.5	58.5	69.2	76.5	62.0	68.3	30.20	29.85	30.05	0.485
September	84.0	48.0	61.3	79.0	56.0	65.2	30.20	29.14	29.83	2.903
October ...	66.0	41.5	52.3	65.0	46.0	53.9	30.29	28.66	29.69	2.356
November	52.0	26.0	42.0	53.0	36.0	43.6	30.47	28.94	29.85	2.505
December	50.0	24.0	38.5	46.5	32.0	39.8	30.55	29.69	30.29	0.310
Whole Year			51.7			52.8			29.91	17.313

Mean variation of the needle, 22° 41'.

Mean dip for the month of June, 72° 17'.

*XV. New Experiments on Gunpowder, &c. By Benjamin Thompson,\* Esq.,  
F. R. S. p. 229.*

These experiments were undertaken principally with a view to determine the most advantageous situation for the vent in fire-arms, and to measure the velocities of bullets, and the recoil under various circumstances. Mr. T. hoped also to find out the velocity of the inflammation of gunpowder, and to measure its force more accurately than had hitherto been done.

These experiments, on the force of fired gunpowder, on the same principle of those of Mr. Robins and Dr. Hutton, appear to have been made with great care and accuracy, but on a small scale, being performed only with a musket barrel. This and the other parts of the machinery were very nicely made, and contrived to answer the several purposes; which were, to determine the velocity of the bullets, the recoil of the barrel, the effect of firing the charge in different parts of it, the most advantageous situation for the vent, &c. Mr. T. had a contrivance for shutting the vent as soon as the fire was communicated to the charge; and it is very certain, that no part of the elastic fluid made its escape by this vent; for, on firing the piece, there was only a simple flash from the explosion of the priming, and no stream of fire was to be seen issuing from the vent, as is always to be observed when a common vent is made use of, and in all other cases where this fluid finds a passage. So that no part of the charge was lost by the vent. The velocities of the bullets were determined by means of a pendulum, into which they were discharged, according to the method invented by Mr. Robins, and pursued by Dr. Hutton with cannon-balls, as described at p. 282, &c. vol. 14. The chord of the arc, through which the pendulum ascended in each experiment, was measured by a ribbon, according to the method invented and described by Mr. Robins.

The recoil was measured in the following manner. The barrel was suspended in a horizontal position, and nearly in a line with the centre of the target, by two small pendulous rods, 64 inches in length, and 25.6 inches asunder; which being parallel to each other, and moving freely on polished pivots about the axes of their suspension, and on two pair of trunnions that were fixed to the barrel, formed, together with the barrel, a compound pendulum; and from the lengths of the vibrations of this pendulum, the velocity with which the barrel began to recoil, or rather its greatest velocity, was determined. But in order that the velocity of the recoil might not be too great, so as to endanger the apparatus when large charges were made use of, it was found necessary to load the barrel with an additional weight of more than 40 lbs. of iron. The chord of the arc through which the barrel ascended in its recoil, was measured by a ribbon also; and

\* Now Count of Rumford.



the lengths of those chords, expressed in inches and decimal parts of an inch, are set down in the tables. The method of computing the velocity of the recoil from the chord of the arc through which the barrel ascended, is too well known to require an explanation: and it is also well known, that the velocities are to each other as the chords of those arcs. The lengths of those chords, therefore, as they are set down in the tables, are, in all cases, as the velocities of the recoil.

The powder made use of in these experiments was of the best kind, such as is used in proving great guns at Woolwich. A cartridge, containing 12 lbs. of this powder, was given to Mr. T. by the late general Desaguliers of the Royal Artillery. This powder was immediately taken out of the cartridge, and put into glass bottles, which were previously made very clean and dry; and in these it was very carefully sealed up till it was opened for use. When it was wanted for the experiments, it was weighed out in a very exact balance, with so much attention, that there could hardly be an error in any instance greater than a quarter part of a grain. The bottles were never opened but in fine weather, and in a room that was free from damp, and no more charges of powder than were necessary for the experiments of the day were weighed out at a time. Each charge was carefully put up in a cartridge of very fine paper, and these filled cartridges were kept in a turned wooden box, that was varnished on the inside as well as the outside, to prevent its imbibing moisture from the air. The paper of which these cartridges were made, was so fine and thin, that 1280 sheets of it made no more than an inch in thickness, and a cartridge capable of containing half an ounce of powder weighed only  $\frac{3}{4}$  of a grain. The cartridges were formed on a wooden cylinder, and accurately fitted to the bore of the piece, and the edges of the paper were fastened together with paste made of flour and water.

When a cartridge was filled, the powder was gently shaken together, and its mouth was tied up and secured with a piece of fine thread; and when it was used it was put entire into the piece, and gently pushed down into its place with the ramrod, and afterwards it was pricked with a priming-wire thrust through the vent, and the piece was primed; so that no part of the powder of the charge was lost in the act of loading, as is often the case when the powder is put loose into the barrel: nor was any part of it expended in priming; but the whole quantity was safely lodged in the bottom of the bore or chamber of the piece, and the bullet was put down immediately upon it, without any wadding either between the cartridge and the bullet, or over the bullet.

The bullets were all cast in the same mould, and consequently could not vary in their weights above 2 or 3 grains at most, especially as care was taken to bring the mould to a proper temperature as to heat before the casting; and when

leather was put about them, or other bullets than those of lead were used, the weight was determined very exactly before they were put into the piece. The diameter of the bullet was determined by measurement and also by computation from its weight; and the specific gravity of the metal of which it was formed; and both these methods gave the same dimensions very nearly.

*A Table showing the weights and dimensions of the principal parts of the apparatus.*

*Of the barrel.*

	Inches.
Length.....	44.7
Length of the bore from the muzzle to the breech-pin .....	43.45
Diameter of the bore .....	0.78
Thickness of metal at the lower vent .....	0.36
Thickness of metal at the muzzle .....	0.1
Weight of the barrel, with the breech pin, the vent-screws, and vent tube, 6 lbs. 6 oz.	

*Of the gun carriage.*

Length.....	28.4
Distance between the two pair of trunnions .....	25.6
Diameter of each trunnion.....	0.25
Weight 40 lbs. 14 oz.	

*Of the rods by which the carriage was suspended.*

Length from the axis of suspension, or centre of the pivots, to the centre of the trunnions of the gun carriage, 64 inches.

Weight of each rod, 1 lb. 4 oz.

Total weight of the barrel and its carriage, with the allowance made for the weight of the rods by which it was suspended, 48 lbs. This was its weight from experiment N<sup>o</sup> 3, to experiment N<sup>o</sup> 123 inclusive.

*Of the bullet.*

Diameter 0.75 inch. Weight when of lead, 580 grains.

*Of the pendulum.*

Total length of the pendulum from the axis of suspension to the bottom of the circular plate.. 69.5

Diameter of the circular plate to which the targets were fastened .....

Distance between the shoulders of the pivots .....

Diameter of the pivots.....

Weight of the iron part of the pendulum 47 lb. 4 oz.

*Of the pendulum with the targets fixed to it, as it was prepared for making the experiments, and numbered.*

	Total length to the ribbon.	Distance from the axis of suspension.		Total weight of iron and wood.
		To the centre of gravity.	To the centre of oscillation.	
	Inches.	Inches.	Inches.	lbs. oz.
Pendulum N <sup>o</sup> 1	69.25	50.25	58.45	57 0
..... 2	69.5	54.4	59.15	82 4
..... 3	.....	55.62	60.23	100 12
..... 4	.....	54.6	59.18	88 4



*General table of the experiments.*

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Weight of the bullet.	Chord of the ascending arc of the pendulum.	Bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.							
	Grs.	In.	In.	Grs.	Inches.	Inches.	Inches.	Ft. in sec.	
N <sup>o</sup> 1	208	1.8	0	..	13.2	64.5	33.5	1267	First day.
2	..	..	.5	..	14.5	—	36.5	1399	
3	..	..	0	..	12.6	65.	17.8	1213	Second day.
4	..	..	.5	..	—	—	18.5	—	The pendulum gave way.
5	..	..	0	..	—	—	38.68	—	4 bullets fired at once.
6	..	..	.5	..	—	—	38.48	—	Ditto.
7	..	..	0	..	—	—	6.1	—	Without any bullet.
8	416	3.6	..	..	—	—	16.5	—	Ditto.
9	208	1.8	0	..	8.5	65.	17.69	1281	Pen. N <sup>o</sup> 2; very fair; 3d day.
10	104	.9	..	..	5.2	65.25	10.18	782	
11	310	2.7	0	..	9.6	64.6	24.69	1459	
12	..	..	1.22	..	10.1	65.	24.95	1527	
13	..	..	2.65	..	11.85	64.75	24.9	1801	The powder was lighted by the long vent-tube.
14	..	..	..	..	10.9	65.25	....	1646	
15	330	2.9	2.65	..	10.9	61.5	26.2	1748	
16	..	..	..	..	13.25	63.5	....	2060	
17	330	2.7	2.65	..	....	....	12.7	..	The barrel very much heated.
18	..	2.9	0	..	10.4	63.5	26.3	1619	
19	..	..	..	..	....	63.	26.4	1633	
20	165	1.45	0	..	6.8	62.2	14.73	1084	
21	..	..	..	..	6.85	....	14.2	1093	
22	..	..	1.32	..	6.7	....	14.8	1071	
23	..	..	..	..	6.3	60.6	14.58	1035	The short vent-tube was made use of.
24	..	..	..	..	7.5	61.5	14.68	1142	
25	165	1.45	0	..	6.8	65.	14.95	1004	Fourth day.
26	..	..	..	..	7.8	....	15.6	1153	
27	..	..	..	..	8.05	....	16.15	1192	
28	330	2.9	..	..	10.2	63.	26.	1559	
29	..	..	2.6	..	....	64.	28.1	1536	
30	165	3.2	..	..	5.9	62.4	13.2	914	
31	..	1.45	1.3	..	6.65	62.6	15.15	1027	
32	165	1.45	0	..	5.45	63.	15.45	839	Not leathered; weight of the bullet and was 603 grs. In exp. N <sup>o</sup> 32 no less than 40 large grs. of unfired powder were driven through the screen.
33	..	..	..	..	....	....	12.65	839	
34	..	..	..	..	7.9	....	15.45	1217	In these 6 experts. the bullets were leathered, and the powder was lighted by the valve-vent.
35	..	..	..	..	7.	60.25	15.25	1129	
36	..	..	..	..	7.4	62.	16.3	1161	
37	..	..	1.3	..	8.	61.	17.9	1277	
38	290	2.6	2.6	..	9.	58.6	23.5	1497	The pend. N <sup>o</sup> 2 ruined.
39	..	..	..	..	....	....	24.8	..	
40	218	1.9	0	..	6.45	64.6	18.	1236	5th day; medium velocity in these experiments and N <sup>o</sup> 47, 1225.
41	..	..	..	..	6.31	65.	17.71	1197	
42	..	..	..	..	6.45	65.	17.91	1230	

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Weight of the bullet.	Chord of the ascending arc of the pendulum.	Bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.							
	Grs.	In.	In.	Grs.	Inches.	Inches.	Inches.	Ft in sec.	
N <sup>o</sup> 43	..	..	1.3	..	6.5	64.6	18.3	1248	} Medium velocity 1276.
44	..	..	..	..	6.75	64.5	18.35	1299	
45	..	..	..	..	6.6	64.9	....	1265	
46	..	..	..	..	6.4	61.6	....	1293	
47	..	..	0	..	6.3	62.	18.1	1266	} Medium velocity 1427.
48	290	2.6	0	..	7.2	63.5	22.58	1414	
49	..	..	..	..	7.4	....	22.92	1455	
50	..	..	..	..	7.3	64.6	22.38	1412	
51	290	2.6	1.3	..	7.4	63.	23.21	1476	} Medium velocity 1493.
52	..	..	..	..	7.6	64.	23.76	1520	
53	..	..	..	..	7.25	61.	23.6	1483	
54	..	..	2.6	..	7.5	62.3	....	1502	
55	..	..	..	..	7.4	64.	23.26	1450	} Medium velocity 1460.
56	..	..	..	..	7.1	62.2	....	1433	
57	..	..	..	..	7.4	64.	23.56	1454	
58	..	..	..	..	1.31	—	11.12	—	} In these 4 experts. the piece was fired with powder alone, and the screen was taken away from before the pendulum.
59	..	..	..	..	1.2	—	11.62	—	
60	..	..	0	..	1.16	—	9.62	—	
61	..	..	1.3	..	0.6	—	11.33	—	
62	330	2.9	1.3	..	8.	63.	26.4	1599	} 6th day; medium velocity 1625.
63	..	..	..	..	8.5	65.	....	1652	
64	..	..	2.6	..	7.2	59.5	25.3	1562	
65	..	..	..	..	7.7	63.	....	1495	
66	..	..	0	..	8.4	....	26.35	1633	} Medium velocity 1594.
67	..	..	..	..	8.	....	25.8	1556	
68	218	1.9	0	..	6.82	64.	19.56	1349	
69	..	..	..	..	6.6	64.6	18.2	1294	
70	..	..	..	..	6.85	....	19.12	1345	} The powder was rammed very hard. Ditto much harder. Ditto as hard as in N <sup>o</sup> 68. Ditto ditto.
71	..	..	1.3	..	5.5	....	16.33	1080	
72	..	..	0	..	—	—	8.72	—	
73	..	..	..	..	—	—	8.44	—	
74	..	..	1.3	..	—	—	8.47	—	} Government powder, no bullet. Best double Battle powder. Government powder. Double Battle powder.
75	..	..	..	..	—	—	9.3	—	
76	145	1.3	0	..	5.3	65.	13.25	1037	
77	..	..	..	..	..	64.6	13.25	1044	
78	..	..	..	..	3.2	....	8.92	—	} 7th day; medium velocity 1040. 20 grs. best alkaline salt of tartar. 20 grs. æthiops mineral. 20 grs. sal ammoniac 20 grs. fine brass dust.
79	..	..	..	..	4.35	....	11.68	—	
80	..	..	..	..	3.3	63.6	9.83	—	
81	..	..	..	..	4.2	63.4	11.45	—	
82	..	..	..	..	—	—	15.25	—	} The screws which held the hooks by which the pendulum was suspended gave way, and the pendulum came down. 8th day; in each of these 4 experiments, from 50 to 70 granulæ or particles of unfired powder were driven through the screen.
83	..	..	..	..	—	—	14.35	—	
84	145	1.3	0	—	—	—	4.5	—	
85	..	..	..	90	1.33	62.2	7.16	1763	
86	..	..	..	251	2.82	63.2	9.62	1317	}
87	..	..	..	354	3.32	61.2	11.3	1136	



Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Weight of the bullet.	Chord of the ascending arc of the pendulum.	Bullet struck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.							
	Grs.	In.	In.	Grs.	Inches.	Inches.	Inches.	Ft. in sec.	
N <sup>o</sup> 88	..	..	0	600	6.5	65.4	15.22	1229	} Very few unfired grains of powder struck the screen.
89	..	..	..	603	6.34	64.6	15.13	1229	
90	..	..	..	1184	10.12	65.	21.92	978	} There were no marks of any unfired powder having reached the screen.
91	..	..	..	1754	13.65	63.4	27.18	916	
92	..	..	..	2352	16.55	63.3	32.25	833	
93	145	1.3	0	..	—	....	4.3	..	
94	165	1.45	..	..	—	....	5.5	..	
95	..	..	..	..	—	....	5.6	..	
96	290	2.6	..	..	—	....	11.70	..	
97	437 $\frac{1}{2}$	3.9	0	..	1.68	....	17.5	..	The screen was taken away.
98	..	..	..	..	6.7	....	15.88	..	} The whole surface of the target was bespattered with unfired grains of powder.
99	..	..	..	..	—	—	17.9	..	
100	165	1.45	0	..	.65	60.5	4.9	138	} In each of these experiments near $\frac{1}{10}$ part of the substance of the bullet was melted and blown away by the impulse of the flame.
101	..	..	..	..	.43	uncert.	4.8	92	
102	..	..	..	..	.86	63.	5.6	180	
									9th day.
103	104	.9	0	..	4.51	65.	10.6	732	} About 40 grs. of powder were driven through the screen.
104	145	1.3	..	..	5.4	....	12.92	877	
105	..	..	..	..	5.6	....	13.28	910	} About 40 unfired grs. of powder. Medium velocity 894. 40 unfired grains.
106	..	1.14	..	..	6.18	65.8	14.3	990	
107	218	1.8	..	..	8.48	65.	19.68	1380	} Double-proof Battle powder; no unfired grains.
108	290	2.6	..	..	9.45	65.6	23.9	1526	
109	..	..	..	..	8.73	65.2	22.8	1419	} Ditto, ditto.
110	..	..	..	..	9.3	65.5	23.4	1460	
111	..	..	..	..	....	....	....	1462	} Government powder: bullet leathered; weight 602 grains.
112	..	..	..	..	8.85	65.5	22.94	1436	
113	..	..	2.6	..	8.65	64.	23.7	1438	} Bullet naked; very few unfired grains.
114	..	..	..	..	8.5	63.6	24.1	1423	
115	..	..	..	..	8.4	65.	23.8	1378	} Medium velocity 1444.
116	..	2.28	..	..	9.15	64.	24.6	1525	
117	437 $\frac{1}{2}$	3.9	..	..	10.56	64.9	33.	1738	} Double proof Battle powder.
118	..	..	..	..	11.	64.5	33.3	1824	
119	..	..	..	..	10.5	65.	33.6	1729	} Gov. pow. } No unfired grs. through the scr..
120	..	..	2.6	..	10.35	....	32.5	1706	
121	..	..	..	..	10.65	....	33.2	1757	} Medium velocity 1764.
122	..	..	..	..	10.6	63.6	32.9	1789	
123	..	..	0	..	—	....	17.9	—	Without any bullet.

In the 2 first experiments the barrel was fixed to a carriage, that has not been described, which, together with the barrel and rods by which it was suspended, weighed only 23 $\frac{1}{2}$  lb. Length of the bore of the piece 43.5 inches. Weight.

of the bullet 580 grains. This gun carriage being found to be too light, another was substituted in the room of it.

To determine how much of the force of the powder was lost by windage and by the vent, oiled leather was fastened round the bullet, so that it now accurately fitted the bore of the piece; and in the 5 experiments, from N<sup>o</sup> 35 to N<sup>o</sup> 39 inclusive, the valve-vent was made use of. Weight of the bullet, in the exper. 3 to 24, with the leather in which it was enveloped, 603 grains.

Finding that the blast of the powder always reached as far as the pendulum, when large charges were used, and suspecting that this circumstance, with the impulse of the unfired grains, might in a great measure occasion the apparent irregularity in the velocities of the bullets; to remedy these inconveniences, a large sheet of paper of a moderate thickness was stretched on a square frame of wood, and interposed as a screen before the pendulum at the distance of 2 feet from the surface of the target, in the exper. 25, 31. The screen was found to answer perfectly well the purpose for which it was designed, and it was continued during the remainder of the experiments, the paper being replaced every 3d or 4th experiment.

The bullets were now put naked into the piece, exper. 32, 39, and the powder was lighted by the short vent-tube, and some little improvement was made in the steel edges between which the ribbons passed that served to measure the ascending arcs of the pendulum and of the recoil, by which means the friction was lessened, and the ribbon was prevented from twisting or entangling itself as it was drawn out.

The apparatus, commencing exper. 40, the barrel with its carriage as before; the pendulum, N<sup>o</sup> 3, and leaden bullets, weighing 580 grains each.

The experiments N<sup>o</sup> 78, 79, 80, and 81, were made in hopes of being able to discover a method of adding to the force of gunpowder. Twenty grains of the substances mentioned in the remarks on each experiment were intimately mixed with the powder of the charge. In the experiment N<sup>o</sup> 82, a large wad of tow well soaked in ethereal spirit of turpentine, was put into the piece immediately on the bullet: and in the experiment N<sup>o</sup> 83 a wad, soaked in alkohol, was put into the piece in like manner.

In the 9 experiments, viz. from N<sup>o</sup> 84 to N<sup>o</sup> 92 inclusive, the valve-vent was used, and the bullets were made to fit the bore of the piece very exactly by means of oiled leather, which was so firmly fastened about them that in each experiment it entered the target with the bullet. The bullet used in experiment N<sup>o</sup> 85, was of wood. Those used in the experiments N<sup>o</sup> 86 and N<sup>o</sup> 87, were formed in the following manner: a small bullet was cast of plaister of Paris, which being thoroughly dried, and well heated at the fire, was fixed in the centre of the mould that served for casting all the leaden bullets used in these experiments;



and melted lead being poured into this mould, the cavity that surrounded the small plaister bullet was entirely filled up, and a bullet was produced, which to the eye had every appearance of solidity, but was as much lighter than a solid leaden bullet of the same diameter, as the plaister bullet was lighter than a leaden bullet of the same size. In the experiments N° 88 and N° 89, solid leaden bullets were used. In the experiment N° 90, 2 bullets were discharged at once; in the experiment N° 91, 3; and in the experiment N° 92, 4 were used. In each of these experiments a fresh sheet of paper was used as a screen to the pendulum, that the velocities of the bullets might be measured more accurately; and also, that the quantity of unfired powder might be estimated with greater precision.

In the experiments N° 93 to 99 the piece was fired with powder only.

In the experiments N° 100 and N° 101, the bullets were not put down into the bore, but were supported by 3 wires, which being fastened to the end of the barrel projected beyond it, and confined the bullet in such a situation that its centre was in a line with the axis of the bore, and its hinder part was  $\frac{1}{10}$  of an inch without or beyond the mouth of the piece. In experiment N° 102, the bullet was just stuck into the barrel in such a manner that near  $\frac{1}{2}$  of it was without the bore. All that part of the bullet which lay towards the bore of the piece appeared to be quite flat from the loss of substance it had sustained; and its surface was full of small indents, which probably were occasioned by the unfired grains of powder that struck against it.

The experiments N° 103 to 123 were made with the pendulum N° 4. The rest of the apparatus as before.

*Of the method used in computing the velocities of the bullets.*—As the method of computing the velocity of a bullet from the arc of the vibration of a pendulum into which it is fired is so well known, Mr. T. does not enlarge on it, but just gives the theorems that have been proposed by different authors, and refers those who wish to see more on the subject to Mr. Robins's New Principles of Gunnery; to Mr. Euler's Observations on Mr. Robins's book; and lastly to Dr. Hutton's paper on the initial Velocities of Cannon Balls, published in the Philos. Trans. for the year 1778.

If  $a$  denote the length of the axis of the pendulum to the ribbon which measures the chord of the arc of its vibration;

$g$ , the distance of the centre of gravity below the axis;

$f$ , the distance of the centre of oscillation;

$h$ , the distance of the point struck by the bullet;

$c$ , the chord of the ascending arc of the pendulum;

$p$ , the weight of the pendulum;

$b$ , the weight of the bullet, and

$v$ , the original velocity of the bullet: Then

$v = \frac{c}{a} \times \frac{rg}{bh} + \frac{h}{f} \times \frac{f}{\sqrt{2h}}$  is a theorem for finding the velocity on Mr. Robins's principles.

$v = \frac{c}{a} \times \frac{rg}{bh} + \frac{f+h}{2f} \times \sqrt{\frac{f}{2}}$ , is the theorem proposed by Mr. Euler, who has corrected a small error in Mr. Robins's method; and

$v = 5.672 \, cg \sqrt{f} \times \frac{r+b}{bha}$  is Dr. Hutton's theorem, which is sufficiently accurate, and far more simple and expeditious than either of the preceding. It is to be remembered, that  $g$ ,  $h$ , and  $c$ , may be expressed in any measure; but  $f$  must be English feet, and  $v$  will be the velocity of the bullet in English feet in a second.

The velocities of the bullets in most of the foregoing experiments were first computed by Euler's method; but in going over the calculations a 2d time, Mr. T. used Dr. Hutton's theorem. Both these methods gave the same velocity very nearly, but the Doctor's method is by much the easiest in practice. In these computations care was taken to make a proper allowance for the bullets that were lodged in the pendulum, and also for the velocity lost by the bullet in passing through the screen.

The corrections necessary on account of the bullets lodged in the pendulum were made in the following manner.

$b$  was continually added to the value of  $r$ ,  
 $\frac{h-g}{r} \times b$  ..... to the value of  $g$ , and  
 $\frac{f-h}{r} \times b$  ..... to the value of  $f$ .

*Of the spaces occupied by the different charges of powder.*—The heights of the charges of powder, or the lengths of the spaces which they occupied in the bore, were determined by measurement; and in order that this might be done with greater accuracy, inches and tenths of inches were marked on the ram-rod, and the charge was gently forced down till it occupied the same space in each experiment. The annexed table shows the heights of the charges as they were determined by measurement, and also their heights computed from the diameter of the bore of the piece, and the specific gravity of the powder that was used.

Weight of the powder.	Height of the charge.	
	Measured.	Computed.
Grs.	Inches.	Inches.
104	.9	0.8957
145	1.3	1.2490
165	1.45	1.4211
208	1.8	1.7914
218	1.9	1.8775
290	2.6	2.4980
310	2.7	2.6700
330	2.9	2.8422
416	3.6	3.5828
437½	3.9	3.7680

In the experiment N<sup>o</sup> 30, the powder was put into a cartridge so much smaller than the bore of the piece, that the charge, instead of occupying 1.45 inches, extended 3.2 inches. By this disposition of the powder, its action on the bullet appears to have been very much diminished.



*Of the effect that the heat which pieces acquire in firing produces on the force of powder.*—It is very probable, that the excess of the velocity of the bullet in the 2d experiment, over that of the first, was occasioned more by the heat the barrel had acquired in the first experiment than by the position of the vent, or any other circumstance; for Mr. T. found, on repeated trials, that the force of any given charge of powder is considerably greater when it is fired in a piece that has been previously heated by firing, or by any other means, than when the piece has not been heated. Every body that is acquainted with artillery knows, that the recoil of great guns is much more violent after the 2d or 3d discharge, than it is at first; and on ship-board, where it is necessary to attend to the recoil of the guns, in order to prevent dangerous accidents that might be occasioned by it, the constant practice has been on board of ships, to lessen the quantity of powder after the first 4 or 5 rounds. By the recoil it should seem that the powder exerted a greater force also in the 4th experiment, being the 2d on the 2d day, than it did on the 3d, or the 1st of that day; but the pendulum giving way, it was not possible to compare the velocities of the bullets in the manner we did in the 2 experiments above-mentioned.

Concluding from the result of the experiments mentioned above, as well as from other reasons, that the temperature of the piece has a considerable effect on the force of the powder, Mr. T. afterwards took care to bring the barrel to a proper degree of heat, by firing it once or oftener with powder each time he recommenced the experiments after the piece had been left to cool.

*Of the manner in which pieces acquire heat in firing.*—Mr. T. was much surprised, on taking hold of the barrel immediately after the experiment N<sup>o</sup> 17, when it was fired with 330 gr. of powder without any bullet, to find it so very hot that he could scarcely bear it in his hand, evidently much hotter than he had ever observed it before, though the same charge of powder had been used in the two preceding experiments, and in both these experiments the piece was loaded with a bullet, which one would naturally imagine, by confining the flame, and prolonging the time of its action, would heat the barrel much more than when it was fired with powder alone. This, Mr. T. remarks, cannot happen from the heat of the inflamed powder, but from the rapidity of its action on the piece, by which the particles of the metal are put into a very quick vibratory motion, which soon produces a great heat through its whole substance; like as when any body is struck with a rapid blow by another hard body, even when cold; and as a bullet becomes hot when striking against any hard obstacle. This Mr. T. illustrates in various instances, and then adds:

Now the effort of any given charge of powder on the gun is very nearly the same, whether it be fired with a bullet or without; but the velocity with which the generated elastic fluid makes its escape, is much greater when the powder is fired alone, than when it is made to impel one or more bullets; the heat ought



therefore to be greater in the former case than in the latter, as I found by experiment. But to make this matter still plainer, we will suppose any given quantity of powder to be confined in a space that is just capable of containing it, and that in this situation it is by any means set on fire. Let us suppose this space to be the chamber of a piece of ordnance of any kind, and that a bullet, or any solid body, is so firmly fixed in the bore immediately on the charge, that the whole effort of the powder shall not be able to remove it. As the powder goes on to be inflamed, and the elastic fluid is generated, the pressure on the inside of the chamber will be increased, till at length, all the powder being burnt, the strain on the metal will be at its greatest height, and in this situation things will remain, the cohesion or elasticity of the particles of metal counterbalancing the pressure of the fluid. Under these circumstances very little heat would be generated; for the continued effort of the elastic fluid would approach to the nature of the pressure of a weight; and that concussion, vibration, and friction, among the particles of the metal, which in the collision of elastic bodies is the cause of the heat that is produced, would scarcely take effect.

But instead of being firmly fixed in its place, let the bullet now be moveable, but let it give way with great difficulty, and by slow degrees. In this case, the elastic fluid will be generated as before, and will exert its whole force on the chamber of the piece; but as the bullet gives way to the pressure, and moves on in the bore, the fluid will expand itself and become weaker, and the particles of the metal will gradually return to their former situations; but the velocity with which the metal restores itself being but small, the vibration that remains in the metal, after the elastic fluid has made its escape, will be very languid, as will be the heat that is generated by it. But if, instead of giving way with so much difficulty, the bullet is much lighter, so as to afford but little resistance to the elastic fluid in making its escape, or if the powder is fired without any bullet at all; then, there being little or nothing to oppose the flame in its passage through the bore, it will expand itself with an amazing velocity, and its action on the gun will cease almost in an instant, the strained metal will restore itself with a very rapid motion, and a sharp vibration will ensue, by which the piece will be much heated.

*Of the effect of ramming the powder in the chamber of the piece.*—The charge, consisting of 218 gr. of powder, being put gently into the bore of the piece in a cartridge of very fine paper, without being rammed, the velocity of the bullets at a mean of the 40th, 41st, 42d, and 47th exper., was at the rate of 1225 feet in a second; but in the 68th, 69th, and 70th exper., when the same quantity of powder was rammed down with 5 or 6 hard strokes of the ram-rod, the mean velocity was 1329 feet in a second. Now the total force or pressure exerted by the charge on the bullet is as the square of its velocity, and  $1329^2$  is to  $1225^2$  as 1.1776 is to 1; or nearly as 6 is to 5; and in that proportion was the force of the given charge of powder increased by being rammed.



In the 71st experiment the powder was also rammed, but the vent, instead of being at the bottom of the bore, was at 1.3, and the velocity of the bullet was very considerably diminished, being only at the rate of 1080 feet in a second, instead of 1276 feet in a second, which was the mean velocity with this charge, and with the vent in this situation when the powder was rammed. See experiments N<sup>o</sup> 43, 44, 45, and 46.

When, instead of ramming the powder, or pressing it gently together in the bore, it is put into a space larger than it is capable of filling, the force of the charge is thereby sensibly lessened, as Mr. Robins and others have found by repeated trials. In the 30th experiment the charge, consisting of no more than 165 grains of powder, was made to occupy 3.2 inches of the bore, instead of 1.45 inches, which space it just filled when it was gently pushed into its place without being rammed; the consequence was, the velocity of the bullet, instead of being 1100 feet in a second or upwards, was only at the rate of 914 feet in a second, and the recoil was lessened in proportion.

And hence we may draw this practical inference, that the powder, with which a piece of ordnance or a fire-arm is charged, ought always to be pressed together in the bore; and if it is rammed to a certain degree, the velocity of the bullet will be still further increased. It is well known, that the recoil of a musket is greater when its charge is rammed than when it is not; and there cannot be a stronger proof that ramming increases the force of the powder.

*Of the relation of the velocities of bullets to the charges of powder by which they are impelled.*—It appears by all the experiments that have hitherto been made on the initial velocities of bullets, that when the weights and dimensions of the bullets are the same, and they are discharged from the same piece by different quantities of powder, the velocities are in the sub-duplicate ratio of the weights of the charges very nearly.

The following table will shew how accurately this law obtained in the foregoing experiments.

Charges.	Velocities.		Difference.	N <sup>o</sup> of exp.
	Computed.	Actual.		
437 $\frac{1}{2}$	1764	1764	.....	3
330	1533	1594	+ 61	2
310	1486	1459	- 27	1
290	1436	1436	0	7
218	1232	1225	- 7	4
208	1216	1256	+ 40	3
165	1083	1087	+ 4	2
145	1018	1040	+ 22	2
104	860	757	- 103	2

The computed velocities, as they are set down in this table, were determined from the ratio of the square root of 437 $\frac{1}{2}$  (the weight in grains of the largest charge of powder) to the mean velocity of the bullet with that charge and the

vent at O, viz. 1764 feet in a second, and the square root of the other charges expressed in grains. And the actual velocities are means of all experiments that were made under similar circumstances. The 4th column shows the difference of the computed and actual velocities, or the number of feet in a second by which the actual velocity exceeds or falls short of the computed: and in the 5th column is set down the number of experiments with each charge, from the mean of which the actual velocity was determined.

The agreement of the computed and actual velocities will appear more striking, if we take the sum and difference of those velocities with all the charges except the first: thus,

Sum of the velocities, — 1764.			
Computed.	Actual.	Difference.	N <sup>o</sup> of exp.
9864	9854	— 10	23

So that it appears, that the difference, or the actual velocity, was smaller than the computed by  $\frac{1}{985}$  part only at a mean of 23 experiments.

But as by far the greater number of the experiments were made with the following charges, viz. 290, 218, 208, 165, and 145 grains of powder, let us take the sum and difference of the computed and actual velocities of those charges: thus,

Sum of the velocities.			
Computed.	Actual.	Difference.	N <sup>o</sup> of exp.
5985	6044	+ 59	18

Here the agreement of the theory with the experiments is so very remarkable, that we must suppose it was in some measure accidental; for the difference of the velocities in repeating the same experiment, is in general much greater than the difference of the computed and actual velocities in this instance; but we may fairly conclude, from the result of all these trials, that the velocities of like musket bullets, when they are discharged from the same piece by different quantities of the same kind of powder, are very nearly in the sub-duplicate ratio of the weights of the charges.

*On the effect of placing the vent in different parts of the charge.*—There have been 2 opinions with respect to the manner in which gunpowder takes fire. Mr. Robins supposes that the progress of its inflammation is so extremely rapid, “that all the powder of the charge is fired and converted into an elastic fluid, before the bullet is sensibly moved from its place;” while others have been of opinion that the progress of the inflammation is much slower, and that the charge is seldom or never completely inflamed before the bullet is out of the gun. The large quantities of powder that are frequently blown out of fire-arms uninflamed, seem to favour the opinion of the advocates for the gradual firing;



but Mr. Robins endeavours to account for that circumstance on different principles, and supports his opinion by showing that every increase of the charge within the limits of practice produces a proportional increase of the velocity of the bullet, and that when the powder is confined by a great additional weight, by firing 2 or more bullets at a time instead of 1, the velocity is not sensibly greater than it ought to be according to his theory.

If this were a question merely speculative, it might not be worth while to spend much time in the discussion of it; but as it is a matter upon the knowledge of which depends the determination of many important points respecting artillery, and from which many useful improvements may be derived, too much pains cannot be taken to come at the truth. Till the manner in which powder takes fire, and the velocity with which the inflammation is propagated, are known, nothing can with certainty be determined with respect to the best form for the chambers of pieces of ordnance, or the most advantageous situation for the vent; nor can the force of powder, or the strength that is required in different parts of the gun, be ascertained with any degree of precision.

As it would be easy to determine the best situation for the vent from the velocity of the inflammation of powder being known, so on the other hand I had hopes of being able to come at that velocity by determining the effect of placing the vent in different parts of the charge; for which purpose the following experiments were made.

By the annexed experiments it appears, 1st, that the difference in the force of any given charge of powder which arises from the particular situation of the vent is extremely small.

2dly. From the result of all these experiments it appears, that the effect of placing the vent in different positions with respect to the bottom of the chamber is different, in different charges; thus, with 165 grains of powder, the velocity of the bullet was rather diminished by removing the vent from 0, or the bottom of the bore, to 1.32; but with 218 grains of powder the velocity was a little increased, as was also the recoil. With 290 grains of powder the velocity was greatest

*Experiments showing the effect of placing the vent in different parts of the charge.*

Weight of the charge of powder.	Space occupied by the powder.	Vent from the bottom of the bore.	Velocity of the bullet at a medium.	Recoil measured upon the ribbon at a medium.	Number of experiments.
Grains.	Inches.	Inches.	Ft. in a sec.	Inches.	
165	1.45	0	1087	14.465	2
....	....	1.32	1082	14.31	3
218	1.9	0	1225	17.93	4
....	....	1.3	1276	18.34	4
290	2.6	0	1427	22.626	3
....	....	1.3	1493	23.34	3
....	....	2.6	1460	23.286	4
....	....	0	1444	23.135	4
....	....	2.6	1413	24.5	3
310	2.7	0	—	24.69	1
....	....	1.32	—	24.95	1
....	....	2.65	—	24.9	1
330	2.9	0	1594	26.075	2
....	....	1.3	1625	26.4	2
....	....	2.6	1525	25.3	2
437½	3.9	0	1764	33.3	3
....	....	2.6	1751	32.866	3



when the powder was lighted at the vent 1.3, which was near the middle of the charge, and rather greater when it was lighted at the top, or immediately behind the bullet, than when it was lighted at the bottom. And by the recoil it would seem, that the velocities of the bullets varied nearly in the same manner when the charge consisted of 310 grains of powder. With 330 grains of powder, both the velocity and the recoil were greater when the powder was lighted at the middle of the charge, than when it was lighted at the bottom; but they were least of all when it was lighted near the top. And when an ounce of powder was made use of for the charge, its force was greatest when it was lighted at the bottom. But the difference in the force exerted by the powder which arose from the particular position of the vent was in all cases so inconsiderable, being less than what frequently occurred in repeating the same experiment, that no conclusion can be drawn from the experiments, except only this, that any given charge of powder exerts nearly the same force, whatever is the position of the vent.

And hence the following practical inference naturally occurs, viz. that in the formation of fire-arms, no regard need be had to any supposed advantages that gun-smiths and others have hitherto imagined were to be derived from particular situations for the vent, such as diminishing the recoil, increasing the force of the charge, &c.; but the vent may be indifferently in any part of the chamber where it will best answer on other accounts: and there is little doubt but the same thing will hold good in great guns, and all kinds of heavy artillery. The form Mr. T. recommends for the bottom of the bore, is that of a hemisphere; and the vent to be brought out directly through the side of the barrel, in a line perpendicular to its axis, and pointing to the centre of the hemispherical concavity of the chamber. In this case the vent would be the shortest possible; it would be the least liable to be obstructed, and the piece would be more easily cleaned, than if the bottom of the bore was of any other form. All these advantages, and several others not less important, would be gained by making the bottom of the bore and vent of great guns in the same manner.

*A new\* method of determining the velocities of bullets.*—From the equality of action and re-action it appears that the momentum of a gun must be precisely equal to the momentum of its charge; or that the weight of the gun, multiplied into the velocity of its recoil, is just equal to the weight of the bullet and of the powder (or the elastic fluid that is generated from it) multiplied into their respective velocities: for every particle of matter, whether solid or fluid, that issues out of the mouth of a piece, must be impelled by the action of some power, which power must re-act with equal force against the bottom of the bore. Even

\* This method is not new; as a specimen of it was given by Robins, in his *New Principles of Gunnery*, prop. 11, p. 109. Nor is the method generally true, as is proved in Dr. Hutton's *Tracts*, p. 462.



the fine invisible elastic fluid, generated from the powder in its inflammation, cannot put itself in motion without re-acting against the gun at the same time. Thus we see pieces when they are fired with powder alone, recoil as well as when their charges are made to impel a weight of shot, though the recoil is not in the same degree in both cases.

It is easy to determine the velocity of the recoil in any given case, by suspending the gun in a horizontal position by two pendulous rods, and measuring the arc of its ascent, by means of a ribbon according to the method already described, and this will give the momentum of the gun, its weight being known, and consequently the momentum of its charge. But in order to determine the velocity of the bullet from the recoil, it will be necessary to find out how much the weight and velocity of the elastic fluid contributes to it. That part of the recoil which arises from the expansion of this fluid is always very nearly the same, whether the powder is fired alone, or whether the charge is made to impel one or more bullets; which Mr. T. says he has found by a great variety of experiments.\* If therefore a gun, suspended according to the method prescribed, be fired with any given charge of powder, but without any bullet or wad, and the recoil be observed; and if the same piece be afterwards fired with the same quantity of powder, and a bullet of a known weight, the excess of the velocity of the recoil in the latter case, over that in the former, will be proportional to the velocity of the bullet; for the difference of these velocities, multiplied into the weight of the gun, will be equal to the weight of the bullet multiplied into its velocity.

Thus if  $w$  be put equal to the weight of the gun,

$u$  = the velocity of its recoil when fired without a bullet,

$v$  = the velocity of the recoil when the same charge impels a bullet,

$B$  = the weight of the bullet, and

$v$  = its velocity; then it will be

$$v = \frac{v - u}{B} \times w.$$

Let us see how this method of determining the velocities of bullets will answer in practice. In the 94th exper. the recoil, with 165 gr. of powder, without a bullet, was 5.5 inches, and in the 95th exper. with the same charge, the recoil was 5.6 inches. The mean is 5.55 inches; and the length of the rods by which the barrel was suspended being 64 inches, the velocity of the recoil  $u$ , answering to 5.55 inches measured on the ribbon, is that of 1.1358 feet in a second. Now in 5 experiments, viz. exper. 20, 21, 22, 23, 24, with the same charge of powder, and a bullet weighing 580 gr., the medium recoil was 14.6 inches.

\* Remark'd in Robins's New Principles of Gunnery, prop. 11, p. 111. But it is not generally true.

And the velocity of the recoil ( $= v$ ) answering to this length is that of 2.9880 feet in a second: consequently  $v - u$ , or  $2.9880 - 1.1358$  is equal to 1.8522 feet in a second.

But as the velocities of recoil are known to be as the chords of the arcs through which the barrel ascends, it is not necessary, in order to determine the velocity of the bullet, to compute the velocities  $v$  and  $u$ ; but the quantity  $v - u$ , or the difference of the velocities of the recoil when the given charge is fired with and without a bullet, may be computed from the value of the difference of the chords, by one operation. Thus the velocity answering to the chord 9.05 is that of 1.8522 feet in a second, which is just equal to  $v - u$ , as was before found.

The weight of the barrel, together with its carriage, was  $47\frac{1}{4}$  pounds, to which three quarters of a pound is to be added on account of the weight of the rods by which it was suspended, which makes  $w = 48$  pounds, or 336,000 grains, and the weight of the bullet was 580 grains.  $B$  is therefore to  $w$  as 580 is to 336,000, that is, as 1 is to 579.31, very nearly; and  $v = \frac{v - u}{B} \times w$  is equal to  $(v - u) \times 579.31$ .

The value of  $v - u$  answering to the experiments before-mentioned was found to be 1.8522; consequently the velocity of the bullets,  $= v$ , was  $1.8522 \times 579.31 = 1073$  feet in a second, which is extremely near 1083 feet in a second, the mean of the velocities, as they were determined by the pendulum.

But the computation for determining the velocity of a bullet on these principles may be rendered still more simple and easy in practice; for the velocities of the recoil being as the chords measured on the ribbon, if  $c$  be put equal to the chord of the recoil when the piece is fired with powder only, and

$c =$  the chord when a bullet is discharged by the same charge, then  $c - c$  will be as  $v - u$ ; and consequently as  $\frac{v - u}{B} \times w$ , which measures the velocity of the bullet, the ratio of  $w$  to  $B$  remaining the same.

If therefore we suppose a case in which  $c - c$  is equal to 1 inch, and the velocity of the bullet be computed from that chord, the velocity in any other case, when  $c - c$  is greater or less than 1 inch, will be found by multiplying the difference of the chords  $c$  and  $c$  by the velocity that answers to a difference of 1 inch. The length of the parallel rods by which the barrel was suspended being 64 inches, the velocity of the recoil answering to  $c - c = 1$  inch measured on the ribbon is 0.204655 parts of a foot in a second; and this is also, in this case the value of  $v - u$ ; the velocity of the bullet is therefore  $v = 0.204655 \times 579.31 = 118.35$  feet in a second. Consequently the velocity of the bullet, expressed in feet per second, may in all cases be found by multiplying the difference of the chords  $c$  and  $c$ , by 118.35, the weight of the barrel, the length of the



rods by which it is suspended, and the weight of the bullet remaining the same, and this whatever the charge of powder may be that is made use of, and however it may differ in strength and goodness.

According to this rule the velocities of the bullets in the several experiments have been computed from the recoil; and by comparing them with the velocities shown by the pendulum, we shall be enabled to judge of the accuracy of this new method of determining the velocities of bullets. The consequence of this comparison is, that several of the velocities, as determined by the two methods, agree nearly together; and that several, on the contrary, disagree very much, and that by differences which Mr. T. cannot account for. Hence it ought to be inferred, that the new method is not generally to be relied on.

*Of a very accurate method of proving gunpowder.*—All the epreuves, or powder-triers, in common use are defective in many respects. Neither the absolute force of gunpowder can be determined by means of them, nor the comparative force of different kinds of it, but under circumstances very different from those in which the powder is made use of in service. As the force of powder arises from the action of an elastic fluid, generated from it in its inflammation, the quicker the charge takes fire, the more of this fluid will be generated in any given short space of time, and the greater of course will be its effect on the bullet. But in the common method of proving gunpowder, the weight by which the powder is confined is so great in proportion to the quantity of the charge, that there is time quite sufficient for the charge to be all inflamed, even when the powder is of the slowest composition, before the body to be put in motion can be sensibly moved from its place. The experiment therefore may show which of two kinds of powder is the stronger when equal quantities of both are confined in equal spaces, and completely inflamed; but the degree of the inflammability, which is a property essential to the goodness of the powder, cannot by these means be ascertained.

Hence it appears how powder may answer to the proof, such as is commonly required, and may yet turn out very indifferent when it comes to be used in service. And this, Mr. T. believes, frequently happens; at least he knows complaints from officers of the badness of our powder are very common; and he would suppose that no powder is ever received by the Board of Ordnance but such as has gone through the established examination, and has answered to the usual test of its being of the standard degree of strength. But though the common powder triers may show powder to be better than it really is, they never can make it appear to be worse than it is; it will therefore always be the interest of those who manufacture that commodity to adhere to the old method of proving it. But the purchaser will find his account in having it examined in a method by which its goodness may be ascertained with greater precision.



The method Mr. T. would recommend, is as follows. A quantity of powder being provided, which, from any previous examination or trial, is known to be of a proper degree of strength to serve as a standard for the proof of other powder, a given charge of it is to be fired, with a fit bullet, in a barrel suspended by 2 pendulous rods, and the recoil is to be carefully measured on the ribbon. And this experiment being repeated 3 or 4 times, or oftener if there be any difference in the recoil, the mean and the extremes of the chords may be marked on the ribbon by black lines drawn across it, and the word proof may be written on the middle line; or if the recoil be uniform (which it will be to a sufficient degree of accuracy, if care be taken to make the experiments under the same circumstances) then the proof mark is to be made in that part of the ribbon to which it was constantly drawn out by the recoil in the different trials.

The recoil, with a known charge of standard powder, being thus ascertained and marked on the ribbon, let an equal quantity of any other powder, that is to be proved, be fired in the same barrel, with a bullet of the same weight, and every other circumstance alike; then if the ribbon is drawn out as far or farther than the proof mark, the powder is as good or better than the standard; but if it falls short of that distance, it is worse than the standard, and is to be rejected. For the greater the velocity is with which the bullet is impelled, the greater will be the recoil; and when the recoil is the same, the velocities of the bullets are equal, and the powder is of the same degree of strength, if the quantity of the charge be the same. And if care be taken in proportioning the charge to the weight of the bullet, to come as near as possible to the medium proportion that obtains in practice, the determination of the goodness of gunpowder, from the result of this experiment, cannot fail to hold good in actual service.

The length of the bore recommended, is 30 inches, and its diameter 1 inch, consequently it is just 30 calibres in length, and will carry a leaden bullet of about 3 ounces. The barrel may be made of gun metal, or of cast iron as that is a cheaper commodity; but great care must be taken in boring it, to make the cylinder perfectly straight and smooth, as well as to preserve the proper dimensions. Of whatever metal the barrel is made, it ought to weigh at least 50 lbs. in order that the velocity of the recoil may not be too great; and the rods by which it is suspended should be 5 feet in length. The vent may be about  $\frac{1}{8}$  of an inch in diameter; and it should be bouched or lined with gold, in the same manner as the touch-hole is made in the better kind of fowling-pieces, in order that its dimensions may not be increased by repeated firing.

The bullets should be made to fit the bore with very little windage; and it would be better if they were all cast in the same mould, and of the same parcel of lead, as in that case their weights and dimensions would be more accurately the same, and the experiments would of course be more conclusive. The stated



charge of powder may be half an ounce, and it should always be put up in a cartridge of very fine paper; and after the piece is loaded it should be primed with other powder, first taking care to prick the cartridge by thrusting a priming wire down the vent. The machine for the tape to slide through may be the same as is described by Dr. Hutton in his account of his experiments on the initial velocities of cannon balls; as his method is much better calculated to answer the purpose than that proposed and made use of by Mr. Robins. It will also be better for the axis of the pendulous rods to rest on level pieces of wood or iron, than for them to move in circular grooves: only care must be taken to confine them by staples or some other contrivance, to prevent their slipping out of their places. The trunnions, by means of which the barrel is connected with the pendulous rods, and on which it is supported, should be as small as possible, in order to lessen the friction; and for the same reason they should be well polished, as well as the grooves that receive them. They need not be cast on the barrel, but may be screwed into it after it is finished.

In making the experiments, regard must be had to the heat of the barrel, as well as to the temperature of the atmosphere; for heat and cold, dryness and moisture, have a very sensible effect on gunpowder to increase or diminish its force. If therefore a very great degree of accuracy is at any time required, it will be best to begin by firing the piece 2 or 3 times merely to warm it; after which 3 or 4 experiments may be made with standard powder, to determine anew the proof mark, as the strength of the same powder is different on different days; and when this is done, the experiments with the powder that is to be proved are to be made, taking care to preserve the same interval of time between the firings, that the heat of the piece may be the same in each trial. If all these precautions are taken, and if the bullets are of the same weight and dimensions, powder may be proved by this method with much greater accuracy than has hitherto been done by any of the common methods made use of for that purpose.\*

*Of the comparative goodness, or value, of powder of different degrees of strength.*—Estimating the strength of powder by the square of the velocity a given charge gives to a bullet, by experiment Mr. T. finds that double proof Battle powder is stronger than the same weight of government powder, in the proportion of 6 to 5. But as the former was sold at 2 shillings the lb. and the

\* The foregoing method for an epreuve, appears to be the same as a new one hinted and recommended by Mr. Robins, at p. 123 of his *New Principles of Gunnery*. And from the same remark of Mr. Robins's also, Dr. Hutton made one with a small brass cannon, and several other improved contrivances, which was much used at Woolwich, and found to be a very regular and accurate epreuve, and most ready and convenient when used with powder only, without balls.



latter at 1s. 0 $\frac{3}{5}$ d. Mr. T. infers that Battle powder is therefore sold at the rate of 10d. a pound, or 41 $\frac{2}{3}$  per cent dearer than in proportion it ought to be.

*Of the relation of the velocities of bullets to their weights.*—According to Mr. Robins's theory, when bullets of the same diameter, but different weights, are discharged from the same piece by the same quantity of powder, their velocities should be in the reciprocal sub-duplicate ratio of their weights. But as this theory is founded on a supposition that the action of the elastic fluid, generated from the powder, is always the same in any and every given part of the bore when the charge is the same, whatever may be the weight of the bullet; and as no allowance is made for the expenditure of force required to put the fluid itself in motion, or for the loss of it by the vent; it is plain that the theory is defective. It is true that Dr. Hutton, in his experiments, found this law to obtain without any great error, and possibly it may hold good with sufficient accuracy in many cases; for it sometimes happens that a number of errors or actions, whose operations have a contrary tendency, so compensate each other, that their effects when united are not sensible. But when this is the case, if any one of the causes of error is removed, those which remain will be detected.

When any given charge is loaded with a heavy bullet, more of the powder is inflamed in a very short space of time than when the bullet is lighter, and the action of the powder ought of course to be greater on that account; but then a heavy bullet takes up more time in passing through the bore than a light one, and consequently more of the elastic fluid, generated from the powder, escapes by the vent and by windage. It may happen then, that the augmentation of the force, on account of one of these circumstances, may exactly counterbalance the diminution of it arising from the other; and if it should be found on trial that this is the case in general, in pieces as they are now constructed, and with all the variety of shot that are used in practice, it would be of great use to know the fact: and possibly it might answer as well, as far as it relates to the art of gunnery, as if we were perfectly acquainted with, and were able to appreciate, the effect of each varying circumstance under which an experiment can be made. But when, concluding too hastily from the result of a partial experiment, we suppose with Mr. Robins, that because the sum total of the action or pressure of the elastic fluid on the bullet, during the time of its passage through the bore, happens to be the same when bullets of different weights are used (which collective pressure is in all cases proportional to, and is accurately measured by, the velocity, or rather motion, communicated to the bullet) that therefore the pressure in any given part is always exactly the same, when the quantity of powder is the same with which the piece is fired; and thence endeavour to prove, that the inflammation of gunpowder is instantaneous, or that the whole charge is in all cases inflamed, and “converted into an elastic fluid before the bullet is



sensibly moved from its place;" such reasonings and conclusions may lead to very dangerous errors.

It is undoubtedly true, that if the principles assumed by Mr. Robins with respect to the manner in which gunpowder takes fire, and the relation of the elasticity of the generated fluid to its density, or the intensity of its pressure on the bullet as it expands in the barrel, were just; and if the loss of force by the vent and windage was in all cases inconsiderable, or if it was prevented, the deductions from the theory respecting the velocities of bullets of different weights would always hold good. But if, on the contrary, it should be found, on making the experiments carefully, and in such a manner as entirely to prevent inaccuracies arising from adventitious circumstances, that the velocities observe a law different from that which the theory supposes, we may fairly conclude that the principles, on which the theory is founded, are erroneous.

Mr. T. now makes a comparison of this relation, by means of the experiments from N<sup>o</sup> 84 to 92 inclusive, and finds that the ratio above-mentioned does not generally hold good. But it may be justly objected to this comparison, that those very experiments were very bad ones, being condemned as such by Mr. T. himself in several parts of his paper; and that besides, he makes the calculation from the velocities as deduced from the recoil of the gun, instead of those from the pendulum, which have in this instance a very different relation.

Mr. T. proceeds. There are many reasons to suppose, that the diminution of the action of the powder on the bullet, when it is lighter, is not so much owing to the smallness of the quantity of powder that takes fire in that case, as to the vis inertię of the generated fluid. It is true, that a greater portion of the charge takes fire when the bullet is heavy than when it is light; but then the quantity of unfired powder in any case was much too small to account for the apparent diminution of the force when light bullets were used. If the elastic fluid, in the action of which the force of powder consists, were infinitely fine, or if its weight bore no proportion to that of the powder that generated it; and if the gross matter, or caput mortuum, of the powder remained in the bottom of the bore after the explosion, then, and on no other supposition, would the pressure on the bullet be inversely as the space occupied by the fluid: but it is evident that this can never be the case.

A curious subject for speculation here occurs: how far would it be advantageous, were it possible, to diminish the specific gravity of gunpowder, and the fluid generated from it, without lessening its elastic force? It would certainly act on very light bullets with greater force; but when heavy ones came to be used, there is reason to think that, except extraordinary precaution was taken to prevent it, the greatest part of the force would be lost by the vent and by windage. The velocity with which elastic fluids rush into a void space, is as the



elasticity of the fluid directly, and inversely as its density; if therefore the density of the fluid generated from powder was 4 times less than it is, its elasticity remaining the same, it would issue out at the vent, and escape by the side of the bullet in the bore, with nearly 4 times as great a velocity as it does at present; but we know from experiment that the loss of force on these accounts is now very considerable.

An elastic bow, made of very light wood, will throw an arrow, and especially a light one, with greater velocity than a bow of steel of the same degree of stiffness: but, for practice, gunpowder may be supposed to be so light as to be rendered entirely useless: and for some purposes it seems probable, that it would not be the worse for being even heavier than it is now made. Vents are absolutely necessary in fire-arms, and in large pieces the windage must be considerable, in order that the bullets, which are not always so round as they should be, may not stick in the bore; and those who have been present at the firing of heavy artillery and large mortars with shot and shells, must have observed, that there is a sensible space of time elapses between the lighting of the prime and the explosion; and that, during that interval, the flame is continually issuing out at the vent with a hissing noise, and with a prodigious velocity, as appears by the height to which the stream of fire mounts up in the air.

As it appears from these experiments, says Mr. T. that the relation of the velocities of bullets to their weights is different from that which Mr. Robins's theory supposes, it remains to inquire what the law is which actually obtains. And first, as the velocities bear a greater proportion to each other than the reciprocal sub-duplicate ratio of the weights of the bullets, Mr. T. examines how near they come to the reciprocal sub-triplicate ratio of their weights: and here the velocities computed on the last supposition appear to agree rather better with the experiments than those computed on Mr. Robins's principles; but still there is a considerable difference between the actual and the computed velocities in the 3 last experiments in the table, which it has been observed were the erroneous ones.

As the powder itself is heavy, it may be considered as a weight that is put in motion along with the bullet; and if we suppose the density of the generated fluid is always uniform from the bullet to the breech, the velocity of the centre of gravity of the powder, or, which amounts to the same thing, of the elastic fluid, and the gross matter generated from it, will be just half as great as the velocity of the bullet. If therefore we put  $P$  to denote the weight of the powder,  $B$  the weight of the bullet, and  $v$  its initial velocity; then  $Bv + \frac{1}{2}Pv = (B + \frac{1}{2}P) \times v$  will express the momentum of the charge at the instant when the bullet quits the bore. If now, instead of ascertaining the relation of the velocities to the weights of the bullets, we add half the weight of the powder to the



weight of the bullet, and compute the velocities from the reciprocal sub-triplicate ratio of the quantity  $(B + \frac{1}{6}P)$  in each experiment, there then results a pretty near agreement between the actual and computed velocities in 5 out of the 8 experiments compared; but in the other same 3 faulty ones, as before, the difference is still very great.

*Of an attempt to determine the explosive force of aurum fulminans, or a comparison between its force and that of gunpowder.*—Mr. T. having provided himself with a small quantity of this wonderful powder, he endeavoured to ascertain its explosive force by making use of it instead of gunpowder for discharging a bullet, and measuring, by means of the pendulum, the velocity which the bullet acquired; and concluding, from the tremendous report with which this substance explodes, that its elastic force was vastly greater than that of gunpowder, he took care to have a barrel provided of uncommon strength, on purpose for the experiment. Its length in the bore was 13.25 inches, the diameter of the bore 0.55 of an inch, and its weight 7 lbs. 2 oz. This barrel being charged with  $\frac{1}{16}$  of an ounce ( $= 27.34$  grains) of aurum fulminans and 2 leaden bullets, which, together with the leather that was put about them to make them fit the bore without windage, weighed 427 grains; it was laid upon a chafing-dish of live-coals, at the distance of about 10 feet from the pendulum, and against the centre of the target of the pendulum the piece was directed. After some minutes the powder exploded, but with a report infinitely less than what was expected, the noise not greatly exceeding the report of a well-charged wind-gun. The bullets struck the pendulum nearly in the centre of the target, and both of them remained in the wood: and Mr. T. found, on making the calculation, that they had impinged against it with a velocity of 428 feet in a second.

If we now suppose that the force of aurum fulminans arises from the action of an elastic fluid generated from it in the moment of its explosion, and that the elasticity of this fluid, or rather the force it exerts on the bullet as it goes on to expand, is always as its density, or inversely as the space it occupies; then, from the known dimensions of the barrel, the length of the space occupied by the charge (which in this experiment was 0.47 of an inch), and the weight and velocity of the bullets, the elastic force of this fluid at the instant of its generation may be determined: and on making the calculation on these principles, Mr. T. found that its force was 307 times greater than the mean elastic force of common air; and consequently was but about the 5th or 6th of the strength of gunpowder.

*Of the specific gravity of gunpowder.*—To determine the specific gravity of gunpowder, Mr. T. used the following method. A large glass bucket, with a narrow mouth, being suspended to one of the arms of a very nice balance, and



exactly counterpoised by weights put in the opposite scale, it was filled first with government powder poured in lightly, then with the same powder shaken well together, afterwards with powder and water together, and lastly with water alone, and in each case the contents of the bucket were very exactly weighed. The specific gravity of gunpowder, as determined from these experiments, is as follows :

Specific gravity of rain water . . . . .	1.000
Government powder, as it lies light in a heap, mixed with air. .	0.836
Government powder well shaken together . . . . .	0.937
The solid substance of the powder. . . . .	1.745

Hence it appears, that a cubic inch of government powder, shaken well together, weighs just 243 grains; that a cubic inch of solid powder would weigh 442 grains; and consequently that the interstices between the particles of the powder, as it is grained for use, are nearly as great as the spaces which those particles occupy.

#### MISCELLANEOUS EXPERIMENTS.

*Of some unsuccessful attempts to increase the force of gunpowder.*—It has been supposed by many, that the force of steam is even greater than that of gunpowder; and that if a quantity of water, confined in the chamber of a gun, could at once be rarefied into steam, it would impel a bullet with prodigious velocity. Several attempts have been made to shoot bullets in this manner; but Mr. T. knew of none that had succeeded; at least so far as to render it probable that water can ever be substituted instead of gunpowder for military purposes, as some have imagined. The great difficulty that attends making these experiments lies in finding out a method by which the water can at once be rarefied, and converted into elastic steam. Mr. T. contrived a method for this purpose, which was by filling with water the thin air-bladders of very small fishes, and inclosing them in the middle of cartridges of gunpowder, and then firing them; but he constantly found, that the force of the charge was very sensibly diminished by the addition of the globule of water, and the larger the quantity of water was that was thus confined, the less was the effect of the charge; neither the recoil of the pistol, nor the penetration of the bullet, being near equal to what they were when the given quantity of powder was fired without the water; and the report of the explosion appeared to be lessened in a still greater proportion than the recoil or penetration.

Concluding that this diminution of the force of the charge arose from the bursting of the little bladder, and the dispersion of the water among the powder before it was all inflamed, by which a great part of it was prevented from taking fire, Mr. T. repeated the experiments with highly rectified spirits of wine instead of water; but the result was nearly the same as before: the force of the charge



was constantly and very sensibly diminished. He afterwards made use of ætherial oil of turpentine, and then of small quantities of quicksilver; but still with no better success. Every thing he mixed with the powder, instead of increasing, served to lessen the force of the charge. Common pulvis fulminans is made of 1 part of sulphur, 2 parts of salt of tartar, and 3 parts of nitre; and if we may judge by the report of the explosion, the elastic force of this compound is considerably greater than that of gunpowder. Mr. T. tried the effect of mixing salt of tartar with gunpowder; having provided some of this alkaline salt in its purest state, thoroughly dry, and in a fine powder, he mixed 20 grains of it with 145 grains of gunpowder; and on discharging a bullet with the mixture, he found that the alkaline salt had considerably lessened the force of the powder. Mr. T. next made use of sal ammoniacum. That salt has been found to produce a very large quantity of elastic air, or vapour, when exposed to heat under certain circumstances; but when 20 grains of it were mixed with a charge of gunpowder, instead of adding to its force, it diminished it very sensibly.

Most, if not all, the metals, are thought to produce large quantities of air when they are dissolved in proper menstrua, and particularly brass, when it is dissolved in spirit of nitre. Desirous of seeing if this could be done by the flame, or acid vapour of fired powder, Mr. T. mixed 20 grains of brass in a very fine powder, commonly called brass dust, with 145 grains of powder, and with this compound and a fit bullet he loaded the barrel and discharged it; but the experiment showed, that the force of the powder was not increased by the addition of the brass dust, but the contrary. It seems probable however, that neither brass dust nor æthiops mineral are of themselves capable of diminishing the force of gunpowder in any considerable degree, otherwise than by filling up the interstices between the grains, and obstructing the passage of the flame, and so impeding the progress of the inflammation. And hence it appears, how earthy particles and impurities of all kinds are so very detrimental to gunpowder. It is not that they destroy or alter the properties of any of the bodies of which the powder is composed, but simply, that by obstructing the progress of the inflammation, they lessen its force, and render it of little or no value. Too much care therefore cannot be taken, in manufacturing powder, to free the materials from all heterogeneous matter.

*Of an attempt to shoot flame instead of bullets.*—Having often observed paper and other light bodies to come out of great guns and small arms inflamed, Mr. T. was led to try if other inflammable bodies might not be set on fire in like manner, and particularly inflammable fluids; and he thought if this could be effected, it might be possible to project such ignited bodies by the force of the explosion, and by that means communicate the fire to other bodies at some considerable distance; but in this attempt he failed totally. Mr. T. never could set



dry tow on fire at the distance of 5 yards from the muzzle of the barrel. He repeatedly discharged large wads of tow and paper, thoroughly soaked in the most inflammable fluids, such as alkohol, ætherial spirit of turpentine, balsam of sulphur, &c. ; but none of them were ever set on fire by the explosion. Sometimes he discharged 3 or 4 spoonfuls of the inflammable fluid, by interposing a very thin wad of cork over the powder, and another over the fluid; but still with no better success. The fluid was projected against the wall as before, and left a mark where it hit; but it never could be made to take fire; so he gave up the attempt. If it had succeeded, probably it would have turned out one of the most important discoveries in the art of war that have been made since the invention of gunpowder.

*XVI. A Luminous Appearance in the Heavens. By T. Cavallo, F.R.S. p. 329.*

At about half past 9, March 27, 1781, a white light began to be seen in the sky, which became gradually more and more dense till 10 o'clock, at which time it formed a complete luminous arch from east to west. At that time it appeared to be an arch of about 7 or 8 degrees in breadth, extended nearly from east to west. Its western part quite reached the horizon; but the eastern part of the arch seemed to begin at about  $50^{\circ}$  or  $60^{\circ}$  above the horizon. It did not pass through the zenith, but at about  $8^{\circ}$  or  $10^{\circ}$  southward of it, and it was nearly perpendicular to the horizon.

The whiteness of this arch was much denser than that of any aurora borealis, though it did not cast so much light on terrestrial objects. Towards the middle it was so dense, that the stars over which it passed were eclipsed; but the sides of this luminous arch were more faint and transparent. At about  $\frac{3}{4}$  past 10 it began to lose its brightness, and then vanished gradually, so that at 11 o'clock none of it could be perceived. As soon as any part of this arch lost its dense whiteness, the stars appeared through it quite distinct; so that it could not be a cloud. The light also seemed to vanish without change of place; for it did not appear to be dispersed through the sky, or to be driven in any direction. This extraordinary appearance seemed quite distinct from the aurora borealis, for the following reasons; viz. because it eclipsed the stars over which it passed; because its light, or rather its white appearance, was stationary and not lambent; and because its direction was from east to west. The atmosphere was in other respects very serene, the stars shining very bright, and no cloud appearing. The northern light was exceedingly faint, and very low about the northern point of the horizon. The wind was nearly north-east, and it could be just felt in the streets.



*XVII. Account of an Earthquake at Hafodunos near Denbigh. By John Lloyd, Esq., F. R. S. p. 331.*

August 29, 1781, at 8<sup>h</sup> 37<sup>m</sup> 30<sup>s</sup>, as Mr. L. was sitting on his bed-side, he heard a rumbling noise, as if at a distance: the sound seemed to approach, and when it was greatest the bed rocked and shook so much that he could scarcely keep his seat. The barometer had been stationary nearly for the 3 preceding days, and did not seem to be affected with the shock. The morning was remarkably fine, and not a single cloud to be seen. Two of his sisters and a gentleman were walking on the terrace in the garden by the side of a wall: they all perceived the noise, at first as if at a great distance; but when it was greatest they perceived the wall to shake, though they did not observe any agitation under their feet. It continued from 15 to 18 minutes; and its course was nearly from south-east to north-west. Some other persons in the house perceived a double shock; and this was observed by many who felt it in other places. It was felt at most other parts in Wales. And 2 other shocks were afterwards felt the same year in Wales.

*XVIII. On the Heat of the Water in the Gulf-Stream. By Chas. Blagden, M. D., F. R. S. p. 334.*

One of the most remarkable facts observed in navigating the ocean, is that constant and rapid current which sets along the coast of North America to the northward and eastward, and is commonly known to seamen by the name of the gulf-stream. It seems justly attributed to the effect of the trade-winds, which, blowing from the eastern quarter into the great Gulf of Mexico, cause there an accumulation above the common level of the sea; in consequence of which, it is constantly running out by the channel where it finds least resistance, that is, through the Gulf of Florida, with such force as to continue a distinct stream to a very great distance. Since all ships going from Europe to any of the southern provinces of North America must cross this current, and are materially affected by it in their course, every circumstance of its motion becomes an object highly interesting to the seaman, as well as of great curiosity to the philosopher.

During a voyage to America in the spring of the year 1776, Dr. B. used frequently to examine the heat of sea-water newly drawn, in order to compare it with that of the air. The passage was made far to the southward. In this situation, the greatest heat of the water which he observed was such as raised the quicksilver in Fahrenheit's thermometer to 77½. This happened twice; the first time on the 10th of April, in latitude 21° 10' N. and longitude by reckoning 52° W.; and the 2d time, 3 days afterwards, in latitude 22° 7' and longitude 55°; but in general the heat of the sea near the tropic of Cancer about the middle of April was from 76° to 77°.



The rendezvous appointed for the fleet being off Cape Fear, their course, on approaching the American coast, became north-westward. On the 23d of April the heat of the sea was  $74^{\circ}$ , the latitude at noon  $28^{\circ} 7' N$ . Next day the heat was only  $71^{\circ}$ , then in latitude  $29^{\circ} 12'$ ; the heat of the water, therefore, was now lessening very fast in proportion to the change of latitude. The 25th the latitude was  $31^{\circ} 3'$ ; but though they had thus gone almost  $2^{\circ}$  farther to the northward, the heat of the sea was this day rather increased, it being  $72^{\circ}$  in the morning, and  $72^{\circ}\frac{1}{2}$  in the evening. Next day, the 26th of April, at half after 8 in the morning, the thermometer rose to  $78^{\circ}$ ; higher than he had ever observed it, even within the tropic. As the difference was too great to be imputed to any accidental variation, Dr. B. immediately conceived that they must have come into the gulf-stream, the water of which still retained great part of the heat that it had acquired in the torrid zone. This idea was confirmed by the subsequent regular and quick diminution of the heat: the ship's run for a quarter of an hour had lessened it  $2^{\circ}$ ; the thermometer at  $8\frac{3}{4}^h$  being raised by sea-water fresh drawn only to  $76^{\circ}$ ; by 9 the heat was reduced to  $73^{\circ}$ , and in  $\frac{1}{4}$  of an hour more, to  $71^{\circ}$  nearly: all this time the wind blew fresh, and they were going 7 knots an hour on a north-western course. The water now began to lose the fine transparent blue colour of the ocean, and to assume something of a greenish olive tinge, a well-known indication of soundings. Accordingly, between 4 and 5 in the afternoon ground was struck with the lead at the depth of 80 fathoms, the heat of the sea being then reduced to  $69^{\circ}$ . In the course of the following night and next day, as they came into shallower water and nearer the land, the temperature of the sea gradually sunk to  $65^{\circ}$ , which was nearly that of the air at the time.

Bad weather on the 26th prevented them from taking an observation of the sun; but on the 27th, though it was then cloudy at noon, they calculated the latitude from 2 altitudes, and found it to be  $33^{\circ} 26' N$ . The difference of this latitude from that which was observed on the 25th, being  $2^{\circ} 23'$ , was so much greater than could be deduced from the ship's run marked in the log-book, as to convince the seamen that they had been set many miles to the northward by the current.

From these observations, Dr. B. thinks it may be concluded, that the gulf-stream, about the 33d degree of north latitude, and the 76th degree of longitude west of Greenwich, is, in the month of April, at least 6 degrees hotter than the water of the sea through which it runs. As the heat of the sea-water evidently began to increase in the evening of the 25th, and as the observations show that they were getting out of the current when he first tried the heat in the morning of the 26th, it is most probable that the ship's run during the night is nearly the breadth of the stream measured obliquely across; that, as it blew a



fresh breeze, could not be much less than 25 leagues in 15 hours, the distance of time between the two observations of the heat, and hence the breadth of the stream may be estimated at 20 leagues. The breadth of the Gulf of Florida, which evidently bounds the stream at its origin, appears by the charts to be 2 or 3 miles less than this, excluding the rocks and sand-banks which surround the Bahama Islands, and the shallow water that extends to a considerable distance from the coast of Florida; and the correspondence of these measures is very remarkable, since the stream, from well-known principles of hydraulics, must gradually become wider as it gets to a greater distance from the channel by which it issues.

*XIX. On the Appearance of the Soil at opening a Well at Hanby, Lincolnshire.*  
*By Sir Henry C. Englefield, Bart., F. R. and A. S. p. 345.*

The spot on which the well was sunk is nearly on a level with Lincoln Heath, and of course high ground compared with the fen, which is distant from it above 6 miles. The soil was uniformly a blue clay, in parts rather inclining to a shaly structure, and contained many casts of tellinæ, a very little pyrites, and some few small, but very elegant belemnites. These are all the usual fossils of clay; but what Sir H. thinks without example is, that through the whole mass of clay were interspersed nodules of pure chalk, evidently rounded by long attrition, and of all sizes from that of a pea to a child's head. They lay in no sort of order that he could find. How deep this appearance might have continued he could not determine, but no water having been found at the depth of 30 feet, the trial was given up, as the expense would have exceeded the advantage proposed. In all the environs there is not the least trace of chalk in any form whatever that he could discover or hear of.

*XX. Astronomical Observations, by Nath. Pigott, Esq., F. R. S. p. 347.*

In 1778 and 1779 Mr. P. observed in Glamorganshire; and by 35 meridian observations of the sun and stars, all agreeing within  $12^s$  from the mean, he determined the latitude of his observatory at Frampton House  $51^{\circ} 25' 1''$  N. And its longitude, by comparison of many observations of the eclipses of Jupiter's satellites, was  $3^{\circ} 29' 30''$  west of Greenwich. Frampton House lies between Cowbridge and Lantwit; about 4 miles south of the former, and 1 mile north of the latter, and about 2 miles from the Bristol channel; and is nearly under the same meridian as Watchet, a market town in Somersetshire.

In the beginning of 1778, the declination west of a magnetic needle of 4 inches, made by Mr. Dollond, appeared to be  $22^{\circ} 11'$ .



*XXI. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1780. By Thomas Barker, Esq. p. 351.*

		Barometer.			Thermometer.						Rain.	Mean rain.	
		Highest.	Lowest.	Mean.	In the House.			Abroad.				10 yrs.	45 yrs.
					Hig.	Low	Mean	Hig.	Low	Mean		71—80	36—80
Jan.	Morn.	29.89	28.16	29.39	39	23	34	36 $\frac{1}{2}$	15 $\frac{1}{2}$	27	1.013	1.677	1.573
	Aftern.				40	29	35	43	22 $\frac{1}{2}$	32			
Feb.	Morn.	30.06	28.72	29.49	44	33	37	41	22	31	1.572	1.871	1.378
	Aftern.				44 $\frac{1}{2}$	34	38	46	30 $\frac{1}{2}$	39			
Mar.	Morn.	29.94	28.99	29.47	51 $\frac{1}{2}$	41 $\frac{1}{2}$	46	49	30	40	1.175	1.355	1.315
	Aftern.				53 $\frac{1}{2}$	43 $\frac{1}{2}$	47 $\frac{1}{2}$	61	45	51			
Apr.	Morn.	29.77	28.40	29.21	53	40	46	52	29 $\frac{1}{2}$	38	2.727	1.314	1.465
	Aftern.				55	41 $\frac{1}{2}$	46	60	38	48			
May	Morn.	29.83	28.80	29.48	67	51	55	65	40 $\frac{1}{2}$	50 $\frac{1}{2}$	1.201	2.081	1.610
	Aftern.				72	52 $\frac{1}{2}$	57	80 $\frac{1}{2}$	53	61			
June	Morn.	29.89	29.26	29.55	65 $\frac{1}{2}$	52	57 $\frac{1}{2}$	62 $\frac{1}{2}$	43	53 $\frac{1}{2}$	1.920	2.374	2.249
	Aftern.				71 $\frac{1}{2}$	52 $\frac{1}{2}$	59	80	51	64			
July	Morn.	29.87	29.21	29.61	69 $\frac{1}{2}$	58 $\frac{1}{2}$	63	65	51 $\frac{1}{2}$	57 $\frac{1}{2}$	1.566	2.507	2.516
	Aftern.				71 $\frac{1}{2}$	59 $\frac{1}{2}$	64	83	61	69 $\frac{1}{2}$			
Aug.	Morn.	29.81	29.43	29.65	68	61 $\frac{1}{2}$	64	63	52	58	0.432	2.468	2.247
	Aftern.				72	63	66	81 $\frac{1}{2}$	63	70			
Sept.	Morn.	29.79	28.70	29.38	70	53	61	61	43	52	3.427	3.142	2.016
	Aftern.				73 $\frac{1}{2}$	53 $\frac{1}{2}$	63	82	53 $\frac{1}{2}$	65			
Oct.	Morn.	29.87	28.20	29.23	58 $\frac{1}{2}$	46 $\frac{1}{2}$	51	56	33 $\frac{1}{2}$	44	3.080	2.975	2.158
	Aftern.				59	48	52	64	45 $\frac{1}{2}$	53			
Nov.	Morn.	30.00	28.62	29.41	50	38 $\frac{1}{2}$	43	48 $\frac{1}{2}$	19 $\frac{1}{2}$	36	1.461	2.372	1.943
	Aftern.				50 $\frac{1}{2}$	38 $\frac{1}{2}$	43 $\frac{1}{2}$	48 $\frac{1}{2}$	31	40			
Dec.	Morn.	30.08	29.22	29.83	45	35	40	46 $\frac{1}{2}$	26	35	0.534	1.889	1.740
	Aftern.				45 $\frac{1}{2}$	35	40 $\frac{1}{2}$	50	30 $\frac{1}{2}$	38 $\frac{1}{2}$			
Mean of all .....				29.48	50			48			20.108	26.024	22.210

*XXII. Some Calculations of the Number of Accidents or Deaths which happen in consequence of Parturition; and of the Proportion of Male to Female Children, as well as of Twins, monstrous Productions, and Children dead-born; taken from the Midwifery Reports of the Westminster General Dispensary: with an Attempt to ascertain the Chance of Life at different Periods, from Infancy to Twenty-six Years of Age; also the Proportion of Natives to the rest of the Inhabitants of London. By Robt. Bland, M. D., of the Westminster General Dispensary. p. 355.*

As Dr. B.'s first view was to find the proportion of difficult labours, and of the accidents or deaths that happen in consequence of child-birth, he began as follows:

Of 1897 women delivered under the care of the Dispensary, 63, or 1 in 30, had unnatural labours: in 18 of these, or 1 in 105, the children presented by their feet; in 36, or 1 in 52, the breech presented; in 8 the arms presented; and in 1 the funis.



Again, 17 women, or 1 in 111, had laborious labours: in 8 of these, or 1 in 236, the heads of the children were lessened; in 4 a single blade of a forceps was used; and in the remaining 5, in which the faces of the children were turned to the pubes, the delivery was at length accomplished by the pains.

3dly. 1 woman had convulsions about the 7th month of her pregnancy, and was delivered a month after of a dead child, and recovered. 1 woman had convulsions during labour; brought forth a live child, and recovered: 9 women, or 1 in 210, had uterine hæmorrhage before and during labour. Of these 1 died undelivered; 1 died a few hours, and 1 ten days, after delivery, and 6 recovered.

4thly. 5 women had the puerperal fever, of whom 4 died. In one of these the placenta was undelivered, and continued so to her death: 2 women were seized with mania, but recovered in about 3 months. In 1 woman a suppuration took place, soon after labour, from the vagina into the bladder and rectum. This patient recovered, but the urine and stools continue to pass through the wounds. Of 1 woman the perinæum was lacerated to the sphincter ani. A suture was attempted, but without effect; she recovered, but is troubled with prolapsus uteri: 5 had large and painful swellings of the legs and thighs, but recovered.

105 therefore of these, or 1 in 18, had preternatural or laborious births, or suffered in consequence of labour. Of this number of cases, 43, or 1 in 44, were attended with particular difficulty or danger; and 7 only, or 1 in 270, died. The remaining 62 were delivered and recovered with little more than the common assistance: and 1792 had natural labours, not attended with any particular accidents.

The proportion of male to female children, of the number of twins, and of the children that were deficient or monstrous, and of those that were dead-born, is as follows:

1897 women were delivered of 1923 children; 972 boys, and 951 girls, or as 46 boys to 45 girls.—23 of the women, or 1 in 80, were delivered of twins, 16 of whom were boys and 30 girls,—1 woman was delivered of 3 girls. Of the twins and triplets therefore, the males were only half the number of the females.—8 of the children, or 1 in 241, were deficient or monstrous. Of these, 1 was web-fingered; 1 had a hare-lip; 1 had a dropsical head and distorted spine; 1 a dropsical head; in 1 a part of the palate, and in 2 a considerable portion of the cranium was wanting; one of these lived an hour after it was born; and 1 had 2 heads;\* 1 woman was delivered of a monstrous twin.†—84 of the children,

\* It had 2 heads and necks, 4 hands and arms, 2 spines, uniting at the sacrum, and terminating in one pelvis, whence the lower extremities proceeded single; there was one navel-string, and one male organ of generation. On opening the body there were found, 2 thoracic cavities, the right more complete than the left; the heart also, and the lungs on the right side, were more perfect than



or 1 in 23 of the whole number, were dead-born.\* Of these, 49, or nearly  $\frac{1}{2}$ , were boys, and 35 were girls.

Of 1400 women who returned their letters, or of whom a certain account could be obtained, 85, or nearly 1 in 16, had buried their children before the end of 2 months. Of this number 53, or 5 in 8, were boys, and 32 girls. This singular circumstance of there being a greater number of males than females, among the still-born children, and of a greater number of male children dying in infancy than of females, has been remarked by Dr. Price and other writers on calculations; and Dr. Haygarth has shown, that at Chester more husbands die in a given period than wives. This naturally suggests an inquiry, whether the lives of males are at all ages more precarious than those of females.

To be enabled to assist in answering this question, I add, says Dr. B., the following article to my register, viz. of the children that shall be living when the women apply for their letters, how many will be boys, and how many girls?

*Table of the ages at which women begin and cease to be capable of bearing children, and of the intermediate periods at which they are most so.*

Of 2102 pregnant women,	Years of age.	
36 or 1 in 58 were from.....	15 to 19	} 85, or 1 in 25, from 15 to 20 inclusive.
49 or 1 in 43 were .....	20	
578 or 5 in 19 were from.....	21 to 25	} 1684, or four-fifths were from 21 to 35 inclusive.
699 nearly 1 in 3 were from.....	26 to 30	
407 nearly 1 in 5 were from.....	31 to 35	
291 or 3 in 22 were from.....	36 to 40	} 42, or 1 in 50, from 41 to 49.
36 or 1 in 58 were from .....	41 to 45	
6 or 1 in 350 were from .....	46 to 49	
2102		

those on the left, which latter were very small. There were 2 stomachs, 2 sets of intestines, which, at length uniting, terminated in one rectum and anus. There was but one urinary bladder.—Orig.

† Of this singular production, to which Dr. B. had not ventured to give a name, the following is the history and description. The woman who produced it was about 27 years of age; this was her first pregnancy. She was, after a natural labour, delivered of a female foetus, and its placenta, in which nothing uncommon was observed; and though the uterus remained of an unusual size, yet the pains not recommencing, there was no suspicion entertained but that its bulk was occasioned by coagulated blood. On the 3d day the pains became violent, and this monster was born. Its shape was spherical, but somewhat flattened. It measured in its largest diameter 5 inches, and weighed about 18 ounces. It received its nourishment by an umbilical chord, to which was attached a portion of membranes, and though no placenta was found, it is probable it had a small one, and that it was inclosed in its own involucre. It was completely covered with a cuticula, and a little above the part where the navel-string terminated, there was a hairy scalp covering a bony prominence, somewhat resembling the arch of the cranium. On dissection it was found to be plentifully supplied with blood-vessels, proceeding from the navel-string, and branching through every part of it. It had a small brain and medulla spinalis continued into a bony theca, with nerves passing from thence through the foramina of the bones; but no resemblance of any thoracic or abdominal viscera. The rest of its bulk was made up of fat.—Orig.

\* By dead-born children Dr. B. means those that die after they have been perceived to move, that is, generally after 4 months. Abortions, or deaths before that period, may reasonably be estimated at double this number; so that perhaps 1 child in 8 dies in the womb, or in the act of coming into the world.—Orig.



*Tables of the number of children born by 1389\* women, with the number that were living at the time of their applying to the Dispensary.*

Women.	No. of children born by each woman.	Total of child. born.	Total of child. living.	No. of women who had preserved their children.	No. of children preserved by each woman.	Total of child. preserved.
1	24	24	5	....	....	....
1	17	17	3	....	....	....
3	16	48	5	....	....	....
2	14	28	11	....	....	....
11	13	155	46	....	....	....
14	12	168	44	....	....	....
15	11	165	45	1	11	11
22	10	220	84	....	....	....
33	9	297	93	....	....	....
56	8	448	151	4	8	32
74	7	518	213	3	7	21
89	6	534	214	11	6	66
138	5	690	288	32	5	160
169	4	676	293	84	4	336
208	3	624	299	174	3	522
254	2	508	259	306	2	612
299	1	299	171	464	1	464
1389 and 370 were in their first pregnancy.		†5419	2224	1079 and 310 had lost all their children.		2224
1389				1389		

I have placed (says Dr. B.) these 2 tables together, that we might have an opportunity of observing how exceedingly fertile the women of the poorer classes in this country are; and at the same time how unable to rear any considerable number of children; for, though 321 of the women had borne 6 children and upwards each, and were all again pregnant, 19 only of them had been able to rear 6 or more children; and though 102 of the women had borne 9 children and upwards each, only 1 of them had been able to preserve that number living. I am inclined to believe, that this great mortality among the children does not arise from any natural imbecility or a constitution vitiated from the birth, many of those victims being born with all the appearances of health and vigour; but that we ought rather to search for the cause of it in the poverty of the parents,

\* In order to account for the difference between the number of the women in these and the preceding tables, it is proper to mention, that this account was not begun until some months after the former one. In these also care has been taken that no woman is reckoned more than once, though many of them had been assisted by the midwives to the Dispensary 2, 3, or 4 times; 370, as noted in the table, were in their first pregnancy.—Orig.

† Of these 5419 children, 2747 were boys, and 2672 girls, or nearly as 36 boys to 35 girls. This proportion of the boys to the girls will be found a little different from what is given in a former table.—Orig.

which prevents their taking the necessary care of, or even affording sufficient clothing and nourishment to, their offspring.

I shall now from these tables attempt to collect what the chance of life is at different periods, from infancy to 26 years of age; but, that I may be understood, it will be necessary to premise some account of the method I have followed. I have supposed each of the women to bear a child every 2 years; this, from the account of those who returned to the Dispensary a 2d, 3d, or 4th time, appearing to be the mean term. On this principle, when I find that a woman applied at the Dispensary who had had 1 child before, I conclude, that that child would be 2 years old, if living; but if the woman had borne 2 children, I suppose that the first would be 4, the second 2 years old, and so on. And finding, that of 299 children born by as many women, who were now advanced in their 2d pregnancy, 171, or  $\frac{7}{12}$  only were living, I conclude, that on an average 5 out of 12 die under 2 years of age: and observing that of 508 children born by 254 women, who were now advanced in their 3d pregnancy, 259 only were living, I first deduct 210, which is  $\frac{5}{12}$  of the whole number, who died under 2 years of age; and then find that 39, which is nearly  $\frac{1}{12}$  of the whole number, or  $\frac{1}{7}$  of the survivors, died between 2 and 4 years of age.

*Table of the Chance of Life from Infancy to 26 Years of Age.*

Age.	Persons living.	Decrease of life.	
0	5400	2250	5 in 12
2	3150	450	6 in 12, or 1 in 7 of the survivors
4	2700	180	8 in 15, or 1 in 15 of the survivors
6	2520	204	4 in 7, or 1 in $12\frac{1}{2}$ of the survivors
8	2313	156	6 in 10, or 1 in 15 of the survivors
10	2160	540	7 in 10, or 1 in 4 of the survivors
26	1620		

3780 or  $\frac{7}{10}$  would die.

1620. or  $\frac{3}{10}$  would be living at the end of 26 years.

5400

A comparative table of the population of London, with a view to show the proportion of natives to persons born in the different counties of England and Wales, in Scotland, Ireland, or foreign countries.

Of 3236 married persons, 824, or  $\frac{1}{4}$ , were born in London; 1870, or  $\frac{4}{7}$ , in the different counties of England and Wales; 209, or 1 in 15, in Scotland; 280, or 1 in 11, in Ireland; 53, or 1 in 60, were foreigners.

Of the above number the males and females were in the following proportions.



Men.	Women.
329 were born in London, and ..	495 or 166 more than men.
952 ..... in different counties	917 or 35 fewer than men.
135 ..... in Scotland .....	74 or 61 fewer than men.
162 ..... in Ireland .....	119 or 43 fewer than men.
40 ..... were foreigners ..	13 or 27 fewer than men.
<hr/> 1618	<hr/> 1618 <hr/> 166

Thus, of 824 married persons born in London, there were  $\frac{1}{5}$  more women than men. This may be accounted for either by supposing a greater number of males to die or to migrate before they attain a marriageable age than women. It is also to be observed, that of the Scotch and of the foreigners, the women are in proportion to the men as about 1 to 3; but of the Irish they are as 3 to 7.

By this table we find at how great an expense to the country this city is maintained; and as we may suppose that the bulk of the Scotch, Irish, and foreigners, who come into the kingdom, reside in the metropolis, we hence may also learn in what proportion they contribute to repair the waste which is incurred by its excessive populousness. A more complete knowledge of these facts may give rise to regulations which, if the calculations of Dr. Price shall be found to be just, are but too necessary.

*XXIII. Account of a Child who had the Small-pox in the Womb. In a Letter from Wm. Wright, M. D., F. R. S., to John Hunter, Esq., F. R. S. p. 372.*

I have read with much pleasure and information Mrs. Ford's case, which you published in Philos. Trans., vol. 70, p. 128. From the facts you have adduced it amounts to a certainty, that her foetus had received the variolous infection in the womb. This induces me to lay before you a singular case, that fell under my care some years ago.

In 1768 the small-pox was so general in Jamaica that very few people escaped the contagion. About the middle of June Mr. Peterkin, merchant at Marthabrac, in the parish of Trelawney, got about 50 new negroes out of a ship; soon after they landed, several were taken ill of a fever, and the small-pox appeared; the others were immediately inoculated. Among the number of those who had the disease in the natural way, was a woman of about 22 years of age, and big with child. The eruptive fever was slight, and the small-pox had appeared before I saw her. They were few, distinct, and large, and she went through the disease with very little trouble, till on the 14th day from the eruption she was attacked with the fever, which lasted only a few hours. She was however the same day taken in labour, and delivered of a female child with the small-pox on her whole body, head, and extremities. They were distinct and very large, such as they commonly appear on the 8th or 9th day in favourable cases. The child was small and weakly; she could suck but little; a wet nurse was procured, and



every possible care taken of this infant, but she died the 3d day after she was born. The mother recovered. In the course of many years practice in Jamaica, I have remarked, that where pregnant women had been seized with the natural small-pox, or been by mistake inoculated, that they generally miscarried in the time of, or soon after, the eruptive fever; but I never saw any signs of small-pox on any of their bodies, except on the child's above-mentioned.

*XXIV. Natural History of the Insect which produces the Gum Lacca. By Mr. James Kerr, of Patna. p. 374.*

**COCCUS LACCA.**—The head and trunk form one uniform, oval, compressed, red body; of the shape and magnitude of a very small louse, consisting of 12 transverse rings. The back is carinate; the belly flat; the antennæ half the length of the body, filiform, truncated and diverging, sending off 2 often 3 delicate, diverging hairs, longer than the antennæ. The mouth and eyes could not be seen with the naked eye.

The tail is a little white point, sending off 2 horizontal hairs as long as the body. It has 3 pair of limbs, half the length of the insect.

I have often observed the birth of these insects, but never could see any with wings; nor could I find any distinction of sexes, nor observe their connubial rites: nature and analogy seem to point out a deficiency in my observations, possibly owing to the minuteness of the object, and want of proper glasses.

This insect is described in that state in which it sallies forth from the womb of the parent in the months of November and December. They traverse the branches of the trees on which they are produced for some time, and then fix themselves on the succulent extremities of the young branches. By the middle of January they are all fixed in their proper situations, they appear as plump as before, but show no other marks of life. The limbs, antennæ, and setæ of the tail are no longer to be seen. Around their edges they are environed with a spissid sub-pellucid liquid, which seems to glue them to the branch: it is the gradual accumulation of this liquid, which forms a complete cell for each insect, and is what is called gum lacca. About the middle of March the cells are completely formed, and the insect is in appearance an oval, smooth, red bag, without life, about the size of a small cochineal insect, emarginated at the obtuse end, full of a beautiful red liquid. In October and November are found about 20 or 30 oval eggs, or rather young grubs, within the red fluid of the mother. When this fluid is all expended, the young insects pierce a hole through the back of their mother, and walk off one by one, leaving their exuviae behind, which is that white membranous substance found in the empty cells of the stick lac.

The insects are the inhabitants of four trees. 1. *Ficus Religiosa*, Linnæi.



In Hindostan, Pipul. Banyan tree. 2. *Ficus Indica*, Linnæi. In Hindostan, Bhur. Banyan tree. 3. Plaso, Horti Malabarici. By the natives, Praso. 4. *Rhamnus Jujuba*, Linnæi. In Hindostanic, Beyr.

The insects generally fix themselves so close together, and in such numbers, that I imagine only 1 in 6 can have room to complete her cell: the others die, and are eaten up by various insects. The extreme branches appear as if they were covered with a red dust, and their sap is so much exhausted, that they wither and produce no fruit, the leaves drop off, or turn to a dirty black colour. These insects are transplanted by birds: if they perch on these branches, they must carry off a number of the insects on their feet to the next tree they rest on. It is worth observing, that these fig trees when wounded drop a milky juice, which instantly coagulates into a viscid ropy substance, which, hardened in the open air, is similar to the cell of the coccus lacca. The natives boil this milk with oils into a bird-lime, which will catch peacocks or the largest birds.

A red medicinal gum is procured by incision from the plaso tree, so similar to the gum lacca that it may readily be taken for the same substance. Hence it is probable, that those insects have little trouble in animalizing the sap of these trees in the formation of their cells. The gum lacca is rarely seen on the *Rhamnus Jujuba*; and it is inferior to what is found on the other trees. The gum lacca of this country is principally found on the uncultivated mountains on both sides the Ganges, where nature has produced it in such abundance, that were the consumption 10 times greater, the markets might be supplied by this minute insect. The only trouble in procuring the lac is in breaking down the branches, and carrying them to market. The present price in Dacca is about 12 shillings the 100 pounds weight, though it is brought from the distant country of Assam. The best lac is of a deep red colour. If it is pale, and pierced at top, the value diminishes, because the insects have left their cells, and consequently they can be of no use as a dye or colour, but probably they are better for varnishes.

This insect and its cell have gone under the various names of Gum Lacca, Lack, Loc Tree. In Bengal, La; and by the English it is distinguished into 4 kinds. 1st. Stick lac, which is the natural state from which all the others are formed. 2d. Seed lac is the cells separated from the sticks. 3d. Lump lac is seed lac liquified by fire, and formed into cakes. 4th. Shell lac is the cells liquified, strained, and formed into thin transparent laminæ in the following manner. Separate the cells from the branches, break them into small pieces, throw them into a tub of water for one day, wash off the red water and dry the cells, and with them fill a cylindrical tube of cotton cloth, 2 feet long, and 1 or 2 inches in diameter; tie both ends, turn the bag above a charcoal fire; as the lac liquifies twist the bag, and when a sufficient quantity has transuded the pores of the cloth, lay it on a smooth junk of the plantain tree (*Musa Paradisiaca*, Linn.),



and with a slip of the plantain leaf draw it into a thin lamella; take it off while flexible, for in a minute it will be hard and brittle. The value of shell lac is according to its transparency.

This is one of the most useful insects yet discovered. The natives consume a great quantity of shell lac in making ornamental rings, painted and gilded in various tastes, to decorate the arms of the ladies; and it is formed into beads, spiral and linked chains for necklaces, and other female ornaments.

*For sealing-wax*, take a stick, and heat one end of it on a charcoal fire; put on it a few leaves of the shell lac softened above the fire; keep alternately heating and adding more shell lac, till you have got a mass of 3 or 4 pounds of liquified shell lac on the end of the stick.\* Knead this on a wetted board with 3 ounces of levigated cinnabar, form it into cylindrical pieces; and, to give them a polish, rub them while hot with a cotton cloth.

*For japanning*, take a lump of shell lac, prepared in the manner of sealing-wax, with whatever colour you please, fix it on the end of a stick, heat the polished wood over a charcoal fire, and rub it over with the half-melted lac, and polish, by rubbing it even with a piece of folded plantain leaf held in the hand; heating the lacquer, and adding more lac as occasion requires. Their figures are formed by lac, charged with various colours in the same manner.

*Varnish*.—In ornamenting their images and religious houses, &c. they make use of very thin beaten lead, which they cover with various varnishes, made of lac charged with colours. The preparation of them is kept a secret. The leaf of lead is laid on a smooth iron heated by fire below, while they spread the varnish on it.

*Grindstones*.—Take of river sand 3 parts, of seed lac washed 1 part, mix them over the fire in a pot, and form the mass into the shape of a grindstone, having a square hole in the centre; fix it on an axis with liquified lac, heat the stone moderately, and by turning the axis it may easily be formed into an exact orbicular shape. Polishing grindstones are made only of such sand as will pass easily through fine muslin, in the proportion of 2 parts sand to 1 of lac. This sand is found at Ragimaul. It is composed of small angular crystalline particles, tinged red with iron, 2 parts to 1 of black magnetic sand. The stone-cutters, instead of sand, use the powder of a very hard granite called corune. These grindstones cut very fast. When they want to increase their power they throw sand on them, or let them occasionally touch the edge of a vitrified brick. The same composition is formed on sticks, for cutting stones, shells, &c. by the hand.

*Painting*.—Take one gallon of the red liquid from the first washing for shell

\* In this manner lump lac is formed from seed lac.



lac, strain it through a cloth, and let it boil for a short time, then add half an ounce of soap earth (fossil alkali); boil an hour more, and add 3 ounces of powdered load (bark of a tree); boil a short time, let it stand all night, and strain next day. Evaporate 3 quarts of milk, without cream, to 2 quarts, on a slow fire, curdle it with sour milk, and let it stand for a day or two; then mix it with the red liquid abovementioned; strain them through a cloth, add to the mixture 1 ounce and a half of alum, and the juice of 8 or 10 lemons: mix the whole, and throw it into a cloth-bag strainer. The blood of the insect forms a coagulum with the caseous part of the milk, and remains in the bag, while a limpid acid water drains from it. The coagulum is dried in the shade, and is used as a red-colour in painting and colouring.

*Dyeing.*—Take 1 gallon of the red liquid prepared as before without milk, to which add 3 ounces of alum. Boil 3 or 4 ounces of tamarinds in a gallon of water, and strain the liquor. Mix equal parts of the red liquid and tamarind water over a brisk fire. In this mixture dip and wring the silk alternately till it has received a proper quantity of the dye. To increase the colour, increase the proportion of the red liquid, and let the silk boil a few minutes in the mixture. To make the silk hold the colour, they boil a handful of the bark called load in water, strain the decoction, and add cold water to it; dip the dyed silk into this liquor several times, and then dry it. Cotton cloths are dyed in this manner; but the dye is not so lasting as in silk.

*Spanish wool.*—The lac colour is preserved by the natives on flakes of cotton dipped repeatedly into a strong solution of the lac insect in water, and then dried.

*XXV. Account of a Phenomenon observed on the Island of Sumatra. By William Marsden, Esq. p. 383.*

In the year 1775 the s. e. or dry monsoon, set in about the middle of June, and continued with very little intermission till the month of March in the following year. So long and severe a drought had not been experienced there in the memory of the oldest man. The verdure of the ground was burnt up, the trees were stripped of their leaves, the springs of water failed, and the earth every where gaped in fissures. For some time a copious dew falling in the night supplied the deficiency of rain; but this did not last long: yet a thick fog, which rendered the neighbouring hills invisible for months together, and nearly obscured the sun, never ceased to hang over the land, and add a gloom to the prospect already but too melancholy. The Europeans on the coast suffered extremely by sickness; about a 4th part of the whole number being carried off by fevers and other bilious distempers, the depression of spirits which they laboured

under, not a little contributing to hasten the fatal effects. The natives also died in great numbers.

In the month of November that year, the dry season having then exceeded its usual period, and the s. e. winds continuing with unremitting violence, the sea was observed to be covered, to the distance of a mile, and in some places a league from shore, with fish floating on the surface. Great quantities of them were at the same time driven on the beach or left there by the tide, some quite alive, others dying, but the greatest part quite dead. The fish thus found were not of one but various species, both large and small, flat and round, the cat-fish and mullet being generally the most prevalent. The numbers were prodigious, and overspread the shore to the extent of some degrees; of this I had ocular proof or certain information, and probably they extended a considerable way farther than I had opportunity of making inquiry. Their first appearance was sudden; but though the numbers diminished, they continued to be thrown up, in some parts of the coast, for at least a month, furnishing the inhabitants with food, which, though attended with no immediate ill consequence, probably contributed to the unhealthiness so severely felt. No alteration in the weather had been remarked for many days previous to their appearance. The thermometer stood as usual at the time of year at about  $85^{\circ}$ .

Various were the conjectures formed as to the cause of this extraordinary phenomenon, and almost as various and contradictory were the consequences deduced by the natives from an omen so portentous; some inferring the continuance, and others, with equal plausibility, a relief from the drought. With respect to the cause, I must confess myself much at a loss to account for it satisfactorily. If I might hazard a conjecture, and it is not offered as any thing more, I would suppose, that the sea requires the mixture of a due proportion of fresh water to temper its saline quality, and enable certain species of fish to subsist in it. Of this salubrious correction it was deprived for an unusual space of time, not only by the want of rain, but by the ceasing of many rivers to flow into it, whose sources were dried up. I rode across the mouths of several perfectly dry, which I had often before passed in boats. The fish no longer experiencing this refreshment, necessary as it would seem to their existence, sickened and perished as in a corrupted element.



*XXVI. Further Experiments on Cold, made at the Macfarlane Observatory belonging to Glasgow College. By Patrick Wilson, M. A. p. 386.*

*Monday morning, Jan 22, 1781.*

Some days of very cold weather, lately in this country, afforded an opportunity of prosecuting a little further the experiments and observations begun in the course of last year. The frost set in on Sunday the 21st of Jan. after a considerable fall of snow on the preceding evening, and about midnight the thermometers were exposed near the observatory in the situations mentioned in my former letter. The annexed register shows the difference of temperature between the snow and the air, till 8 o'clock on Monday morning, to which are

h.	m.	Therm. in air.	Therm. in snow.
1	.....	0.....	-12
1	30.....	+2.....	-12
1	45.....	0.....	-8
2	.....	-2.....	-7
3	.....	-0.....	-4
3	45.....	-0.....	-8
4	.....	-1.....	-12
4	30.....	-3.....	-8
5	.....	-2.....	-12
6	15.....	-3.....	-13
6	45.....	-2.....	-10
7	.....	-3.....	-13
7	30.....	-2.....	-10
8	.....	-4.....	-11
8	30.....	-2.....	-10

subjoined some facts which prove very consonant to those described in the former paper. The sign — prefixed, denotes degrees below 0. The sign + degrees above 0, of Fahrenheit's thermometer.

From 1 o'clock till 3 in the morning, the thermometer in air, at the balustrade of the east wing of the observatory, pointed from + 4 to + 6, and on the snow there from - 2 to 0. At half an hour after 1 the thermometer in air, 24 feet from the ground, and to the windward of the house, pointed to + 7, and at 8 o'clock to + 1. At 3 o'clock the snow in the park, 3 inches below the surface, raised the thermometer to + 14, and at 6 inches below, near the ground, to + 24. The barometer stood at 29.8 inches, and there was a perceptible motion of the air from the east and 1 point south. This night was a very general and lively aurora borealis, most part of it of a bright red, which

formed a crown near the zenith; Monday evening.

but it mostly vanished about 3 o'clock, after which time the air became more still. During the whole of this night, as well as of the succeeding times of observing, the air was not nearly so much disposed to give out hoar-frost as it was last year.

h.	m.	Therm. in air.	Therm. in snow.
8	.....	+16	+7
8	30.....	+14	+3
9	.....	+8	+1
9	30.....	+7	+1
10	.....	+7	+3
10	30.....	+6	+0
11	Ball of therm. $\frac{1}{2}$ an inch above the surf. of the sn.		
		+5	+3
12	Ditto.....		+3

Tuesday morning.

On Monday evening the difference of temperature was found to be as in the annexed register.

1	Ball of therm. as formerly half immersed in the sn.		+3
2	.....	+8	+5
2	30.....	+10	+6

No aurora this evening; the air very still and serene till about 2 o'clock Tuesday morning, when the wind rose remarkably, and clouds formed in the north-east.

On Thursday, January 25, the difference of temperature was found to be as here set down in the margin.

From 10 till 11 o'clock this forenoon the thermometer on the ballustrade in air, 6 inches above the snow, pointed to  $+14$ , and when tried on the snow to  $+10$ . About noon this day some clouds were formed, which became quite general by 1 o'clock.

Thurs. morning.	Therm.	Therm.
h. m.	in air.	in snow.
9 45.....	$+10$ .....	$+3$
10 .....	$+10$ .....	$+3$
10 30.....	$+14$ .....	$+4$
11 .....	$+14$ .....	$+8$
11 30.....	$+17$ .....	$+9$
12 .....	$+20$ .....	$+12$
12 30.....	$+22$ .....	$+20$
1 .....	$+25$ .....	$+26$
1 30.....	$+27$ .....	$+27$

During the last 2 times of observing, 3 experiments were made with a view of discovering whether the snow without doors was gaining any thing from the air; or if any of it was carried off in the way of evaporation. For this purpose, a shallow dish, made of sheet brass, 4 inches in diameter, was exactly filled with snow, and carefully weighed. In order to defend the outside of the dish from the air, that no hoar-frost might attach itself to the metal, a circular hole was cut in the lid of a paste-board box, so wide, as just to let in the dish to the very brim, so that nothing communicated with the external air but the snow itself. The apparatus, in this state, was set without doors for 3 hours each time, and then brought in to the lobby of the observatory, where the dish was again weighed: but in none of these trials did it ever appear that any weight was lost. On the contrary, at the first weighing, which was on Monday night, 12 o'clock, it had gained 5 grains. In the other 2 trials the increase of weight was scarcely perceivable.

The temperature of the air in the west room of the observatory remaining very constantly for nearly 2 days at  $+27^{\circ}$ , a dish of snow, similar to the other exposed there, was found to lose weight very sensibly, and for the most part at the rate of 2 grains in an hour. Notwithstanding this, the snow thus wasting or évaporating had no power of sinking the thermometer below  $+27$ , the temperature of the surrounding air; though at one time it was fanned for 4 minutes by a piece of paper fastened to the end of a long stick. Not to disturb the uniform temperature of this room during these experiments, care was taken to stay in it a very short time at every visit, and to keep the door and the window-shutters close.

On Christmas-day there was a frost, which in the morning made the thermometer in air point to  $+21$ ; and during the preceding night there had been a profuse deposition of hoar-frost. A pound of this was collected, and its capacity for heat compared with that of ice, and found equal as nearly as could be judged.



Before making the 2 mixtures necessary for this experiment, the ice was reduced to a powder, and spread out on a paper beside the hoar-frost till both had acquired the same temperature.

On Monday night, January 22, about 12 o'clock, having occasion to take up a little snow, there was observed a cohesion among its parts rather greater than what might have been expected in a substance, at that time, so much frozen. This circumstance was further examined by the following experiment. A pane of glass was laid on the surface of the snow till it had acquired the temperature of  $+3$ , after which, with a bit of parchment equally cold, some snow was scraped from the very surface, and shaken all over the pane, so as to cover it in most parts lightly. On now lifting the pane, and holding it with the snow undermost, the whole of it adhered, and it required some smart raps before the greater part fell away. What remained cleaved to the glass with still a greater adhesion.

The experiments related above afford further reasons against the opinion of the difference of temperature between the snow or hoar-frost and the air depending on evaporation. It would also appear, that neither does this phenomenon depend on the deposition of hoar-frost. What renders this the more probable is, that last year there was a much more copious deposition at times when the difference of temperature was not more remarkable. But allowing that a deposition had been found a necessary circumstance, and always in proportion to that difference, the experiments on the capacities of hoar-frost and ice seem to show, that the sensible heat which disappears enters not into the composition of the hoar-frost; otherwise the capacity of this substance for heat, compared to that of ice or common snow, should be very different. It must be confessed however, that the abovementioned experiment would have been more applicable to this reasoning, had it been made with hoar-frost given out in colder states of the air.

If the air, at low temperatures, had any power of acting on the snow or hoar-frost, so as to produce a gradual melting, this circumstance, according to the known laws of heat, might occasion the difference of temperature under consideration. And what renders this idea not altogether improbable, is the peculiar cohesion among the parts of the snow above described. Perhaps a gentle melting might take place without much altering the appearance of the snow or hoar-frost at the surface, as the parts, when dissolved, might be gradually sucked downwards, and be afterwards distributed through the whole drier mass. It may also be worthy of an experimental inquiry to determine, how far that sort of concretion, observable all over the surface of snow which has been long frozen, bears any marks of a slow process of this kind. From a hill, a little way to the N.E. of the town, and which was to windward during the frost,

there were gathered 2 portions of snow, the one from the surface, and the other 7 inches below it. The water produced from the 2 kinds was preserved in very clean phials, in order to be compared together by some chemical trials, which perhaps might throw some light on the whole of this matter.

One other fact was new to me; namely, the power of ardent spirits of dissolving snow, and consequently of producing with it a freezing mixture. The alcohol and snow separately were at 8 degrees below the freezing point, and when mixed suddenly and intimately, the temperature became in the space of 20 seconds  $28^{\circ}$  below 0. This is a cold only  $12^{\circ}$  short of that which Fahrenheit first produced by using spirit of nitre for the experiment; and it is not improbable, had the present experiment been tried with more precaution and address, that the result would have been still more remarkable. There was employed only about a pint of alcohol, but the proportion of snow was not then attended to, and the thaw coming soon afterwards prevented a repetition of the experiment.

*Postscript.*—The water mentioned as produced from the superficial snow has been examined by several chemical trials, with a view of discovering if it differed in any respect from the water obtained from snow gathered at considerable depths, and near the ground. Had the atmosphere, when the thermometers pointed so low, been disposed to furnish any saline principle, the union of such an ingredient with the snow would have tended to produce an excess of cold at the surface, similar to what was then observed. Or if the snow at these low temperatures had acquired any remarkable power of dephlogisticating the air in contact with it, a cooling process at and near the confines of the snow and air might thereby have been maintained. In either of these cases, some very sensible indications of a saline, or of a phlogistic principle, might be expected on the water given by the snow collected from the surface. But in opposition to both these views it remains now to be mentioned, that nothing of this kind did appear in the course of the experiments, which indeed were contrived chiefly to detect such circumstances. If therefore the arguments produced in both papers on this subject will not allow us to account for so remarkable a cooling process by an evaporation at the surface of the snow, it would appear, that there remains still something unknown with respect to the cause. A proper investigation of this matter, in climates favourable to such experiments, may possibly unfold some further properties of heat with which at present we may be wholly unacquainted.\*

\* This interesting subject was further prosecuted by the author during the winter of 1783-4, by a variety of other experiments, of which he transmitted an account to Dr. Black, of Edinburgh, who communicated the paper to the Royal Society, which had been very recently instituted there; and which paper was afterwards published in the first volume of their Transactions, in 1788. We there find that, from a careful review of all the experiments stated in that and the two former accounts,



*XXVII. A General Theory for the Mensuration of the Angle subtended by Two Objects, of which One is observed by Rays after Two Reflections from Plane Surfaces, and the other by Rays coming Directly to the Spectator's Eye. By George Atwood, M. A., F. R. S. p. 395.*

The actual determination of an angle implies 2 observations, one taken at each extremity of the arc by which that angle is measured. When fixed astronomical quadrants or other sectors are used for the practical estimation of angles, one of these observations is previously made by directing the axis of the telescope or line of collimation to some fixed point in the heavens, the index being then coincident with the initial point on the arc of the sector: after this adjustment, one observation only is necessary to ascertain the angular distance between that point and any other celestial object in the plane of the sector. This method however is evidently impracticable, unless the instrument can be steadily fixed; for which reason astronomical quadrants become useless at sea; and from the difficulties which attend placing them in their due position and adjustment on firm ground, they are almost wholly confined to regular observatories.

Mr. Hadley, by an ingenious application of optical principles, contrived to bring both extremities of the arc measured into the field of the spectator's view at the same time; by which improvement, angles are taken at sea, as well as on land, with an unfixed instrument, to a degree of accuracy sufficient for nautical and other purposes, when the utmost exactness is not required. Mr. Hadley's invention is a particular case of a very extensive theory, as yet but little attended to. According to his method, which is well known, the 2 reflecting surfaces used in the observation are perpendicular to the plane of motion; the direction

the author was convinced that evaporation had no share whatever in producing the remarkable cold which was observed. The title of the paper is thus expressed: "Experiments and Observations on a remarkable Cold which accompanies the separation of Hoar-frost from a Clear Air;" and the following are the general conclusions which, in the author's opinion, the experiments establish:—"That when bodies attract hoar-frost from a clear air, there is a cold produced at their surfaces; and that this cold does not originate from any peculiar qualities of bodies on which the hoar-frost settles, any further than as some bodies are capable of attracting from the air more or less of it in a given time." And again, "That the disposition of the air of thus parting with hoar-frost, and the cold which accompanies that separation, has a constant dependance on the general serenity of the atmosphere, and is always interrupted on the sky being overcast with clouds or foginess, especially near the place of observation."

After stating these conclusions, the author observes: "That the nature or essence of the thing we call heat is so far removed beyond the immediate reach of our senses, that we need not wonder, though new facts relating to it come into view, and even though they cannot immediately be traced up to any general laws hitherto established. That if, on mature consideration, the present phenomena cannot be accounted for in this way, they ought, on that very account, to challenge our attention the more, as opening to us the necessity of enlarging our stock of principles, and inviting us forward to so desirable a work."

of the telescope, and of the rays passing between the reflectors, being parallel to that plane; whereas the inclination of the telescope, and of the intermediate rays, as well as of the reflectors themselves to the plane of motion, admit of unlimited variety. A general theory to determine the angle observed by 2 reflections, from the data on which its magnitude depends, without limitation or restriction, seems applicable to several useful purposes in practical astronomy. Having never seen any geometrical construction or analysis of this curious problem, Mr. A. was induced to bestow some consideration on the subject. And it must be acknowledged that his labours have produced a long and elaborate essay, more fit for a separate volume, than for a paper in the Philos. Trans. or these Abridgments; and of which the intricate constructions and analytical calculations could be of little or no use to the mere practical optician and astronomer.

*XXVIII. On the Ophidium Barbatum Linnei. By P. M. Augustus Broussonet, M. D. p. 436.*

This species of fish seems not to have been unknown to the ancients, though probably they confounded it with the Conger, to which it bears some resemblance. Perhaps the early Greek and Latin writers on natural history have mentioned it under the name of Tragus, or Callarias; but, for want of descriptions, they left us much in the dark concerning it. Pliny indeed speaks of a fish which appears to be of this species: he calls it Ophidion, and as that is the name given to it by all the modern writers, we are obliged to accept his synonymy without further inquiry.

The first author to whom we are indebted for a description and figure of the ophidium, is Bellonius; yet it appears, that he was not certain of the name of this fish, since he calls it gryllus, falso congrus, tragus, aselli species: nor was he less doubtful of the class to which he should refer it, and therefore placed it among the aselli, or gadi, though very different from the species of that family. Rondeletius, who wrote soon after Bellonius, has given a better description, and a more accurate figure of this fish, which he calls ophidion, with a reference to Pliny. In the figure of Bellonius the cirri are very ill represented, and the whole fish appears without any spots, whereas in the plate of Rondeletius it is covered with oblong spots. This remarkable difference between the figures of these authors was sufficient to determine Gesnerus, and others who have written since their time, and who are to be considered rather as compilers than authors, to take the fish described by Bellonius to be a different species from that of Rondeletius.

Willoughby, who is the first ichthyologist who has given any good description of fish, treats largely of the ophidium; and in his account describes the



scales, which are, as we shall hereafter explain, oblong, distinct, and disposed without any regular order. This description was sufficient to ascertain, that the difference between the figures arose from Rondeletius having drawn the scales omitted by Bellonius: yet the authors who wrote immediately after Willoughby, and particularly Ray in his Synopsis, follow Gesnerus, in maintaining two different species of cirrata ophidia, one with, the other without, spots.

Artedi did not take notice of the spots; he describes the fish in a genus to which he gives the name of ophidion, and places that genus among the Malacopterygii. After him Kleinius once more took notice of the spots; but at the same time introduced another confusion concerning this fish, arising from Rondeletius having said, that it has 2 cirri, while Willoughby asserts it has 4; but it is easy to reconcile these authors; for though the ophidium has only 2 cirri, yet each of these being divided in 2, they appear as 4; so that Willoughby might justly say, that it is quadri-cirratus. The same author places the ophidium in a genus which he calls enchelyopus, which is indeed not a good family, since it comprehends the genera of gymnotus, anarrhichas, cepola, blennius, cobitis, &c.

Linne, in his description of the ophidium barbatum, says that its whole body is covered with oblong spots, without any regular direction. Dr. Gouan, in his description of the genus of the ophidium, does not mention the scales; but gives the spots as a generic character. The last author who has mentioned these spots, and given a description of this fish, is Mr. Brunniche in his Ichthyologia Massiliensis. The genus of ophidium has the following principal characters, viz. the body long; the fins of the back, tail, and anus, confounded in one; no fin on the under part of the body; and the eyes covered by the common skin.

*Abridged Description of Ophidium Barbatum.*—Head compressed, sub-acute, naked, loosely covered by the common skin.

*Gape* wide; *upper mandible* doubled, and rather longer than the lower: *lips* skinny, thin.

*Teeth*, on the margin of both jaws, disposed into a narrow area, wider in front; minute, sharp, thick set, the anterior ones rather larger.

*Tongue* sub-obtuse, smooth.

*Palate* smooth in the middle, but in front roughened by three areas of teeth; the two lateral areas of a linear, and the middle of a sub-triangular, form.

*Eyes* large, near each other; situated on the upper part of the head, covered by the common skin; iris silvery, pupil yellowish.

*Cirri* or beards two, at the tip of the lower jaw, bipartite, one part longer than the other. Before the eyes a covered, recumbent, bony tubercle.

*Body* compressed, attenuated towards the tail.

*Lateral line* high, smooth, parallel with the back, adorned beneath with a silvery line.

*Scales* obovate, covered, umbonated, separate.

*Colour* of the head and body silvery flesh-colour.

*Dorsal fin* long, longer but narrower than the anal fin, continued into the tail-fin, dingy white at the base, but black at the margin, owing to numerous black points.

*Pectoral fins* obovate, pellucid, the membrane freckled or marked by extremely minute spots.



*Anal fin* united with the caudal, whitish at the base, black on the margin, furnished with simple rays.

*Caudal fin* black, with obtuse tip.

The scales of the ophidium, which have been figured by Rondeletius, but overlooked by many other writers, have been mentioned by Willoughby, but without any particular description. They are very different from those on the skin of the ophidium imberbe, which are shortly described by Gronovius. Their position, as may be seen in the figure, is irregular. They are dispersed over the whole body. Their form is sometimes round, sometimes nearly oval. They are larger near the head, and in the lower part of the body; but are hardly to be distinguished near the tail. They adhere to the body by means of a particular transparent skin, which is in general very thin, but somewhat thicker near the neck, and extended loosely over the whole head: this skin is very easily destroyed, after which the scales falling, the body appears spotted (fig. 1, pl. 3.) These scales are of the same sort as those that Leeuwenhoek has described on the eel, like those I have seen on the anarrhichas lupus, the blennius viviparus, and many other fishes, which are commonly thought to be without scales. When you look at them with the naked eye (fig. 2,) they appear as covered with very small grains; but viewed through a microscope (fig. 3,) the middle of them appears more elevated than the margin; and from the centre to the margin, close by each other, there are many lines or rays, formed by small scales, placed one upon another, like tiles on a roof, the superior being always the nearer to the centre. This sort of scales, which may be called umbonatae, are fastened to the body by very small vessels inserted in their middle; they are to be seen on the body only, not on the head nor the fins.

I shall now proceed to the anatomy of this fish, which certainly comprehends some very remarkable circumstances, which, I believe, have not yet been observed in any other species. When we have drawn off the skin, there appears a thin membrane of a silver colour, which covers the muscles. The muscles being removed, we find the peritoneum, which lines the abdominal cavity, and is adherent to the swimming bladder by some elongations. It is of a silver hue, with some very small black points. The ventricle is not to be distinguished from the intestines by any other mark but by its size: its form is oblong; it is extended almost to the anus, whence the intestinal duct has a retrograde course, and then descends again, having a little dilatation near the anus. On the vertebræ next the anus, on the outside of the peritoneum, is a kind of cavity of an oblong form, containing a reddish viscus, which I take to be the kidney.

The first vertebra from the head has nothing very remarkable in its structure. The 2d has on each side an elongated and sharp apophysis, to the apex of which is annexed a small ligament. The 3d is very flat, and has on each side a kind of



triangular and sharp apophysis, to which adheres a ligament as to the 2d. The 4th is remarkable in having a sharp apophysis on each side, articulated with the body of the vertebra, and under each of them is another articulated apophysis, flattish, thick, roundish at its extremities, and forked at its basis (fig. 5.) The 5th, which is strongly adherent to the former, has in its middle a bifid process. The 6th has in its middle a flattish elevation, sharp on each side. Between the extremity of the larger apophysis of the 4th vertebra, is a bone, or rather a hard cartilage, which bears the figure of a kidney (fig. 6); its convexity being turned towards the body of the vertebra: its position is parallel to the bodies of the vertebræ; its motion is half circular; one of its parts, viz. the lowest, being in the cavity of the swimming bladder, to which it adheres by a thin membrane, so that no air can escape at that part. It is covered by membranes, which adhere strongly to its middle; in this part are fastened the 2 ligaments of the apophysis of the 2d and 3d vertebræ, of which we spoke before, and which are of a greater tenuity. In the same point are fastened also 2 ligaments; each of which belongs to an oblong muscle parallel to each other, and fixed to the bones of the lowest and posterior part of the head (fig. 4.)

All this apparatus is certainly subservient to the purpose of swimming, I suppose, by the cavity of the bladder being made larger or less by the motions of the cartilaginous bone; but it is very remarkable, that if these parts are necessary to some animal function, they should not be found in all the individuals; for I have seen 2, of which the vertebræ were not different from the vertebræ of the other species: which difference depends perhaps on the difference of sex. I am inclined to believe so; but the generation in this fish seems to be no less mysterious than that of the eel: I could never distinguish a male from a female in this species. I do not know if the other species of ophidium have the same structure; I could not perceive it in some specimens of *mastacembelus*. Willoughby mentions that singular structure, but without any particular description.

This fish commonly grows to the size of 8 or 9 inches. It is to be found in all the Mediterranean Sea, and in great plenty in the Adriatic. It is taken by nets in Provence and Languedoc, with many other small species, which are not esteemed, that is, what they call *ravaillā*. It is often confounded with the *cepola* by the fishermen, though they have different names for each species. In Languedoc the ophidium is called *donzellā*, and the *cepola*, *flammā*. In Provence the former has the name of *corrudgiaö*, and the latter that of *rougeollā*. But the name of *donzellā*, very common on all the coast of the Mediterranean, is also applied to the *cepola*, and the *sparus julis* Linn. which however is commonly called *girellā*. In summer the ophidium is more common: its flesh is not of a good taste, rather coarse, as that of all the species of fishes, which having no ventral fins, are obliged to make great efforts in swimming, and have conse-



quently the muscles harder. The want of ventral fins induces me to believe, that it is not a migratory species. It feeds on small crabs and fishes.

*XXIX. A Further Account of the Usefulness of Washing the Stems of Trees.*  
By Mr. Robert Marsham, of Stratton, F. R. S. p. 449.

In the former account, Mr. M. showed how much a beech increased on its stem being cleaned and washed; and in this he shows that the benefit of cleaning the stem continues several years: for the beech which he washed in 1775 increased in the 5 years after the washing  $8\frac{6}{10}$  inches, or above an inch and  $\frac{7}{10}$  yearly; and the aggregate of 9 unwashed beeches of the same age does not amount to 1 inch and  $\frac{3}{10}$  yearly to each tree. In 1776 Mr. M. washed another beech of the same age, viz. seed in 1741; and the increase in 4 years after the washing was  $9\frac{2}{10}$  inches, or 2 inches and  $\frac{3}{10}$  yearly, when the aggregate of 9 unwashed beeches amounted to but 1 inch and  $\frac{3}{10}$  and a half. In 1776 he washed an oak which he planted in 1720, which increased in the 4 years after washing  $7\frac{2}{10}$  inches, and the aggregate of 3 oaks planted the same year (viz. all he measured) amounted to but 1 inch yearly to each tree. In 1779 Mr. M. washed another beech of the same age, and the increase in 1780 was 3 inches, when the aggregate of 15 unwashed beeches was not full  $15\frac{6}{10}$  inches, or not 1 inch and half a tenth to each tree; yet most of these trees grew on better land than that which was washed. But Mr. M. apprehends the whole of the extraordinary increase in the 2 last experiments should not be attributed to washing: for in the autumn of 1778 he had greasy pond-mud spread round some favourite trees, as far as he supposed their roots extended, and though some trees did not show to have received any benefit from the mud, yet others did, that is, an oak increased half an inch, and a beech  $\frac{3}{10}$ , above their ordinary growth. Now though the beech gained but  $\frac{3}{10}$ , yet perhaps that may not be enough to allow for the mud; for the summer of 1779 was the most ungenial to the growth of trees of any since he had measured them, some not gaining half their ordinary growth, and the aggregate increase of all the unwashed and unmudded trees that he measured (93 trees in number of various kinds) was in 1779 but 6 feet  $5\frac{7}{10}$  inches, or  $77\frac{7}{10}$  inches, which gives but  $\frac{8}{10}$  and about  $\frac{1}{3}$  to each tree; when in 1778 (a very dry summer in Norfolk) they increased near 85 inches, which gives above  $\frac{9}{10}$  to each tree: and this summer of 1780 being also very dry, yet the aggregate increase was above half an inch more than in 1778. But the best increase of these 3 years was low, as there were but 20 of the 93 trees that there were not planted by Mr. M., and greater increase is reasonably expected in young than old trees; yet he had an oak 200 years old in 1780, which was 16 feet and 5 inches in circumference, or 197 inches in 200 years. But this oak cannot properly be called old,



Mr. M. observes, that all the ingredients of vegetation united, which are received from the roots, stem, branches, and leaves of a mossy and dirty tree, do not produce half the increase that another gains whose stem is clean to the head only, and that not 10 feet in height. Is it not clear that this greater share of nourishment cannot come from rain? for the dirty stem will retain the moisture longer than when clean, and the nourishment drawn from the roots, and imbibed by the branches and leaves, must be the same to both trees. Then must not the great share of vegetative ingredients be conveyed in dew? May not the moss and dirt absorb the finest parts of the dew? and may they not act as a kind of screen, and deprive the tree of that share of air and sun which it requires?

*XXX. On the Use which may be made of the Tables of Natural and Logarithmic Sines, Tangents, &c. in the Numerical Resolution of Affected Equations. By Wm. Wales, F. R. S. p. 454.*

The first intimation met with relating to the use which may be made of the tables of sines, tangents, and secants, in resolving affected equations, is in the latter part of the 2d vol. of Prof. Saunderson's Elements of Algebra, printed in 1741, after his decease. The professor there shows how to resolve those 2 cases which make the 1st and 2d of the following examples, by means of the tables; but it appears, from many circumstances, he was not aware that the 3d case could be resolved in the same manner. All the 3 forms however were resolved by the late Mr. Anthony Thacker, a very ingenious man, who died in the beginning of the year 1744, by the help of a set of tables, of his own invention; different from, but in some measure analogous to, the tables of sines and tangents. These tables were computed and published, with several papers concerning them, after his death, by a Mr. Brown, of Cleobury. In these papers, besides explaining fully the use of the tables in resolving cubic equations, Mr. Thacker shows that his method comprehends the resolution of all biquadratic equations, if they be first reduced to cubic ones in the manner which has been explained by Descartes and others, and the 2d term then taken away.

Since that time M. Mauduit has shown how to find the roots of all the 3 forms of cubic equations, by means of the tables of sines, &c. in his excellent Treatise of Trigonometry. But none of these authors have attempted to resolve equations of more dimensions than 3, by these means, without first reducing them to that number; nor even these, till after the 2d term is taken away: whereas such reductions will generally take up more time than is required to bring out the value of the unknown quantity by the following method; and, after all, frequently serve no other purpose but that of rendering the operation more intricate and troublesome.

Mr. Landen, in his lucubrations, published in 1755, has given a general



method of resolving that case of cubic equations, by means of the tables of sines, where all the roots are real, without the trouble of taking away the 2d term of the equation; and Mr. Simpson has shown how to resolve equations of any dimensions, by the same means, provided those equations involve only the odd powers of the unknown quantity, and that the co-efficients observe such a law as will restrain the equation to that form which is expressive of the cosine of the multiple of an arc, of which the unknown quantity is the cosine. This was first done, it seems, by John Bernoulli, and afterwards by Mr. Euler, in his *Introduct. ad Analyt. Infinit.* and Mr. de Moivre, in his *Miscell. Analyt.*; but the resolution of all equations of this form, as well as many others, is comprehended in the first of the following observations.

The first thought of extending the use of the tables of sines, tangents, and secants, beyond the cases which have been already mentioned, occurred while I was considering the problem which produced the equation given in this paper as the 4th example. And it is remarkable, that the very same thought occurred to Dr. Hutton about the same time, and in the resolution of the same problem; and we were not a little surprized, on comparing our solutions together, to find that our ideas had taken so exactly the same turn; and that both should have stumbled on a thought, which, as far as either of us knew, had never presented itself to any one before. Having since examined further into the matter, I have the satisfaction to find, that the principle is very extensive, and that a great number of equations, especially such as arise in the practice of geometry, astronomy, and optics, may be resolved by it with great ease and expedition.

But besides the facility with which the value of the unknown quantity is brought out by means of the tables of sines, tangents, and secants, this method of resolution has another considerable advantage over most others which have been proposed, inasmuch as the first state of the equation, without any previous reduction, is generally the best it can be in for resolution; and from which it may most readily be discovered, how to separate it into such parts as express the sine, or the tangent, or the secant of the arc of a circle; or into the sine, tangent, or secant of some multiple of that arc, or of a part of it: and in the doing of which consists the principal part of the business in question. It will also be of some advantage to preserve the original substitutions as distinct as possible, by using only the signs of the several operations which it may be necessary to go through in bringing the solution of a problem to an equation, instead of performing the operations themselves.

Besides the advantages which this method of procedure affords to the mode of solution now more particularly under consideration, it has so many others over that which is commonly used, that I am much surprized the latter should ever have been adopted. By preserving thus the original substitutions distinct, all



the way through an operation, every expression, even to the final equation, will exhibit the whole process up to that step; and it will appear as clearly, how every expression has been derived, as it does in that mode of analysis which was used by the ancient geometers; whereas, when the several original expressions are melted down as it were into one mass, by the multitude of actual additions, subtractions, multiplications, and divisions, which they generally undergo, in a long algebraical process, conducted in the usual manner, it is impossible to trace the smallest vestige of the original quantities in the final equation, except such as are represented by a single letter. Of course, however obvious the several steps might be at the time when they were taken, every idea of them must be totally lost in the result; and it will be utterly impossible to trace them back again, in the manner they are done in the composition of a problem, the solution of which has been investigated by the geometrical analysis. Let me add, that it is to this cause we must attribute all that obscurity which the algebraic mode of investigation has been so frequently charged with.

Mr. W. then sets down, in 6 tables, the analytical expressions or forms of the correspondent sines, tangents, and secants of arcs, and their multiples, as far as the 6th, each of those being expressed in terms of the others; which tables are to serve as formulæ or theorems, with which to compare the forms of equations, when these occur in practice.

*Observations on the tables.*—Each of the formulæ in these tables may be considered as one side of an equation, involving the unknown quantity  $x$  to different dimensions. In some of the formulæ the odd powers of  $x$  are only found, in others the even ones alone, and in others both; but they are all equally useful in finding the value of the unknown quantity in affected equations which contain all the powers of that quantity, as will plainly appear from the following considerations.

1. If, on bringing the solution of any problem to an equation with some known quantity, it be found to correspond with any of the formulæ in these tables; or, if by any means it can be reduced to any of them; it is manifest, that nothing remains to be done but to divide the known side of the equation by the value of the quantity which is here denoted by  $r$ , and to seek for the quotient in the tables of sines, cosines, or tangents, as the case may require, and the value of the unknown quantity will be the sine, tangent, secant, or versed sine, of a given part of that arc (according as the expression is found in the 1st, 2d, 3d, or 4th table) multiplied by the value of  $r$ .

2. If, as will more frequently happen, the final equation of an operation be found equivalent to the sum, difference, product, or quotient, of some 2 or more of these formulæ; or to the sum, difference, product, or quotient, of some 2 or more of them multiplied or divided, increased or lessened, by some known



quantity or quantities; then, having taken away the known quantities by the common algebraic rules, observe the following ones.

1st. When the equation is found to correspond with the sum or difference of 2 formulæ in these tables, which are the sine and tangent, sine and cosine, or cosine and tangent, of the same arc, by running the eye along the tables of natural sines and tangents, find these 2 arcs, immediately following each other, the sum or difference of the sine and tangent, sine and cosine, or cosine and tangent, of which are one of them greater, and the other less, than the number which constitutes the known side of the equation. Take the excess of one of these sums or differences above, and what the other sum or difference wants of the said given number, add these two errors together, and say, as the sum of them is to  $60''$ , so is that error which belongs to the less arc to a number of seconds; which being added to the less arc will give one, the sum or difference of whose sine and tangent, sine and cosine, or cosine and tangent, is exactly equal to the number which constitutes the known side of the equation. Of the arc, thus found, let such a part be taken as the table in which the formulæ are found directs, and the natural sine, tangent, secant, or versed sine (as the case may require) of this part, being multiplied by the value of  $r$ , if  $r$  be found in the equation, will be the value of  $x$  sought.

2d. When the equation happens to be the product or quotient of 2 formulæ which express the sine and cosine, sine and tangent, or cosine and tangent, of the same arc, take the logarithm of the number which constitutes the known side of the equation, and then follow exactly the directions given in the first case, using the tables of logarithmic sines and tangents, instead of the tables of natural ones.

3d. If the equation, finally resulting from the resolution of any problem, present itself in an expression which is composed of the sum or difference of the sine, cosine, or tangent, of an arc, of which the unknown quantity is the sine, cosine, tangent, or versed sine, and the sine, cosine, or tangent, of some multiple of that arc, it will then be convenient to have 2 tables of sines and tangents; and in running the eye along them to find the 2 arcs immediately following each other, of which the sum or difference of the sine, cosine, or tangent, of one of them, and the sine, cosine, or tangent, of some multiple of it, may be less, and the sum or difference of the sine, cosine, or tangent, of the other, and the sine, cosine, or tangent, of the same multiple of it, may be greater than the number which constitutes the known side of the equation, for every minute of a degree that the finger is moved over in one, it must be moved over a number of minutes in the other, which is equal to the number of times that the single arc is contained in the multiple one. When these 2 arcs



are found, the operation will not differ so materially from that which is pointed out in the first rule as to merit repetition.

4th. If instead of the sum or difference of the sine, cosine, or tangent, of an arc, and the sine, cosine, or tangent, of some multiple of it, the form of the equation be such as to be constituted of the product of them, or the quotient of one divided by the other, the last rule will still hold good, using only the logarithmic sines and tangents instead of the natural ones, and comparing the sum or difference of them, according as the equation is composed of the product or quotient of the 2 factors, with the logarithm of the number which constitutes the known side of the equation, instead of that number itself.

5th. Sometimes the final equation will come out in expressions which are constituted of the sum, difference, product, or quotient, of the sine, cosine, or tangent, of some multiple of an arc, of which the unknown quantity is the sine, tangent, secant, or versed sine, and the sine, cosine, or tangent, of some other multiple of the same arc. And in any of these cases it is manifest, that the method of proceeding, in order to obtain one of the multiple arcs, and from thence the single one, of which the unknown quantity is the sine, tangent, &c. will not be greatly different from those which have been described in the 3d and 4th rules. The most material difference consists in this, that instead of proceeding minute by minute, according to the directions in the 3d rule to find the single arc, it will now be most convenient to proceed in each table by as many minutes at each step as are equal in number to the number of times which the single arc is contained in the multiple ones respectively.

6th. Equations will frequently occur in formulæ which express the square, cube, &c. of the sine, cosine, or tangent, of the multiple of some arc, of which the unknown quantity is the sine, tangent, secant, or versed sine; or in formulæ which are expressive of the sum, difference, product, &c. of the sine, cosine, or tangent, of an arc, and some power of the sine, cosine, or tangent, of the same arc; or of some multiple of it; the unknown quantity being some other trigonometrical line belonging to that arc. Or the equation may be compounded of the sum, difference, product, &c. of the same, or different powers of the sines, tangents, or cosines, of different multiples of an arc, the unknown quantity being the sine, tangent, secant, or versed sine, of that arc. In every one of these cases the tables will give the value of the unknown quantity, and in most of them with great ease and expedition. The method which is to be pursued in each case will readily present itself to a skilful analyst, who attends carefully to what has been already said, and to the examples which follow.

4. The formulæ in the 4 tables may be greatly varied by supposing  $x$ , the unknown quantity, to be some part or parts of the sine, tangent, &c. as



$\frac{1}{2}, \frac{1}{3}, \frac{2}{3}, \frac{3}{4}$ , &c. or some multiple of it, as twice, thrice, &c. Or  $x$  may be the square, or the square root, or any other power of the sine, tangent, secant, or versed sine, of an arc; in every one of which cases the formulæ will put on different appearances, either with respect to the powers or co-efficients of the unknown quantity, and yet admit of the same kind of application.

5. The tables may be rendered yet more extensively useful by inserting expressions for the sines, cosines, and tangents, of half the arc which has  $x$  for its sine, tangent, secant, or versed sine; and also for the sines, cosines, and tangents, of the odd multiples of this half arc, which expressions, together with those already inserted, may be considered as the sines, cosines, and tangents, of the multiples of an arc, the unknown quantity being the sine, tangent, &c. of twice that arc. And this consideration may sometimes be applied to very useful purposes.

6. In order to render the formulæ in the tables more general, I have put  $r$  for the radius of the circle; whereas it will frequently happen, that the equation, finally resulting from the resolution of a problem, especially those which relate to the doctrine of the sphere, will present itself in a form where the radius must be taken equal to unity: what these forms are will readily appear by substituting unity for  $r$  and its powers every where in the expression.

*Exam. 1.*—Required to find the value of  $x$  in an equation of the form  $x^3 - r^2x = a$ .

If  $r^2$  be expounded by 50, and  $a$  by 120, the equation may be reduced to  $\sqrt{x} \times \sqrt{(x^2 - 50)} = \sqrt{120}$ ; and, consequently, by the tables, if  $x$  be considered as the secant of an arc, of which the radius is  $\sqrt{50}$ , then  $\sqrt{(x^2 - 50)}$  will be its tangent, and we shall have to find an arc, such that the tangent multiplied by the square root of the secant may be equal to  $\sqrt{120}$ ; or, which amounts to the same thing, such an arc that the log. tang. together with half the log. secant may be equal to half the log. of 120. But because the tangent and secant, here required, are to the radius of the  $\sqrt{50}$ , the log. tangents and secants in the tables must be increased by the logarithm of that number, and therefore  $\log. \text{tang.} + \frac{1}{2} \log. 50 + \frac{1}{2} \log. \text{secant} + \frac{1}{4} \log. 50 = \frac{1}{2} \log. 120$ : or  $\log. \text{tang.} + \frac{1}{2} \log. \text{secant} = \frac{1}{2} \log. 120 - \frac{3}{4} \log. \text{of } 50$ . Hence, having taken  $\frac{3}{4}$  the log. of 50 from  $\frac{1}{2}$  the log. of 120, run the eye along the tables of logarithmic tangents and secants till an arc be found of which the sum of the log. tangent and  $\frac{1}{2}$  the log. secant is equal to 19.7653631, the remainder. In this manner it will be readily found, that the sum of the log. tangent and  $\frac{1}{2}$  the log. secant of  $28^\circ 37'$  is less than that difference by 2012, and that the sum of the log. tangent and  $\frac{1}{2}$  the log. secant of  $28^\circ 38'$  is greater than it by 1337: therefore  $3349 (2012 + 1337) : 60'' :: 2012 : 36''$ . The exact arc therefore, of which the sum of the log. tangent and  $\frac{1}{2}$  the log. secant is equal to 19.7653631 is  $28^\circ$



37' 36", and the log. secant of it is 10.0566242, which being increased by 0.8494850, the log. of  $\sqrt{50}$ , gives 0.9061092, which is the logarithm of 8.055810, the value of  $x$  sought, and which is true to 7 places of figures.

*Exam. 2.*—To find the value of  $x$  in the equation  $x^3 - r^2x = -a$ .

If  $r$  be expounded by 3, and  $a$  by 10, the equation will be  $x^3 - 9x = -10$ , and may be transformed to  $\sqrt{x} \times \sqrt{(9 - x^2)} = \sqrt{10}$ ; and therefore by the tables, the square root of the sine into the cosine of an arc, of which the radius is 3, is equal to the square root of 10. Consequently an arc must be found, such that the sum of the log. cosine and half the log. sine is equal to half the log. of 10. But because the radius of this arc must be 3, the log. sines and cosines must be increased by the log. of 3; and therefore log. cos. + log. of 3 +  $\frac{1}{2}$  log. sine +  $\frac{1}{2}$  log. of 3 must be equal to half the log. of 10; or, an arc must be found of which the sum of the tabular log. cosine and half the log. sine is equal to the difference between half the log. of 10 and  $1\frac{1}{2}$  the log. of 3. Hence, having subtracted  $1\frac{1}{2}$  log. of 3 from half the log. of 10, run the eye along Gardiner's tables of logarithmic sines, by which means it will be readily found, that the sum of the log. cosine and half the log. sine of  $28^\circ 53' 30''$  is less than 19.7843181, the excess of half the log. of 10 above  $1\frac{1}{2}$  log. 3, by 15, and that the sum of the log. cosine and half the log. sine of  $28^\circ 53' 40''$  is greater than that difference by 60. Consequently  $75 (15 + 60) : 10'' :: 15 : 2''$ . The exact arc therefore, of which the sum of the log. cosine and half the log. sine is equal to 19.7843181, is  $28^\circ 53' 32''$ ; and the log. sine of this arc, increased by the log. of 3, is 0.1612153, the logarithm of 1.44949, the value of  $x$  required, true to the last place.

But many equations of this form, and this example among the rest, admit of two positive values of the unknown quantity; and by carrying the eye farther along the tables it will be found also, that the sum of the log. cosine and half the log. sine of  $41^\circ 48' 30''$  is greater than 19.7843181 by 50, and that the sum of the log. cosine and half the log. sine of  $41^\circ 48' 40''$  is too little by 21. Consequently,  $71 (50 + 21) : 10'' :: 50 : 7''$ : of course,  $41^\circ 48' 37''$  is another arc, of which the sum of the log. cosine and half the log. sine is equal to 19.7843181, and the log. sine of this arc, increased by the log. of 3, is the logarithm of 1.999999, another value of  $x$ , and which errs but by unity in the 7th place.

The 3d root, as it is generally called, of this equation, which is necessarily negative, and equal to the sum of the other two, belongs properly to the equation which is given as the first example, of which it is the affirmative root, and may be found by the directions there given.

*Exam. 3.*—To find the value of  $x$  in the equation  $x^3 + r^2x = a$ .

Let us take as examples of this equation  $x^3 + 3x = .04$ ,  $x^3 + 3x = .08$ , and  $x^3 + 3x = .12$ , which are 2 of the instances given by Dr. Halley, in his Synopsis



of the Astronomy of Comets, to illustrate the mode of computation that he pursued in constructing his general table for calculating the place of a comet in a parabolic orbit; and it is obvious,  $a$  being put for the known side of the equation, that it may be transformed to  $\sqrt{x} \times \sqrt{3 + x^2} = \sqrt{a}$ : where, if  $x$  be considered as the tangent of an arc, the radius of which is  $\sqrt{3}$ , then  $\sqrt{3 + x^2}$  will be the secant of that arc; and consequently, by what is shown in the 1st example, an arc must be found such, that the sum of the tabular log. secant and half the tabular log. tangent may be equal to the excess of half the log. of  $a$  above  $\frac{3}{4}$  of the log. of 3. In the first of the above 3 instances this excess will be found, 18.9431891, in the 2d 19.0937041, and in the 3d, 19.1817497; and by running the eye along Gardiner's tables of logarithmic sines and tangents; it will be found, that the first falls between  $0^\circ 26' 20''$  and  $0^\circ 26' 30''$ , the 2d between  $0^\circ 52' 50''$  and  $0^\circ 53' 0''$ , and the 3d between  $1^\circ 19' 20''$  and  $1^\circ 19' 30''$ ; and, by pursuing the mode described in the former 2 examples, the exact arcs will be found  $0^\circ 26' 27''.7$ ,  $0^\circ 54' 51''.7$ , and  $1^\circ 19' 20''.1$ , and their respective tangents, to the radius  $\sqrt{3}$ , .01333248, .0266611, and .0399787, the 3 values of  $x$  sought. And in this manner Dr. Halley's table may be extended to any length with the utmost ease, expedition, and accuracy.

Thus far this matter has been carried by former writers; but those who may be at the trouble of consulting them will find that I have not copied their methods: on the contrary, these which are given here are more plain and obvious than theirs are, and the operations considerably shorter. What follows has not, I believe, been adverted to by any before me.

*Exam. 4.*—Let the equation arising from the proportion  $a : b + x(1 - c^2) :: c\sqrt{1 - x^2} : c^2x$  be taken, which is the result of an inquiry into the situation of that place on the surface of the earth, considered as a spheroid, which is at the greatest distance from a given one, suppose London. In this inquiry  $a$  and  $b$  were put to represent the sine and cosine of the latitude of the given place, in the spheroid;  $c$  for  $\frac{2}{3}\frac{2}{3}\frac{2}{3}$ , the ratio of the axes; and  $x$  for the sine of the distance of the required place from the opposite pole, in the spheroid also. The equation, which is of 4 dimensions with all the terms, is manifestly  $acx = [b + x(1 - c^2)] \times \sqrt{1 - x^2}$ , or  $\frac{x}{\sqrt{1 - x^2}} = x \cdot \frac{1 - c^2}{ac} = \frac{b}{ac}$ ; in which it is evident from the tables that the difference between the tangent and the product of the sine into a given quantity is known. In order therefore to find the value of  $x$ , compute  $\frac{b}{ac}$ , and  $\frac{1 - c^2}{ac}$ , and find the logarithm of the latter. Now because the elliptic meridian differs but little from a circle, the place sought will not be far from the antipodes of the given one, and its distance from the opposite pole may therefore be estimated at  $39^\circ 5'$ ; and, having taken out the natural tangent, and logarithmic sine of this arc, add the logarithm of  $\frac{1 - c^2}{ac}$  to the latter, and find



the number corresponding to the sum, which will be less than the natural tangent of  $39^{\circ} 5'$  by 2869. As this assumption is so near, take  $39^{\circ} 6''$  for the next, repeat the operation, and the result will be 1935 too great. Then 4804 ( $2869 + 1935$ ): $60'' :: 2869 : 36''$ ; which being added to  $39^{\circ} 5'$ , gives  $39^{\circ} 5' 36''$ , for the co-latitude of the place sought, and the natural sine of this arc, or .6305856 is the value of  $x$  in this equation.

To the foregoing Mr. W. adds three other similar examples.

*XXXI. Experiments on the Power that Animals, when placed in certain Circumstances, possess of producing Cold. By Adair Crawford, M. D. p. 479.*

The opinions of the ancients, respecting the nature and properties of fire, consisted of bold conjectures, which seem rather to have been the offspring of a lively and vigorous imagination, than of a just and correct judgment; their ideas on this subject being evidently derived, not so much from an accurate observation of facts, as from those sentiments of admiration and awe which many of the phenomena of fire are calculated to excite. Thus, this element was supposed, on the original formation of the universe, to have ascended to the highest place, and to have occupied the region of the heavens: it was conceived to be the principle which first communicated life and activity to the animal kingdom: it was considered as constituting the essence of inferior intellectual beings; and, by many of the ancient nations, it was revered as the supreme Deity. Indeed the profound veneration with which the element of fire was contemplated, for a long succession of ages, by a great part of mankind, appears to be one of the most curious circumstances in the history of ancient opinions. To account for this we may observe, that there is no principle in nature, obvious to the senses, which produces such important effects in the material system, and which, at the same time, in the mode of its operation, is so obscure and incomprehensible.

It appears to be accumulated in an immense quantity in the sun and fixed stars, whence its beneficial influence seems to be continually diffused over the universe; it is the great instrument by means of which the changes of the seasons are effected; the diversity of climates is chiefly owing to the various proportions in which it is distributed throughout the earth. If we add to this the mighty alterations which have been produced in human affairs by the introduction of artificial fire, by its employment in the separation of metals from their ores, and in the various arts which are subservient to the comfort, the ornament, and the preservation of the species, it will not appear surprising, that in a rude and ignorant age, this wonderful principle should have been considered as endued with life and intelligence, and that it should have become the object of religious veneration.

In the dark ages the alchymists regarded pure fire as the residence of the



Deity: they conceived it to be uncreated and immense, and attributed to its influence most of the phenomena of nature. Indeed, it is not wonderful that they should have assigned it a high rank in the scale of being, as it was the great agent which they employed in the chemical analysis of bodies, and was the instrument of those discoveries that attracted such universal admiration, and that enabled them so successfully to impose on the ignorance and credulity of the times.

On the revival of literature, the importance of this branch of science began very soon to engage the attention of philosophers. It could not escape the general observation, in a penetrating and inquisitive age, when the powers of the human mind were employed with so much ardour and success in exploring the operations of nature, that the element of fire acts a principal part in the system of the world; that by the influence of this element those motions are begun and supported in the animal and vegetable kingdoms, which are essential to the production and preservation of life; and that it is the great agent in those successive combinations and decompositions, by which all things on the surface of the earth, and probably throughout the universe, are kept in a continual fluctuation.

But though the utility of this branch of science was perceived, yet the progress that was made in the cultivation of it did not keep pace with the opinion which men entertained of its importance. Our senses inform us, that heat has a real existence, but they give us no direct information with regard to its nature and properties: it is endowed with such infinite subtilty, that it has been called, by a very eminent philosopher, an occult quality: by some it has even been considered as an immaterial being. It is therefore with great difficulty that it can be made the subject of philosophical investigation; and hence the opinions of men concerning it have been fluctuating and various, and the words which express it vague and ambiguous. The first step that was taken, with a view to the cultivation of this branch of science, was the construction of a machine for measuring the variations of sensible heat; observing, that heat has the power of expanding bodies, and considering the degree of expansion as proportional to the increase of heat, philosophers have endeavoured by means of the former to render the latter obvious to the senses.

To this important invention, the author of which cannot be distinctly traced, we are indebted for all the succeeding improvements in the philosophy of heat. By means of it men were enabled to establish a variety of interesting facts, and to bring some of the most obscure and intricate phenomena of nature to the test of experiment. The opinion, that the heats inherent in various heterogeneous substances differed from each other in kind, as well as in degree, was now exploded, since all were found to produce similar effects on the thermometer. The increase and diminution of temperature in the different seasons and cli-



inates, the laws which nature observes in the heating and cooling of bodies, the melting, the vaporific, and shining points, and the degrees of heat in the animal, the mineral, and the vegetable kingdoms, were accurately determined. In consequence of the attention that was paid to this subject, many curious questions arose, which have long exercised the ingenuity of philosophers. That property of heat by which it is capable of expanding the densest and hardest bodies; its power in producing fluidity; its tendency to an equilibrium; and the causes of its various distribution throughout the different substances in nature, have become the objects of philosophical inquiry. It was observed, that some bodies on exposure to heat, become red and luminous, but are incapable of producing flame, or of maintaining fire: that, on the contrary, others, by the application of fire, and the contact of fresh air, kindle into flame, and continue to emit light and heat, apparently from a source within themselves, till they are consumed. Hence arose the questions concerning the pabulum of fire, the use of the air in inflammation, and the distinction of bodies into combustible and incombustible.

From the first dawnings of philosophy it must have been perceived, that most animals have a higher temperature than the medium in which they live; and that a constant succession of fresh air is necessary to the support of animal life. The causes of these phenomena have afforded matter for much speculation in ancient as well as modern times: but the discovery that animals have, in certain circumstances, the power of keeping themselves at a lower temperature than the surrounding medium, was reserved for the industry of the present age. This discovery seems originally to have arisen from observations on the heat of the human body in warm climates. It was mentioned by governor Ellis in 1758; it was taught by Dr. Cullen before the year 1765; and at length it was completely established by the experiments of Dr. Fordyce in heated rooms, which were laid before the Society in 1774.

In the course of these experiments the doctor remained in a moist air heated to  $130^{\circ}$  for the space of 15 minutes, during which time the thermometer under his tongue stood at  $100^{\circ}$ , his pulse made 139 beats in a minute, his respiration was but little affected, and streams of water ran down over his whole body, proceeding from the condensation of vapour, as evidently appeared from a similar condensation on the side of a Florentine flask that had been filled with water at  $100^{\circ}$ . He found however, that he could bear a much greater degree of heat when the air was dry. In this situation, he frequently supported, naked, for a considerable time, without much inconvenience, the heat of  $260^{\circ}$ , his body preserving very nearly its proper temperature, being never raised more than  $2^{\circ}$  above the natural standard.

Various opinions have been entertained with regard to the causes of the facts



which were established by these experiments. Some have attributed the cold solely to evaporation, and have conceived that the same degree of refrigeration would have been produced by an equal mass of dead matter, containing an equal quantity of moisture. Others have affirmed, that the cold did not arise solely from this cause; but have maintained, that it depended partly on the energy of the vital principle, being greater than what would have been produced by an equal mass of inanimate matter. The ingenious Dr. Munro, of Edinburgh, ascribes the cold in the above-mentioned experiments to the circulation of the blood, in consequence of which the warmer fluids are continually propelled from the surface towards the centre, where they are mixed with blood at a lower temperature, and hence the animal is slowly heated, in the same manner as the water in a deep lake, during the winter, is slowly cooled, and not without a long continuance of frost congealed, no part of it becoming solid till the whole is brought down to the freezing point.

The following experiments were made with a view to determine with greater certainty the causes of the refrigeration in the above instances. To discover whether the cold produced by a living animal, placed in air hotter than its body, be not greater than what would be produced by an equal mass of inanimate matter, Dr. C. took a living and a dead frog, equally moist, and of nearly the same bulk, the former of which was at  $67^{\circ}$ , the latter at  $68^{\circ}$ , and laid them on flannel in air which had been raised to  $106^{\circ}$ . In the course of 25 minutes the order of heating was as annexed.\*

	Min.	Air.	Dead frog.	Living frog.
In	1	— $^{\circ}$	$70\frac{1}{2}^{\circ}$	$67\frac{1}{2}^{\circ}$
	2	102	72	68
	3	100	$72\frac{1}{2}$	$69\frac{1}{2}$
	4	100	73	70
	25	95	$81\frac{1}{4}$	$78\frac{1}{4}$

The thermometer being introduced into the stomach, the internal heat of the animals was found to be the same with that at the surface. Hence it appears, that the living frog acquired heat more slowly than the dead one. Its vital powers must therefore have been active in the generation of cold.

To determine whether the cold produced in this instance depended solely on the evaporation from the surface, increased by the energy of the vital principle, a living and dead frog were taken at  $75^{\circ}$ , and were immersed in water at  $93^{\circ}$ , the living frog being placed in such a situation as not to interrupt respiration.†

	Min.	Dead Frog.	Living frog.
In	1	$85^{\circ}$	$81^{\circ}$
	2	$88\frac{1}{2}$	85
	3	$90\frac{1}{2}$	87
	5	$91\frac{1}{2}$	89
	6	$91\frac{1}{2}$	89
	8	$91\frac{1}{2}$	89

These experiments prove, that living frogs have the faculty of resisting heat,

\* In the two following experiments the thermometers were placed in contact with the skin of the animals under the axillæ.—Orig.

† In the above experiment the water, by the cold frogs and by the agitation which it suffered during their immersion, was reduced nearly to  $91^{\circ}\frac{1}{2}$ .—Orig.



or producing cold, when immersed in warm water: and the experiments of Dr. Fordyce prove, that the human body has the same power in a moist as well as in a dry air: it is therefore highly probable, that this power does not depend solely on evaporation.

It may not be improper here to observe, that healthy frogs, in an atmosphere above  $70^{\circ}$ , keep themselves at a lower temperature than the external air, but are warmer internally than at the surface of their bodies: for when the air was  $77^{\circ}$ , a frog was found to be  $68^{\circ}$ , the thermometer being placed in contact with the skin; but when the thermometer was introduced into the stomach, it rose to  $70^{\circ}\frac{1}{2}$ . It may also be proper to mention, that an animal of the same species placed in water at  $61^{\circ}$ , was found to be nearly  $61^{\circ}\frac{1}{4}$  at the surface, and internally it was  $66^{\circ}\frac{1}{2}$ . These observations are meant to extend only to frogs living in air or water at the common temperature of the atmosphere in summer. They do not hold with respect to those animals, when plunged suddenly into a warm medium, as in the preceding experiments.

To determine whether other animals also have the power of producing cold, when surrounded with water above the standard of their natural heat, a dog at  $102^{\circ}$  was immersed in water at  $114^{\circ}$ , the thermometer being closely applied to the skin under the axilla, and so much of his head being uncovered as to allow him a free respiration.

In 5 minutes the dog was  $108^{\circ}$ , water  $112^{\circ}$

6 .....	109 .....	112	
11 .....	108 .....	112	the respiration having become very rapid.
13 .....	108 .....	112	the respiration being still more rapid.
30 .....	109 .....	112	the animal then in a very languid state.

Small quantities of blood being drawn from the femoral artery, and from a contiguous vein, the temperature did not seem to be much increased above the natural standard, and the sensible heat of the former appeared to be nearly the same with that of the latter.

In this experiment a remarkable change was produced in the appearance of the venous blood: for it is well known, that in the natural state, the colour of the venous blood is a dark red, that of the arterial being light and florid; but after the animal, in the experiment in question, had been immersed in warm water for half an hour, the venous blood assumed very nearly the hue of the arterial, and resembled it so much in appearance, that it was difficult to distinguish between them. It is proper to observe, that the animal which was the subject of this experiment, had been previously weakened by losing a considerable quantity of blood a few days before. When the experiment was repeated with dogs which had not suffered a similar evacuation, the change in the colour of the venous blood was more gradual; but in every instance in which the trial was made, and it was repeated 6 times, the alteration was so remarkable, that



the blood which was taken in the warm bath could readily be distinguished from that which had been taken from the same vein before immersion, by those who were unacquainted with the motives or circumstances of the experiment.

To discover whether a similar change would be produced in the colour of the venous blood in hot air, a dog at  $102^{\circ}$  was placed in air at  $134^{\circ}$ . In 10 minutes the temperature of the dog was  $104^{\circ}\frac{1}{2}$ , that of the air being  $130^{\circ}$ . In 15 minutes the dog was  $106^{\circ}$ , the air  $130^{\circ}$ . A small quantity of blood was then taken from the jugular vein, the colour of which was sensibly altered, being much lighter than in the natural state. The effect produced by external heat on the colour of the venous blood, seems to confirm the following opinion, which was first suggested by my worthy and ingenious friend Mr. Wilson, of Glasgow. Admitting that the sensible heat of animals depends on the separation of absolute heat from the blood by means of its union with the phlogistic principle in the minute vessels, may there not be a certain temperature at which that fluid is no longer capable of combining with phlogiston, and at which it must of course cease to give off heat? It was partly with a view to investigate the truth of this opinion that Dr. C. was led to make the experiments recited above.

I shall now endeavour, (says Dr. C.) from the preceding facts, to explain what appear to me to be the true causes of the cold produced by animals when placed in a medium, the temperature of which is above the standard of their natural heat. In a work which I some time since laid before the public, having attempted to prove, that animal heat depends on the separation of elementary fire from the air in the process of respiration, I observed, that when an animal is placed in a warm medium, if the evaporation from the lungs be increased to a certain degree, the whole of the heat separated from the air will be absorbed by the aqueous vapour. From the experiments on venous and arterial blood, recited in the 3d section of that work, it appears, that the capacity of the blood for containing heat is so much augmented in the lungs, that, if its temperature were not supported by the heat which is separated from the air, in the process of respiration, it would sink  $30^{\circ}$ . Hence, if the evaporation from the lungs be so much increased as to carry off the whole of the heat that is detached from the air, the arterial blood when it returns by the pulmonary vein will have its sensible heat greatly diminished, and will consequently absorb heat from the vessels which are in contact with it, and from the parts adjacent. The heat which is thus absorbed in the greater vessels will again be extricated in the capillaries, where the blood receives a fresh addition of phlogiston. If, in these circumstances, the blood during each revolution were to be equally impregnated with this latter principle, it is manifest, that the whole effect of the above process would be to cool the system at the centre, and to heat it at the surface; or to convey the heat to that part of the body where it is capable of being instantly



carried off by evaporation. But it appears, from the experiments which have been last recited, that when an animal is placed in a heated medium, the sanguineous mass, during each revolution, is less impregnated with phlogiston; for we have seen, that the venous blood, in these circumstances, becomes gradually paler and paler in its colour, till at length it acquires very nearly the appearance of the arterial: and it is rendered highly probable by the experiments of Dr. Priestley, that the dark and livid colour of the blood in the veins depends on its combination with phlogiston in the minute vessels. Since therefore, in a heated medium, this fluid does not assume the same livid hue, we may conclude, that it does not attract an equal quantity of the phlogistic principle.\*

It follows, that the quantity of heat given off by the blood in the capillaries will not be equal to that which it had absorbed in the greater vessels, or positive cold will be produced. If the blood, for example, in its passage to the capillaries, absorb from the greater vessels, a quantity of heat as  $30^{\circ}$ , and if, in consequence of its receiving a less impregnation of phlogiston than formerly, it give off at the extreme vessels a quantity of heat only as  $20^{\circ}$ , it is manifest, that on the whole a degree of refrigeration will be produced as  $10^{\circ}$ , and this cause of refrigeration will continue to act while the venous blood is gradually assuming the hue of the arterial, till the difference between them is obliterated; after which it will cease to operate. Thus it appears, that when animals are placed in a warm medium, the same process which formerly supplied them with heat becomes for a time the instrument of producing cold, and probably preserves them from such rapid alterations of temperature as might be fatal to life.

On the whole, the increased evaporation, the diminution of that power by which the blood in the natural state is impregnated with phlogiston, and the constant reflux of the heated fluids towards the internal parts, seem to be the great causes on which the refrigeration depends. Having found that the attraction of the blood to phlogiston was diminished by heat, it appeared probable, on the other hand, that it would be increased by cold. To determine this, a dog at  $100^{\circ}$  was immersed in water nearly at  $45^{\circ}$ . In about a quarter of an hour a small quantity of blood was taken from the jugular vein, which was evidently much deeper in its colour than that which had been taken in the warm bath, and

\* It is of no consequence in the above argument, whether we suppose, with Dr. Priestley, that the alteration of colour in the blood depends on its combination with phlogiston in the capillary arteries, or maintain with some other philosophers that this alteration arises from a change produced in the blood itself by the action of the vessels; it is sufficient for our purpose to assume it as a fact, which, I think, has been proved by direct experiment; that, in the natural state of the animal, the blood undergoes a change in the capillaries, by which its capacity for containing heat is diminished; and that in a heated medium it does not undergo a similar change.—Orig.



appeared to me, as well as to several other gentlemen, to be the darkest venous blood we had ever seen.

From this experiment, compared with those which have been recited before, we may perceive the reason why animals preserve an equal temperature, notwithstanding the great variations in the heat of the atmosphere, arising from the vicissitudes of the weather, and the difference of season and climate: for, as soon as, by exposure to external cold, an unusual dissipation of the vital heat is produced, the blood, in the course of the circulation, begins to be more deeply impregnated with the phlogistic principle. It will therefore furnish a more copious supply of this principle to the air in the lungs, and will imbibe a greater quantity of fire in return. In summer, on the contrary, the reverse of this will take place, less phlogiston will be attracted in the minute vessels, and less fire will be absorbed from the air. And hence the power of generating heat is in all cases proportioned to the demand. It is increased by the winter colds, diminished by the summer heats: it is totally suspended or converted into a contrary power, as the exigencies of the animal may require. From the changes which are produced in the colour of the venous blood by heat and cold, we may also perceive the reason why the temperature of the body is frequently increased by plunging suddenly into cold water, and why the warm bath has such powerful effects in cooling the system, and in removing a general or partial tendency to inflammation.

*XXXII. Account of a Comet. By Mr. Herschel, F. R. S. p. 492.*

On Tuesday the 13th of March, 1781, between 10 and 11 in the evening, while examining the small stars in the neighbourhood of H Geminorum, I perceived one that appeared visibly larger than the rest: being struck with its uncommon magnitude, I compared it to H Geminorum and the small star in the quartile between Auriga and Gemini, and finding it so much larger than either of them, suspected it to be a comet. I was then engaged in a series of observations on the parallax of the fixed stars, which I hope soon to have the honour of laying before the R. S.; and those observations requiring very high powers, I had ready at hand the several magnifiers of 227, 460, 932, 1536, 2010, &c. all which I have successfully used on that occasion. The power I had on when I first saw the comet was 227. From experience I knew that the diameters of the fixed stars are not proportionally magnified with higher powers, as the planets are; I therefore now put on the powers of 460 and 932, and found the diameter of the comet increased in proportion to the power, as it ought to be, on a supposition of its not being a fixed star, while the diameters of the stars to which I compared it, were not increased in the same ratio. Also, the comet being mag-



nified much beyond what its light would admit of, appeared hazy and ill-defined with these great powers, while the stars preserved that lustre and distinctness which from many thousand observations I knew they would retain. The sequel has shown that my surmises were well founded, this proving to be the comet we have lately observed.

Mr. H. reduced all his observations on this comet to 3 tables. The first contains the measures of the gradual increase of the comet's diameter. The micrometers he used, when every circumstance is favourable, will measure extremely small angles, such as do not exceed a few seconds, true to 6, 8, or 10 thirds at most; and in the worst situations true to 20 or 30 thirds: he therefore gave the measures of the comet's diameter in seconds and thirds. The first table, containing the measures of the comet's diameter, shows that, from March 17 till April 18, the apparent diameter increased from  $2'' 53'''$  to  $5'' 20'''$ .

The 2d table contains the comet's distances from several telescopic fixed stars, from March 13 till April 19, and those distances expressed in minutes, seconds, and thirds. And the 3d table contains the comet's angle of position with regard to the parallel of declination of the same stars measured by a micrometer; by which means its places and apparent path might be determined.

*Description of a Micrometer for taking the Angle of Position. By Mr. Wm. Herschel, of Bath. p. 500.*

Fig. 7, pl. 3, represents the micrometer inclosed in a turned case of wood, as it is put together, ready to be used with the telescope. A is a little box which holds the eye-glass. B is the piece which covers the inside work, and the box A is screwed into it. C is the body of the micrometer containing the brass work, showing the index plate a projecting at one side, where the case is cut away to receive it. D is a piece, having a screw b at the bottom, by means of which the micrometer is fastened to the telescope. To the piece C is given a circular motion, in the manner the horizontal motion is generally given to Gregorian reflectors, by the lower part going through the piece D, where it is held by the screw E, which keeps the two pieces C and D together, but leaves them at liberty to turn on each other.

Fig. 8, is a section of the case containing the brass work, where may be observed the piece B hollowed out to receive the box A, which consists of 2 parts inclosing the eye lens. This figure also shows how the piece C passes through D, and is held by the ring E: the brass work, consisting of a hollow cylinder, a wheel and pinion, and index plate, is there represented in its place. F is the body of the brass work, being a hollow cylinder with a broad rim c at the upper end; this rim is partly turned away to make a bed for the wheel d. The pinion e turns the wheel d, and carries the index plate a. One of its pivots moves in



the arm *f*, screwed on the upper part of *c*, which arm serves also to confine the wheel *d* to its place on *c*. The other pivot is held by the arm *g* fastened to *F*.

Fig. 9, is a plan of the brass work. The wheel *d*, which is in the form of a ring, is laid on the upper part of *F* or *c*, and held by 2 small arms *f* and *h*, screwed down to *e* with the screws *i*, *i*.

Fig. 10 is a plan of the brass work; *d*, *d*, is the wheel placed on the bed or socket of the rim of the cylinder *c*, *c*, and is held down by the two pieces *f*, *h*, which are screwed on *c*, *c*. The piece *f* projects over the centre of the index plate to receive the upper pivot of the pinion *m*, *n*, the fixed wire fastened to *c*, *c*. *o*, *p*, the moveable wire fastened to the annular wheel *d*, *d*. The index plate *a* is divided into 60 parts, each sub-divided into 2, and milled on the edge. When the finger is drawn over the milled edge of the index plate from *q* towards *r*, the angle *mso*, will open, and if drawn from *r* towards *q*, it will shut again. The case *c*, *c*, must have a sharp corner *t*, which serves as a hand to point out the division on the index plate.

*XXXIII. Concerning the Longitude of Cambridge in New England. By Mr. Joseph Willard. p. 502.*

The difference of meridians between Greenwich and Cambridge has been generally reckoned  $4^h 44^m$ . This was what the late Dr. Winthrop made use of; but I do not find that he determined it by actual observations, made by him at Cambridge, compared with corresponding ones, made at the Royal Observatory at Greenwich. It appears, that in 1769, at the time of the transit of Venus, the doctor was not quite certain of the longitude of Cambridge. He mentioned  $4^h 44^m$  as near the truth; but for better fixing it, he gave several of his observations of the eclipses of Jupiter's satellites to be compared with those made at Greenwich; but there were too few corresponding ones to determine the point with precision; and as modern astronomers do not place absolute dependence on the difference of meridians deduced from the eclipses of Jupiter's satellites, unless there has been a series of observations, both of immersions and emersions, I have wished to find some observations of solar eclipses and occultations of fixed stars by the moon, made at Cambridge, of which corresponding ones were made at Greenwich. I have met with no observations of occultations made by Dr. Winthrop; but a solar eclipse was observed by him and several other gentlemen, at his house, August 5, 1766, at which I was present and assisting, being then a resident graduate at Harvard College: this eclipse I find was observed at Greenwich, where the beginning of the eclipse was seen at  $5^h 29^m 56^s$  P. M. and the end at  $7^h 11^m 27^s$  P. M. apparent time. At Dr. Winthrop's house at Cambridge, lat.  $42^\circ 25'$  N. the beginning of this eclipse was observed at  $11^h 39^m 23^s$  A. M. and the end at  $2^h 45^m 9^s$  P. M. apparent time. Allowing for



the spheroidal figure of the earth, and going through the parallax calculations and deductions, I find the difference of meridians between Greenwich and Cambridge, by the observations of this eclipse, to be  $4^{\text{h}} 44^{\text{m}} 22^{\text{s}}$ .

In the transit of Venus, in 1769, the internal contact was observed by Dr. Winthrop at  $2^{\text{h}} 47^{\text{m}} 30^{\text{s}}$  apparent time, and at the Royal Observatory, at  $7^{\text{h}} 28^{\text{m}} 57^{\text{s}}$  apparent time. Allowing the sun's parallax on the day of the transit to be  $8''.38$ , I find by calculation from these observations, that the difference of meridians between Greenwich and Cambridge is  $4^{\text{h}} 44^{\text{m}} 12^{\text{s}}$ . Taking the mean between the deduction made from the observations of the internal contact of Venus, and of the beginning and ending of the above solar eclipse, the difference of meridians between Greenwich and Cambridge is  $4^{\text{h}} 44^{\text{m}} 17^{\text{s}}$ . This is the difference that I at present take, when I make use of tables fitted to the meridian of Greenwich; but I should be still glad of more corresponding observations to ascertain this point.

*Some Thermometrical Experiments; containing, 1. Experiments relating to the Cold produced by the Evaporation of various Fluids, with a Method of Purifying Æther. 2. Experiments relating to the Expansion of Mercury. 3. Description of a Thermometrical Barometer. By Tiberius Cavallo, F. R. S. Nominated to prosecute Discoveries in Nat. Hist. pursuant to the Will of the late H. Baker, Esq., F. R. S. p. 509.*

1. *On the cold produced by the evaporation of fluids, with a method of purifying æther.*—It is at present well known, that by the evaporation of various fluids a sensible degree of cold is produced; and that by the evaporation of æther, which is the most volatile fluid we are acquainted with, water may be congealed, and the thermometer may be brought several degrees below the freezing point. But as various thermometrical experiments, which I lately made, have exhibited some new phenomena, and as I have contrived an easy and pleasing method of freezing a small quantity of water in a short time, and in every climate; I think it not improper to give an account of these things in the first part of this lecture.

My first experiments were intended to discover, if possible, a fluid cheaper than æther, by the evaporation of which a degree of cold sufficient for some useful purpose might be generated. But in this my expectation was disappointed, as I found that æther was incomparably superior to any other fluid, as the cold it produced was several degrees greater than that occasioned by any other of the most volatile fluids whatever. Being therefore obliged to use æther, I endeavoured to contrive a method, by which the least possible quantity of it might be wasted in the production of a degree of cold sufficient to freeze water, and in this I met with success. But before we come to the description of this method



I shall briefly relate some observations made on the cold produced by the evaporation of other fluids besides æther.

In a room, the temperature of which was  $64^{\circ}$  according to Fahrenheit's thermometer, and in which the air was gently ventilated, I observed the effects produced by various fluids when thrown upon the ball of a thermometer. The ball of this thermometer was quite detached from the ivory piece on which the scale was engraved. The various fluids were thrown on the thermometer through the capillary aperture of a small glass vessel, shaped like a funnel, and care was taken to throw them so slowly upon the bulb of the thermometer, that a drop might now and then fall from the under part of it; except when those fluids were used, which evaporate very slowly, in which case it was sufficient to keep the ball of the thermometer only moist, without any drop falling from it. During the experiment the thermometer was kept turning very gently round its axis, in order that the fluid used might fall on every part of its bulb. This method I find to answer much better than that of dipping the ball of the thermometer into the fluid and removing it immediately after, or that of wetting the thermometer with a feather. The evaporation, and consequently the cold produced by it, may be increased by ventilation, viz. by blowing with a pair of bellows on the thermometer; but this was not used in the following experiments, because it is not easily performed by one person, and also because it occasions very uncertain results.

With the above described method I began to examine the effects of water, and found, that the thermometer was brought down to  $56^{\circ}$ , viz.  $8^{\circ}$  below the temperature of the room in which the experiment was made, and of the water employed. This effect was produced in about 2 minutes time, after which a longer continuation did not bring the mercury lower. By means of spirit of wine the thermometer was brought down to  $48^{\circ}$ , which is only  $16^{\circ}$  below the temperature of the room, and of the spirit employed. When the spirit of wine is highly rectified, the cold produced by its evaporation is certainly greater than when it is of the common sort; but the difference is not so great as one, who never tried the experiment, might expect. The purer spirit produces the effect much quicker. Using various other fluids, which were either compounds of water and spirituous substances, or pure essences, I found that the cold produced by their evaporation was generally in some intermediate degree between the cold produced by the water and that produced by the spirit of wine. Spirit of turpentine brought the thermometer only  $3^{\circ}$  lower than the temperature of the room; but olive oil and other oils, which evaporate either very slowly or not at all, did not sensibly affect the thermometer.

Wishing to observe how much electrization could increase the evaporation of spirit of wine, and consequently the cold produced by it, I put the tube contain-



ing the spirit into an insulating handle, and connected it with the conductor of an electrical machine, which was kept in action while the experiment was performed; by these means the thermometer was brought down to  $47^{\circ}$ . Having tried the 3 mineral acids, I found that instead of cooling they heated the thermometer, which effect I expected; since it is well known, that those acids attract the water from the atmosphere, and that heat is produced by the combination of water and any of them. The vitriolic acid, which was very strong and transparent, raised the thermometer to  $102^{\circ}$ ; the smoking nitrous acid raised it to  $72^{\circ}$ ; and the marine acid raised it to  $66^{\circ}$ ; the temperature of the room, as well as of the acids, being  $64^{\circ}$ , as mentioned above.

The apparatus which I contrived for the purpose of using the least possible quantity of æther in freezing water, &c. consists in a glass tube, terminating in a capillary aperture, which tube is to be fixed on the bottle that contains the æther. Fig. 11, pl. 3, exhibits such a tube, round the lower part of which, at A, some thread is wound, to make it fit the neck of the bottle. When the experiment is to be made, the stopper of the bottle containing the æther is removed, and the above-mentioned tube is fixed on it. The thread round this tube should be moistened a little with water or spittle before it is fixed on the bottle, to prevent more effectually any escape of æther between the neck of the bottle and the tube. Then holding the bottle by its bottom FG, fig. 12, and keeping it inclined as in the figure, the small stream of æther issuing out of the aperture D of the tube DE, is directed on the ball of the thermometer, or on a tube containing water or other liquor required to be congealed.

Æther being very volatile, and having the remarkable property of increasing the bulk of air, does not require any aperture, through which the air might enter the bottle, in proportion as the æther goes out: the heat of the hand is more than sufficient to force the æther in a stream from the aperture D. After this manner, throwing the stream of æther on the ball of a thermometer in such quantity as that a drop of æther might now and then, for instance every 10 seconds, fall from the under part of the thermometer, I have brought the mercury down to  $3^{\circ}$ , viz.  $29^{\circ}$  below the freezing point, when the atmosphere was somewhat hotter than temperate, and that without blowing on the thermometer. When the æther is very good, viz. is capable of dissolving elastic gun, and the thermometer has a small bulb, not above 20 drops of æther are required to produce this effect, and about 2 minutes of time; but when the æther is of the common sort, a greater quantity of it, and a longer time, are necessary to be employed, though at last the thermometer is brought down very nearly as low by this as by the best sort of æther.

To freeze water by the evaporation of æther, I take a thin glass tube about 4 inches long, and about  $\frac{1}{8}$  of an inch in diameter, hermetically closed at one end,



and put a little water in it, so as to fill about half an inch length of it, as is shown at *CB* in the figure. Into this tube a slender wire *H* is also introduced, the lower extremity of which is twisted in a spiral manner, and serves to draw up the ice, when formed. Things being thus prepared, I hold the glass tube by its upper part *A* with the fingers of the left hand, and keep it continually and gently turning round its axis, first one way, and then the contrary; while with the right hand I hold the phial containing the æther in such a manner as to direct the stream of æther on the outside of the tube, and a little above the surface of the water in it. The capillary aperture *D* should be kept almost in contact with the surface of the tube that contains the water. Continuing this operation for 2 or 3 minutes, the water will be frozen as it were in an instant; since it will appear to become opaque at the bottom *B*, and the opacity will ascend to *c* in less than half a second of time, which exhibits a beautiful appearance. This congelation, however, is only superficial, and in order to congeal the whole quantity of water, the operation must be continued a minute or 2 longer; after which the wire *H* will be found to be kept very tight by the ice. Now the bottle with the æther is left on a table or other place, and to the outside of the glass tube the hand must be applied for a moment, to soften the surface of the ice, which adheres very firmly to the glass, and then pulling the wire *H* out of the tube, a solid and hard piece of ice will come out, fastened to its spiral extremity.

Instead of the wire *H* sometimes I put a small thermometer into this tube, so as to have its bulb immersed in the water. With this thermometer I have observed a very remarkable phenomenon, which seems to be not explicable in the present state of knowledge concerning heat and cold. This is, that water will freeze in the winter with a less degree of cold than it will in the summer, or when the weather is hotter: for instance, in the winter the water in the tube *AB* will freeze when the thermometer is about  $30^{\circ}$ ; but in the summer, or even when the temperature of the atmosphere is about  $60^{\circ}$ , the quicksilver in the thermometer must be brought 10 or 15, or even more, degrees below the freezing point, before the water which surrounds the said thermometer will be converted into ice, even superficially; hence it appears, that in the summer time a greater quantity of æther and longer time are required to freeze a given quantity of water, than in the winter; not only because then a greater degree of heat is to be overcome, but principally because in the summer a much greater degree of cold must be actually produced before the water that is kept in it will assume a solid form. When the temperature of the atmosphere has been about  $40^{\circ}$ , I have frozen a quantity of water with an equal weight of good æther, but at present, being summer, between 2 and 3 times the quantity of the same æther must be used to produce the same effect.



The proportion between the quantity of the ether and of the water that may be frozen by it, seems to vary according to the quantity of water; for a larger quantity of water seems to require a proportionably less quantity of ether than a smaller quantity of water, supposing that the water is contained in cylindrical glass vessels; for I have not tried whether a metal vessel instead of a glass one, and whether some other shape besides the cylindrical, might not facilitate the congelation. In the beginning of the spring I froze about a quarter of an ounce of water with nearly half an ounce weight of ether, the apparatus being larger, though similar to that described above. Now as the price of ether, sufficiently good for the purpose, is generally between 18 pence and 2 shillings per ounce, it is plain, that with less than 2 shillings a quarter of an ounce of ice, or ice cream, may be made in every climate, and at any time; which may afford great satisfaction to those persons who, living in places where no natural ice is to be had, never saw or tasted any such delicious refreshments.

When a small piece of ice, for instance, of about 10 grains in weight, is wanted, the necessary apparatus is very small, and the expense of the ether not worth mentioning. I have a small box, which is  $4\frac{1}{2}$  inches long, 2 inches broad, and  $1\frac{1}{4}$  inch deep, which contains all the apparatus necessary for this purpose, viz. a bottle capable of containing about 1 ounce of ether, 2 pointed tubes, in case that one should break, a tube in which the water is to be frozen, and the wire. With the quantity of ether contained in this small and very portable apparatus, the experiment, when carefully performed, may be repeated about 10 times. A person who wishes to perform such experiments in hot climates, and in places where ice is not easily procured, requires only a large bottle of ether, besides the small apparatus described above.

It is a known fact, that the moment a quantity of water becomes ice, a thermometer kept immersed in it, rises a few degrees, and accordingly this is observed in our experiment, viz. the mercury of the thermometer, which is immersed in the water of the tube AB, will suddenly rise, sometimes as much as 10 degrees, when the water becomes first opaque. Electrization increases very little the degree of cold produced by the evaporation of ether. Having thrown the electrified, and also the unelectrified steam of ether on the bulb of a thermometer, the mercury in it was brought down 2 degrees lower in the former than in the latter case.

As various persons may, perhaps, be induced by this paper to repeat such experiments, and as ether is a fluid which can with difficulty be preserved, it may be useful to mention, that a cork confines ether in a glass bottle much better than a glass stopple, which it is almost impossible to grind so well as entirely to prevent the evaporation of ether. When a stopple, made very nicely out of a uniform and close piece of cork, which goes rather tight, is put on a bottle of



ether, the smell of that fluid cannot be perceived through it; but I never saw a glass stopple that could produce the same effect. By opening the bottle very often, or by long keeping, the cork becomes loose, in which case it must be changed; and thus ether, spirit of wine, or any fluid, excepting those which corrode cork, may be preserved.

I shall now describe a method of purifying vitriolic ether, which is very easy and expeditious, though not very profitable: this method I learned of Mr. Winch, Chemist, in the Haymarket. Fill about a quarter of a strong bottle with common ether, and on it pour about twice as much water, then stop the bottle, and give it a shake, so as to mix for a time the ether with the water. This done, keep the bottle without motion, and with the mouth downwards, till the ether is separated from the water and swims over it, which requires not above 3 or 4 minutes of time; then open the bottle, and keeping it still inverted, let the greatest part of the water come out very gently; after this the bottle being turned with the mouth upwards, more water must be poured in it, and in short the same operation must be repeated 3 or 4 times. Lastly, all the water being separated from the ether by decanting it with dexterity, the ether will be found to be exceedingly pure. By this means I have purified common vitriolic ether, which could not affect elastic gum, and have reduced it into such a state as that elastic gum was easily dissolved by it. Indeed this purified ether appeared by every trial to be purer than I ever saw it, even when made after the best usual method, and in the most careful manner. The only inconvenience attending the process is, that a vast quantity of ether is lost. Not above 3 or 4 ounces of a pound of common ether remain after the purification. As the greatest part of the ether is certainly mixed with the water that is used in the process, it may perhaps be worth while to put that water into a retort, and to distil the ether from it, which must come sufficiently pure for common use. It is commonly believed, that water combines with the purest part of ether, when those 2 fluids are kept together; whereas, by the above described process, the contrary is established; perhaps when ether is kept in contact with water for a long time, the purest part of it may appear to be lost, because the ether may be combined with, and may retain some water in itself, at the same time that the water combines with and retains some ether; whereas the case may be different when the ether is quickly washed in water, and is immediately after separated from it: but in respect to this I have not yet made any experiments, so as to be able to decide the matter.

2. *Experiments relating to the expansion of mercury.*—The difficulty and uncertainty attending the various methods hitherto proposed for investigating the expansion of quicksilver, or its increase of bulk when rarefied by a given degree of heat, determined me to contrive some method by which this purpose might be



effected with more certainty and precision. After various experiments I hit on the following method, which seems both new and capable of great accuracy, though in this I may be deceived.

First, having blown a ball to a capillary tube, such as are commonly used for thermometers, I weighed it, and found that this empty thermometer was equal to 79.25 grains: This empty glass, previous to its being weighed, was rendered as perfectly clean as possible, which is a necessary precaution in this experiment, which depends on a very great accuracy of weight. I then introduced some mercury into the stem of this thermometer, taking care that none of it entered the ball, and, by adapting a scale of inches to the tube, observed that 4.3 inches length of the tube was filled with the mercury. The thermometer was now weighed again, and from this weight, the weight of the glass found before being subtracted, the remainder, viz. 0.24 gr. showed the weight of so much quicksilver as filled 4.3 inches of the tube. Now the ball of the thermometer, and also part of the tube, were entirely filled with quicksilver; then, to find out the weight of the mercury contained in it, the thermometer was weighed for the last time, and from this weight the weight of the glass being subtracted, the remainder, viz. 32.05 gr. showed the weight of the whole quantity of quicksilver contained in the thermometer.

By comparison with a graduated thermometer in hot and cold water, I made a scale to the new thermometer according to Fahrenheit's, and by applying a scale of inches found, that the length of  $20^{\circ}$  in this scale was equal to 1.33 inch. But 0.24 gr. was the weight of so much mercury as filled 4.3 inches length of the tube; therefore, by the rule of proportion it will be found, that the weight of so much quicksilver as fills 1.33 inch of the tube, viz. the length of  $20^{\circ}$ , is equal to 0.0742 gr. nearly, and that the weight of so much quicksilver as fills the length of the tube that is equivalent to  $1^{\circ}$ , is equal to 0.00371 gr. Now it is clear, that the weight of the whole quantity of quicksilver contained in the thermometer, is to the weight of so much quicksilver as fills the length of  $1^{\circ}$  in the tube, as the bulk of the whole quantity of quicksilver in a given degree of heat, to the increase of bulk that the same whole quantity of quicksilver acquires when heated of but  $1^{\circ}$ , viz. 32.05 gr. is to 0.00371 gr. as 1 is to 0.0011 +; so that by this experiment it appears, that  $1^{\circ}$  of Fahrenheit's thermometer increases the bulk of mercury not above  $\frac{1}{100000}$  parts. In this process a small deviation from mathematical exactness is occasioned by the small difference of weight between the quicksilver of the tube when first weighed and when it is afterwards heated to  $1^{\circ}$ ; but by an easy calculation it will be found, that this difference is so exceedingly small as not to be perceived by our exactest weighing and measuring instruments.

For clearness sake I shall subjoin the calculation of the above related experi-



ments, disencumbered from words. Here the decimals are not computed to a very large number, that being unnecessary for this purpose.

Weight of the glass . . . . . 79.25 grs.

Weight of so much quicksilver as filled 4.3 inches length of the tube, 0.24

Weight of the whole quantity of quicksilver contained in the therm. 32.05

Length of the tube equal to  $20^{\circ}$  . . . . . 1.33 inch.

$4.3 : 0.24 :: 1.33 : 0.0742 = 20^{\circ}$

$20^{\circ} : 0.0742 :: 1 : 0.00371$

$32.05 : 0.00371 :: 1 : 0.00011 + =$  to the expansion occasioned by  $1^{\circ}$  of heat.

Having repeated this experiment with other thermometers, and by similar calculations, each process gave a result little different from the others, which irregularity is certainly owing to the imperfection of my scales, which are not of the nicest sort: but taking a mean of various experiments it appears, that  $1^{\circ}$  of heat, according to Fahrenheit's thermometer, increases the bulk of a quantity of quicksilver by  $\frac{9}{100000}$  parts, viz. if the bulk of a quantity of quicksilver in the temperature of  $50^{\circ}$  is equal to 100,000 cubic inches, the bulk of the same quantity of quicksilver in the temperature of  $51^{\circ}$  will be equal to 100,009 cubic inches.

From these observations the method of graduating, or of determining the length of a degree in a new thermometer, is easily deduced, the only requisites for the calculation being the weight of a quantity of quicksilver, which fills a known length of the tube, and the weight of the whole quantity of quicksilver contained in the thermometer when filled. Suppose, for instance, that in making a new thermometer it be found, that the weight of so much quicksilver as fills 5 inches length of the tube is equal to 10 grs., and that the weight of the whole quantity of quicksilver contained in the thermometer weighs 300 grs. It is plain, that if the whole quantity of quicksilver weighs 300 grs., then  $\frac{9}{100000}$  parts of it must weigh 0.027 grs. But the weight of so much mercury as fills 5 inches of the tube is equal to 10 grs.; therefore, 0.027 grs. weight of quicksilver must fill 0.0133 inch of the tube, and this is equal to the length of  $1^{\circ}$ , or the double, treble, &c. of it is equal to 2, 3, &c. degrees.

By this means the scale may be made; that is, it may be divided into degrees, but the numbers cannot be added to them without finding which of those degrees corresponds with the freezing point or boiling point. Either the point of boiling or freezing may be found by experiment, or any other point may be ascertained by comparison with another thermometer, and then the other degrees are nominated accordingly.

3. *Description of a thermometrical barometer.*—The determination of the various degrees of heat shown by boiling water, under different pressures of the atmosphere, has been attempted by various persons, but it was lately completed



by the accurate and numerous experiments of Sir George Shuckburgh. His valuable paper is inserted in the 69th vol. of the Philos. Trans. On considering this paper, I thought it possible to construct a thermometer with proper apparatus, which, by means of boiling water, might indicate the various gravity of the atmosphere, viz. the height of the barometer. This thermometer, with the suitable apparatus, might, I thought, be packed into a small and very portable box, and I even flattered myself, that with such an instrument the heights of mountains, &c. might perhaps be determined with greater facility than with the common portable barometer. My expectations are far from having been disappointed, and though the instrument which I have hitherto constructed has various defects, I have however thought of some expedients which will undoubtedly render it much more perfect; I shall then present to this Society a more particular account of it, and also of the experiments which I intend to make with it. The instrument in its present state consists of a cylindrical tin vessel, about 2 inches in diameter and 5 inches high, in which vessel the water is contained, which may be made to boil by the flame of a large wax candle. The thermometer is fastened to the tin vessel in such a manner, as that its bulb may be about 1 inch above the bottom. The scale of this thermometer, which is of brass, exhibits on one side of the glass tube a few degrees of Fahrenheit's scale, viz. from  $200^{\circ}$  to  $216^{\circ}$ . On the other side of the tube are marked the various barometrical heights, at which the boiling water shows those particular degrees of heat which are set down in Sir G. Shuckburgh's table. With this instrument the barometrical height is shown within  $\frac{1}{16}$  of an inch. The degrees of this thermometer are somewhat longer than  $\frac{1}{9}$  of an inch, and consequently may be subdivided into many parts, especially if a nonius is used. But the greatest imperfection of this instrument arises from the smallness of the tin vessel, which does not admit a sufficient quantity of water: and I find, that when a thermometer is kept in a small quantity of boiling water, the quicksilver in its stem does not stand very steady, sometimes rising or falling even half a degree; but when the quantity of water is sufficiently large, for instance is 10 or 12 ounces, and is kept boiling in a proper vessel, its degree of heat under the same pressure of the atmosphere is very settled.

END OF THE SEVENTY-FIRST VOLUME OF THE ORIGINAL.

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*I. On a new Kind of Rain. By the Count de Gioeni, an Inhabitant of the 3d Region of Mount Etna. From the Italian. Vol. LXXII, Anno 1782. p. 1.*

The morning of the 24th inst. (April 1781) exhibited here a most singular phenomenon. Every place exposed to the air was found wet with a coloured



cretaceous grey water, which, after evaporating and filtrating away, left every place covered with it to the height of 2 or 3 lines; and all the iron-work that was touched by it became rusty. The shower extended from N.  $\frac{1}{4}$  N. E. to S.  $\frac{1}{4}$  S. W. over the fields, about 70 miles in a right line from the vertex of Etna. There is nothing new in volcanos having thrown up sand, and also stones, by the violent expansive force generated within them, which sand has been carried by the wind to distant regions. But the colour and subtilty of the matter occasioned doubts concerning its origin; which increased from the remarkable circumstance of the water in which it came incorporated; for which reasons some other principle or origin was suspected.

It became therefore necessary by all means to ascertain the nature of this matter, in order to be convinced of its origin, and of the effects it might produce. This could not be done without the help of a chemical analysis. To do this then with certainty, I endeavoured to collect this rain from places where it was most probable no heterogeneous matter would be mixed with it. I therefore chose the plant called *Brassica Capitata*, which having large and turned up leaves, they contained enough of this coloured water; many of these I emptied into a vessel, and left the contents to settle till the water became clear. This being separated into another vessel, I tried it with vegetable alkaline liquors and mineral acids; but could observe no decomposition by either. I then evaporated the water, to reunite the substances that might be in solution: and touching it again with the aforesaid liquors, it showed a slight effervescence with the acids. When tried with the syrup of violets, this became a pale green; so that I was persuaded it contained a calcareous salt. With the decoction of galls no precipitation was produced. The matter being afterwards dried in the shade, it appeared a very subtile, fine earth, of a cretaceous colour, but inert, from having been diluted by the rain.

I next thought of calcining it with a slow fire, and it assumed the colour of a brick. A portion of this being put into a crucible, I applied to it a stronger heat, by which it lost almost all its acquired colour. Again, I exposed a portion of this for a longer time to a very violent heat, from which a vitrification might be expected; it remained however quite soft, and was easily bruised, but returned to its original dusky colour. From the most accurate observations of the smoke from the 3 calcinations, I could not discover either colour or smell that indicated any arsenical or sulphureous mixture. Having therefore calcined this matter in 3 portions, with 3 different degrees of fire, I presented a good magnet to each; it did not act either on the first or second; a slight attraction was visible in many places on the third; this persuaded me, that this earth contains a martial principle in a metallic form, and not in a vitriolic substance.

The nature of these substances then being discovered, their volcanic origin



appears; for iron, the more it is exposed to violent calcination, the more it is divided, by the loss of its phlogistic principle; which cannot naturally happen but in the great chimney of a volcano. Calcareous salt, being a marine salt combined with a calcareous substance by means of violent heat, cannot be otherwise composed than in a volcano. As to their dreaded effects on animals and vegetables, every one knows the advantageous use, in medicine, both of the one and the other, and this in the same form as they are thus prepared in the great laboratory of nature. Vegetables, even in flower, do not appear in the least macerated, which has formerly happened from only showers of sand.

How this volcanic production came to be mixed with water may be conceived in various ways. Etna, about its middle regions, is generally surrounded with clouds that do not always rise above its summit, which is 2900 paces above the level of the sea. This matter being thrown out, and descending on the clouds below it, may happen to mix and fall in rain with them in the usual way. It may also be conjectured, that the thick smoke which the volcanic matter contained might, by its rarefaction, be carried in the atmosphere by the winds, over that tract of country; and then, cooling so as to condense and become specifically heavier than the air, might descend in that coloured rain. I must, however, leave to philosophers, to whom the knowledge of natural agents belongs, the examination and explanation of such phenomena, confining myself to observation and chemical experiments.

P. S. On Friday the 4th of May, about a quarter past 3 in the afternoon, a slight shock of an earthquake was felt in the country about Etna, which became more sensible at some distance from the mountain; its direction was from north to south. The volcano had continued its flames and explosions; and the night before, a column of smoke, composed of globes as it were piled on each other, had ascended over the crater to double the height of the mountain, as far at least as one could judge at the distance of 22 miles, which the vertex is in a right line from this city. This remained the whole night perpendicular, only one of the globes had separated and lengthened out to the westward from the summit. Now and then all the inside of the column, and of the lengthened outpart, became illuminated by electric fire, which was of a deep red colour, and gradually went out again, beginning at the bottom, in about 2 seconds. The fire has continued on the crater till this day, May 8th, ejecting red-hot masses or stones, which rolling beautifully down the cone, have illuminated this region; some lava has run over from the crater towards the W. N. W. but without having force enough to burst the sides or walls of the volcano.



*II. New Chemical Experiments relative to the Acid extracted from Fat. By Dr. Crell. An Abstract from the Latin. p. 8.\**

To obviate the objection that in the mode of obtaining the concentrated acid of fat by the action of the vitriolic acid on Segner's salt, some of the vitriolic acid might be volatilized with it; Dr C.

*Exper. 56*, put 3 oz. of Segner's salt (i. e. of the salt compounded of the vegetable alkali and the acid of fat) into a coated glass retort, and subjected the same to the open fire, gradually increased. A small quantity of water first came over, viz. the water of crystallization. When the heat was increased to such a degree that the retort began to be red-hot, there immediately rose up an abundance of grey vapour, indicative, as Dr. C. thought, of a strong acid; but on opening the vessels, after they had become cold, he perceived no fumes nor any of the smell peculiar to the acid; but rather the smell of spirit of tartar, with which the obtained fluid, weighing 11 dr., agreed in other respects, viz. in taste and colour; it effervesced slightly with salt of tartar. The residuum was an alkaline salt, with an admixture of carbonaceous matter, but without any trace of volatile alkali.

The method of obtaining the smoking acid of fat, *acidum pinguedinis fumans*, had been hitherto extremely tedious, 9 distillations, *exper. 1-9*, being required, besides rectification, *exper. 46*; after which the acid was to be saturated with an alkali, the solution to be evaporated, the salt thus obtained to be calcined, and then again to be dissolved and the solution to be evaporated, before the oil of vitriol could expel a pure acid from it, *exper. 53*. He was therefore anxious to obtain a pure acid of fat by a shorter process. Accordingly

*Exper. 57*, he distilled some purified suet in copper vessels lined with tin. On applying a gentle heat nothing rose up but water, but when the heat was increased, there followed a greenish fluid. At the same time the tin in various parts of the alembic, and especially in the tube adapted to it, was melted, and had penetrated to the outside. When the distillation was over, he found in the receiver the acid and oil, not as in the former experiments congealed, but in a fluid state, though the residuum was almost wholly converted into a coally matter. Thus he had discovered a shorter process; not only however was the acid contaminated with copper, but the vessels were so much damaged by the great degree of heat to which they had been subjected, that they would hardly serve for any other operations afterwards.

Abandoning therefore this method, which was not attended with the desired success, he thought of having recourse to a solution of suet in an alkaline salt, i. e. to soap. For it appeared to him highly probable, that the alkaline salt, at

\* See vol. xiv. p. 666, of these Abridgments.



the same time that it dissolved the fat, combined with its acid; so that if the oil of the soap could be separated from Segner's salt, he should then immediately get to that stage of the process, to arrive at which had cost him so much time and trouble in exper. 46. Now the separation of the oil appeared to him to be no difficult matter, seeing that soap is readily decomposed by every acid, as well as by some neutral salts; and that when decomposed, the oil might be separated by filtration from the watery fluid, and to the residuum left by the evaporation of this last, vitriolic acid might be added. Being aware, however, that common soap would not be proper, both because the lixivium with which it is prepared is not pure, and because common salt is added to separate the soap from the water, and in part unites with it, he made a soap for the occasion.

*Exper. 58.* With lb. ss. quicklime, lb. j. salt of tartar, and lb. vj. hot water, he prepared a caustic lixivium, which he afterwards strained through a thick linen cloth. Of this lixivium he took a 4th part, diluted it with a little water, and boiled it with lb. j. of suet, until most of the aqueous part being evaporated, the alkali and suet began to unite. The remainder of the lixivium was then added, and the boiling was continued with a gentle heat, the mixture being constantly stirred, until a transparent, and as it were mucilaginous compound was formed, which gelatinized as it became cold, and was exactly like common soap before common salt is added to it.

Having thus prepared a soap suited to his purpose, his next object was to separate the oil from the alkali, so as to leave the latter combined with the acid of fat, i. e. under the form of Segner's salt. And this he thought he could effect by means of alum. Accordingly,

*Exper. 59,* he dissolved the gelatinous compound obtained in the preceding exper. in water, and added some pulverized alum, which immediately caused the oil to rise up to the surface in a coagulated state. This being skimmed off, some more alum was added; and this was repeated 9 times, till no more oil rose up to the surface.\* The filtrated liquor was then evaporated to dryness.†

*Exper. 60.* It occurred to Dr. C. that alum would be the best chemical agent he could employ, for expelling the acid, unmixed with vitriolic acid, from Segner's salt. Accordingly, to 2 parts of Segner's salt he added 1 part of burnt alum, and subjected the mixture to distillation in a sand bath, with a strong heat. When the distillation was over, he found in the receiver a smoking acid, of the same nature with that which was obtained in exper. 53, and he was therefore

\* The proportions should be as follow: to 10 lb. of the soap-jelly dissolved in water add at different times 22 oz. of alum. This mixture being filtrated and evaporated, yields 21½ oz. of saline matter, consisting of vitriolated tartar, Segner's salt, and a portion of undecomposed alum.

† If this liquor be set by to crystallize, the vitriolated tartar and superabundant alum may for the most part be separated; and the remaining liquor may be afterwards evaporated.



pleased to see he had thus succeeded in shortening the process. Nevertheless he perceived that the acid thus obtained had somewhat of a sulphureous smell, whence he suspected that (contrary to Beaumé's assertion, Chym. Exper. 1, p. 365) by the strong degree of heat employed in the process, some of the vitriolic acid had been expelled from the alum. He therefore resolved to employ the ol. vitrioli, as in that case a less degree of heat would be required.

*Exper.* 61. On 3 parts of the saline mass \* pour 1 part of oil of vitriol; an extrication of grey fumes, with the smell of the acid of fat, will immediately follow: a gentle heat is sufficient for disengaging all the acid; for when a greater degree of heat is applied nothing is forced over into the receiver except a few drops of a reddish brown oil.

To ascertain whether the acid of fat thus procured was contaminated with vitriolic acid, Dr. C. added some of it (the acid of fat) to a solution of saccharum saturni; it threw down a precipitate which was not redissolved on adding wine-vinegar, even when boiled and digested therewith. Having thus detected an admixture of vitriolic acid, he thought it might be separated from the acid of fat, by distillation with a fresh quantity of the saline mass; in which case the vitriolic acid uniting with the alkali, would disengage the acid of fat.†

*Exper.* 62. Accordingly, to 4 oz. of the obtained acid he added a fresh portion (amounting to 1 oz.) of the saline mass, and distilled with a gentle heat. There passed over into the receiver a colourless smoking acid, some of which being added to a solution of saccharum saturni, it did indeed throw down a sediment, but this sediment was redissolved on adding wine-vinegar.

*Exper.* 63. Wishing to see how this concentrated acid would act on metals, he digested 4 gr. of gold, precipitated from its solution in aqua regis by vitriol of iron, in 1 oz. of the acid.—Another quantity of the acid was digested with gold-leaf; a third quantity with 4 gr. of platina; and a fourth quantity with silver leaf. In these exper. the acid which was before colourless, acquired a gold-colour. This he at first supposed to be owing to the actual solution of some particles of gold; but when he observed the same phenomenon to occur when the acid was digested with silver, he was then led to suspect that this change of colour was produced in the acid by the degree of heat alone.

*Exper.* 64-74. He distilled the colourless acid per se 8 different times. That portion which rose up into the receiver was pellucid, but the other portion in

\* The following are the best proportions: to  $\frac{3}{4}$  of the saline mass, *exper.* 59, note\*, add  $4\frac{1}{2}$  oz. of oil of vitriol; to the remaining  $\frac{1}{4}$  of the saline mass add the distilled acid, in order to rectify it. In this manner about 5 oz. of colourless smoking acid may be obtained.

† If this method be adopted, even pearl ashes may be used for making the soap; for by distilling the obtained acid over a fresh quantity of the saline mass, every kind of mineral acid mixed with the acid of fat, will be left behind in the said mass.



the retort was of a gold-colour, and a brown matter was deposited in circles at the bottom of the retort. The strength of the acid was impaired by these repeated distillations.

Being thus convinced that in exper. 63 the gold-colour which the acid acquired was no proof of any of the gold having been dissolved by it, he resolved to make other trials. Accordingly,

*Exper. 75.* He attempted to dissolve gold leaf and some grains of platina by digesting them in this acid for 6 weeks. On adding salt of tartar, no precipitation at first took place; but after subjecting the mixture to digestion, a precipitate was thrown down, which beingedulcorated and dried was of a white colour.\* This precipitate he suspected to be of an earthy nature; but the quantity was too small to allow him to ascertain to which species of earth it belonged. He supposes it to have been volatilized by the acid of fat.—The solution showed no signs of any metallic impregnation on adding Beguin's volatile tincture of sulphur.

*Exper. 76.* He digested for the space of a month 8 gr. of gold calx, obtained by salt of tartar, with  $\frac{1}{4}$  oz. of the acid of fat. The greater part of the calx remained undissolved at the bottom of the vessel. But on adding to the filtrated liquor, some of the volatile tincture of sulphur, a bluish grey-colour was immediately produced. The liquor being strained and the sediment on the filtre being dried, it appeared of a dirty yellow-colour, denoting the presence of gold. This however was more clearly proved, by evaporating a part of the solution, which then yielded some yellowish brown crystals, of an indeterminate figure.

*Exper. 77.* To promote the action of the acid of fat upon gold, he thought of mixing other acids with it. Accordingly to the same quantity of gold calx, he added 40 drops of acid of fat; with which in one vessel were mixed 20 drops of pure nitrous acid, in another 20 drops of spirit of salt. In the first vessel, bubbles of air were immediately extricated, denoting an incipient solution; in the second vessel no change took place. Both vessels were then subjected to a digesting heat, by which the solution in the first vessel was promoted, but in the second no traces of a solution appeared. Of each of these liquors 8 drops were added to 2 separate portions of a diluted solution of tin; the first instantly deposited a purple precipitate, the other only became a little turbid, without undergoing any change of colour.

*Exper. 78.* He was encouraged by this experiment to try whether this acid could not be made to dissolve gold in its metallic state. Accordingly to a bit of gold leaf he added 80 drops of the acid of fat and 20 drops of pure nitrous acid. Its surface was almost immediately covered with air-bubbles, and the solution

\* The same phenomenon was observed when salt of tartar was mixed and digested with acid of fat, previously digested with silver and bismuth.



went on gently and gradually; but on adding 20 drops more of nitrous acid, the solution was greatly promoted, and by subjecting it to a proper degree of heat, the whole portion of gold leaf was dissolved. This shows, he thinks, the difference between this acid and the muriatic acid; for 2 parts of smoking muriatic acid and 1 part of aqua-fortis will not, he says, dissolve gold, especially if a digesting heat be not applied. Hence he infers that the acid of fat is entitled to be classed with the stronger acids.

*Exper. 79.* Calx of platina, precipitated by salt of tartar from its solution in aqua regis, being treated in the same manner (*exper. 76*) was dissolved. One portion of the filtrated solution gave a dark coloured precipitate with Beguin's tincture, which when collected on a filtre and dried, was of a yellowish brown colour. The other portion of the solution being evaporated, yielded oblong yellowish brown crystals, in much greater quantity than the solution of gold had done.

*Exper. 80.* Silver leaf was slightly corroded by the acid of fat. By continued digestion the calx of silver was dissolved by it.

*Exper. 81.* It exerted but little solvent power over quicksilver in its metallic state; but

*Exper. 82.* Calx of quicksilver, obtained by means of salt of tartar from corrosive sublimate, was readily dissolved by the acid of fat, even without the assistance of heat. The solution being subjected to distillation, towards the end of the operation, when the heat was increased, a white sublimate attached itself to the neck of the retort. Dr. C. remarks that the acid of fat is the only acid, excepting the muriatic acid, which gives a dry sublimate with quicksilver; and what is singular, this sublimate is volatilized at a lower degree of heat than the muriatic sublimate of quicksilver.

*Exp. 83.* It dissolved copper, even when not assisted by heat; but more readily when subjected to digestion. The solution yielded crystals which deliquesced in the air.

*Exp. 84.* The solution of iron had an astringent taste; it yielded needle-like crystals, which scarcely attracted moisture from the air.

*Exp. 85.* Lead, in its metallic state, was rather corroded than dissolved by this acid; but it readily dissolved minium, and the solution yielded crystals, which had a sweetish taste.

*Exp. 86.* It dissolved regulus of antimony with the assistance of heat. The evaporated solution yielded crystals which did not deliquesce in the air.

*Exp. 87.* Zinc was readily dissolved. The solution had a strong metallic taste, and on adding salt of tartar, it let fall a white sediment, which (like the flowers of zinc) turned yellow on exposure to flame.

*Exp. 88.* Tin-filings were readily acted upon by this acid, and were converted



into a yellow powder. Half an oz. of the acid was sufficient to corrode 2 scr. of tin, with the assistance of heat. It emitted a very disagreeable smell, like that which is produced by the action of the muriatic acid upon zinc. The small quantity of turbid supernatant liquor could not be rendered clear by filtration through many folds of blotting paper. But after standing at rest for a short time, a yellowish powder was deposited, while the supernatant liquor, now become clear, appeared of a beautiful rose-colour.—This corroded calx of tin being digested in distilled water, and the water being afterwards filtrated and evaporated, a white deliquescent salt was obtained; and on adding to this salt a fresh quantity of the acid, the rose-colour was again produced, without any diminution of the quantity of sediment.

*Exp. 89.* Bismuth, in its metallic state, was not dissolved by this acid; but its calx was. When the solution was diluted with water, it became milky and deposited a white sediment; but it underwent no change on adding either the vitriolic or muriatic acid.

*Exp. 90.* Regulus of cobalt was not dissolved by it; but its calx was. From the solution distilled with nitre a salt was obtained, which being dissolved in water gave a sympathetic ink.

*Exp. 91.* It had very little action upon regulus of nickel; but it dissolved the calx of this metal, even without the assistance of heat. The solution was of a greenish colour, and suffered no precipitation on adding the vitriolic and nitrous acids.

*Exp. 92.* White arsenic was dissolved with difficulty, even when assisted by heat.

*Exp. 93.* The Ilfeld ore of manganese was dissolved by this acid. It first separated a black powder from the ore, and afterwards dissolved the ore itself, in considerable quantity. The acid, which acquired a brown colour by digestion with other metals, suffered no change by digestion with the manganese. The solution, which emitted a smell like that which is given out from a solution of tin, had a metallic taste, and was rendered somewhat turbid by the addition of water. When Beguin's tincture was added to the solution, it acquired a red colour, and the precipitate which it threw down very abundantly, being dried, was of the same colour.

*Precipitations of metals, dissolved in other acids, produced by commixtion with the acid of fat.—Exp. 94. Gold.* Having obtained some beautiful yellow crystals (not unlike in figure to the crystals of common salt) from a solution of gold in aqua regis, which solution had been exposed to the open air, Dr. C. dissolved them in distilled water and added the acid of fat to the solution, whereupon a yellow precipitate was thrown down. This precipitate beingedulcorated and dried, it afterwards attracted moisture from the air.

*Exp. 95. Platina.* This acid being added to a solution of platina in aqua



regis, an orange-coloured precipitate was thrown down. This precipitate beingedulcorated and exsiccated, it became of a yellowish grey colour, and was much less deliquescent than the precipitate from gold.

*Exp. 96. Silver.* The acid of fat being added to a solution of silver in nitrous acid, a grey-coloured precipitate, inclining somewhat to red, was obtained.

*Exp. 97. Quicksilver.* This metal was precipitated from its solution in nitrous acid by the acid of fat. But, what is very remarkable, when this acid was added to a solution of corrosive sublimate; in a short time the solution became milky, and deposited a white powder. This effect takes place sooner if the mixture be subjected to a digesting heat. Dr. C. thinks that this may serve as a test by which the acid of fat may be distinguished from other acids, and particularly from the muriatic acid. This white precipitate being washed and afterwards dissolved with the assistance of a digesting heat, in water, a piece of copper was whitened on being thrown into it. The evaporated solution gave a white residuum, which did not deliquesce in the air.

*Exp. 98. Lead.* The precipitate from a nitrous solution of lead, had the appearance of small needle-like crystals, which beingedulcorated, were readily dissolved in water, subjected to a digesting heat. The evaporated solution gave a powder which was but little deliquescent.

*Exp. 99. Bismuth.* The nitrous acid used for dissolving this metal, had been so much diluted, that when the solution was ended, a fresh addition of water occasioned no precipitation. But as soon as some drops of the acid of fat were added to the solution, a white powder was thrown down. This being washed and dissolved in water with a digesting heat, and the solution being afterwards filtrated and evaporated, a white residuum was obtained, which was very deliquescent.

*Exp. 100. Regulus of Antimony.* To a saturated solution of this metal in aqua regis, distilled water being added, it became turbid; after this was filtrated a fresh addition of water produced no further change in it; but when some of the acid of fat was poured into it, a white precipitate was immediately let fall, which was for the most part soluble in water; from which, by evaporation was obtained a residuum which attracted moisture from the air, and shot into small crystals.

*Exp. 101. Tin,* was precipitated from its solution in aqua regis by this acid. The precipitate was of a yellowish brown colour. Being washed and digested with water, it yielded a white salt, which was very deliquescent.

*Exp. 102. Copper.* No precipitate was obtained either from vitriolated copper or nitrated copper, by admixtion with the acid of fat.

*Exp. 103. Iron.* Nor from nitrated or vitriolated iron.

*Exp. 104. Zinc.* Nor from nitrated or vitriolated zinc.



*Exp. 105. Regulus of Cobalt.* Nor from this metal dissolved in the nitrous acid.

*Exp. 106. Regulus of Nickel.* Nor from this metal whether dissolved in the nitrous or muriatic acid.

*Exp. 107. Arsenic* dissolved in the nitrous acid, gave no precipitate on commixtion with the acid of fat.

*Exp. 108. Manganese* dissolved in the nitrous acid exhibited no change on admixtion with this acid.

*The action of different acids upon Segner's salt.\** It has been already shown that the vitriolic acid expels the acid from Segner's salt.

*Exp. 109. Nitrous acid.* Upon 2 drs. of Segner's salt, Dr. C. poured an equal quantity of double aqua fortis. No effervescence ensued. After subjecting the mixture to distillation, the fluid in the receiver had the taste peculiar to the acid of fat, but had somewhat of the smell of aqua fortis. But that the salt was decomposed and its acid let loose, was evident from the precipitation which took place on adding some of the distilled fluid to a solution of lead in nitrous acid.

*Exp. 110. Muriatic acid.* Equal quantities by weight of Segner's salt and muriatic acid being mixed together, and subjected to distillation; 2 drs. of acid of fat were obtained, which possessed its peculiar smell, and precipitated a white powder from corrosive sublimate.

*Exp. 111. Wine-Vinegar.* Of this, 6 drs. were poured upon 2 drs. of Segner's salt, and the mixture was subjected to distillation. The distilled fluid had the smell of vinegar, and produced no change in corrosive sublimate.

*Exp. 112. Fluoric acid* being added in equal weight to this salt, it very quickly united with it, and the compound appeared almost dry; being afterwards subjected to distillation with a strong heat, the fluid which passed over consisted of fluoric acid unchanged.

*Exp. 113. Salt of phosphorus.* Half an ounce of the sal phosphori dissolved in water, was added to 2 drs. of Segner's salt. At the beginning of the distilla-

\* Relative to the figure of this salt, Dr. C. remarks that, on the authority of Segner, he had asserted in the preceding paper, that it resembled the terra foliata tartari; but having afterwards prepared it in larger quantity, and examined it more attentively, he found that the saline mass was covered with a firm crust, to which, on removing it, there adhered many dagger-like crystals (pugionis quadrangularis forma) of which the 2 opposite sides were narrower than the others. These crystals were for the most part 3 lines in length. If there be no excess of alkaline salt and the crystals be dried on blotting paper, they do not deliquesce in the air: in which circumstance, as well as in the form of the crystals, this salt differs remarkably from the terra foliata tartari. Dr. C. thinks that Segner prepared so small a quantity of his salt, that he could not have an opportunity of observing the crystals concealed under the saline crust. Perhaps, too, his acid was not sufficiently freed from the oily particles, and he might not have used for saturating the acid any other alkaline salt than pearl ashes.



tion what passed over was merely water ; this being poured out of the receiver, the fire was increased ; but what rose up in distillation was not acid, nor did it decompound saccharum saturni.

*Exp. 114. White Arsenic.* Equal quantities of white arsenic and Segner's salt of rather a yellowish colour, were triturated together into a powder, and in order to promote their action on each other, 2 drs. of distilled water were added, and the mixture was digested with a gentle heat. In about a quarter of an hour, part of the powder or mass turned black, and adhered strongly to the sides of the vessel in the form of a black circle. Being afterwards subjected to distillation, only a small quantity of fluid was obtained, and that had no acid taste, nor did it give a precipitate when added to sugar of lead.

*Exp. 115. Nitrated Cobalt.* One drachm of Segner's salt was added to  $\frac{1}{2}$  oz. of a nitrous solution of cobalt, and the mixture was subjected to distillation, till all the fluid part was drawn off. The exsiccated salt in the retort was of a green colour, and when cold turned white ; being dissolved in distilled water, it exhibited a new species of sympathetic ink, not unlike the common sympathetic ink from cobalt, but inclining more to a yellow colour.

*Exp. 116. Sal ammoniacum animale.* Of the animal sal ammoniac (compounded of acid of fat and volatile alkali) 2 drs. were mixed with 15 grs. of the lapis hæmatites, and the mixture was subjected to sublimation ; when the operation was over, it was found that the sal-ammoniacum animale had sublimed unchanged, while the lapis hæmatites remained at the bottom of the retort. The result was the same when the operation was repeated with the addition of a small quantity of water.

*The action of the acid of fat on neutral salts.—Exper. 117. Nitre.* Two dr. of the acid of fat being poured upon 2 dr. of purified nitre, the latter was dissolved with some degree of effervescence or commotion. No sooner was the retort placed in the sand bath, than it was seen to be filled with a yellow vapour, the colour of which grew darker and darker till at length it turned red. The fluid in the receiver had the taste of the nitrous acid, with some admixture of the taste of the acid of fat ; and its action on silver showed that it did not consist of pure nitrous acid.

*Exp. 118. Common Salt.* Two drachms of this salt were dissolved in an equal weight of the acid of fat. Towards the end of the distillation, grey vapours were distinctly seen ; and when the distillation was finished, the smell of the fluid in the receiver was the same as the smell of the muriatic acid ; and he was induced to believe, by subjecting it to other tests, that the fluid in the receiver was the muriatic acid.

*Exp. 119. Terra foliata tartari.* The acid of fat being added in equal quantity to the terra foliata tartari, a slight effervescence ensued ; being subjected to



distillation, the fluid collected in the receiver was found to be vinegar.

*Exp. 120. Sal Mirabile Glauberi.* The acid of fat and this salt were mixed together in equal weights, and were afterwards subjected to distillation. The fluid collected in the receiver, in addition to the smell of the acid of fat, had something of a sulphureous smell mixed with it. On adding some of this distilled fluid to a solution of lead in acid fat, a white precipitate was thrown down; a proof that a small quantity of the vitriolic acid had been disengaged from the alkali with which it was before united. This Dr. C. attributes to the phlogiston still adhering to the acid of fat, by which phlogiston (according to his explanation) a portion of the vitriolic acid is rendered more volatile.

*Exp. 121. Tartarus tartarisatus.* The acid of fat being added to a solution of this salt in water, a copious precipitation took place. On pouring off the liquor, the sediment had the taste and other properties of cream of tartar.

Dr. C. concludes with some remarks on the relationship or affinity between the acid of fat and the muriatic acid. Both acids yield a dry ammoniacal salt with the volatile alkali, and with magnesia alba a deliquescent salt. Both precipitate silver and mercury from their solutions in other acids; and when water is added to a solution of regulus of antimony in either of these acids, a precipitation takes place. And further, when muriatic acid is added to a solution of silver or mercury in the acid of fat, nothing is precipitated. But there is a remarkable difference between them in other respects. For instance, the acid of fat combines intimately with oily substances; the salt which it forms with calcareous earth is not deliquescent; ether is easily prepared from it; and it throws down a precipitate from a solution of corrosive sublimate.

*III. Observations on the Bills of Mortality at York. By William White, M. D., F. A. S. p. 35.*

Mr. Drake, F. R. S., in his *Antiquities of York*, has given the number of births and burials for 7 years, from Aug. 5, 1728, to Aug. 5, 1735, inclusive. This gave a favourable opportunity of comparing our present state after an elapse of 45 years. In order to this, the different parish registers were carefully examined from Jan. 1, 1770, to Dec. 31, 1776, inclusive.

Table 1 shows the number of births and burials in York from Aug. 5, 1728, to Aug. 5, 1735, for all the several parishes, which collected together are, 2803 births, and burials 3488. The burials therefore exceeded the births by 685 in 7 years, or 98 annually.

Table 2 shows the number of births and burials from Jan. 1, 1770, to Dec. 31, 1776, inclusive: and these are, in like manner, 3323 births, and 3175 burials. Therefore decreased in burials 313, or  $44\frac{2}{7}$  annually; births increased 520, or  $74\frac{2}{7}$  ditto; births exceed the burials 148, or  $21\frac{1}{7}$  ditto.

The 3d table shows the number of births and burials, with the proportion of males and females, annually, from Jan. 1, 1770, to Dec. 31, 1776. The result of all is as follows:



Number of males born in 7 years 1666, or 238 annually.

Number of males buried in 7 years 1476, or 210 $\frac{2}{7}$  annually.

Number of females born in 7 years 1657, or 236 $\frac{4}{7}$  annually.

Number of females buried in 7 years 1699, or 242 $\frac{5}{7}$  annually.

The 4th table shows the mortality of the seasons: being for winter 918, spring 816, summer 682, autumn 759.

In order to find the number of inhabitants in any place, where, either from its bulk, or other reasons, a numerical survey cannot be obtained, 2 methods may be used. The 1st is, multiplying the number of houses by the medium of inhabitants in each. The 2d is, one recommended by Mons. Mohean, in a work, entitled, *Recherches et Considerations sur la Population de la France*. He found, by very laborious calculations, that the number of inhabitants may be known by the births, the latter being to the former nearly as 1 to 27.

By an account given into the House of Commons in March 1781, the number of houses in York subject to the new house-tax was 2285: if to those be added such as were too small to come under the tax, which may probably amount to one-third more, the total of the houses in York will be about 3000. This number multiplied by 4 $\frac{1}{4}$ , which is nearly the medium of people in a house, gives 12,750 for the number of inhabitants. By the 2d rule we have 12,798 for the number of inhabitants, being the result of 474, the average annual births, multiplied by 27. The remarkable coincidence of these methods of calculation makes it very probable, that if we estimate the number of inhabitants at 12,800, we shall not be far from the truth.

However this may be as to the exact number of inhabitants, it affects not the principal end of the present inquiry, which is to show how we are improved in population and healthfulness within 40 years past. To prove this, we must find the number of inhabitants in the year 1735, from tab. 1. We there find the average annual births to be 400; this multiplied by 27 gives 10,800 for the number at that time. This number divided by the average annual deaths 498, gives the proportion of deaths 1 in 21 $\frac{3}{4}$ . Such was the state of this city as to mortality 46 years ago.

Very different from this is our present situation, the proportion of deaths being now decreased to 1 in 28 $\frac{1}{4}$ , which is the quotient of 12,800, the number of inhabitants, divided by 453, the present average of annual deaths. This is certainly a great rise in the scale of healthiness. From being near as fatal as London we have become less so than many country places, as will appear from the annexed comparative view of the proportion of deaths in different places: viz. there dies every year, at

Vienna.....	1 in 19 $\frac{1}{2}$
London.....	1 in 20 $\frac{3}{4}$
Edinburgh.....	1 in 20 $\frac{2}{5}$
Berlin.....	1 in 21
Rome.....	1 in 22
Amsterdam....	1 in 22
Dublin.....	1 in 22
Leeds.....	1 in 22
Northampton...	1 in 26
Shrewsbury....	1 in 26
Liverpool.....	1 in 27 $\frac{7}{10}$
Manchester....	1 in 28
York.....	1 in 28 $\frac{1}{4}$



Hence in 1735, at York it would require  $21\frac{3}{4}$  years to bury a number equal to that of its inhabitants; but in 1776,  $28\frac{1}{4}$  years would be required for the same. One-third less die yearly now than in the former period; and we are certainly advancing still higher, for in 1777 the births were more than in any former year, being 516, the burials 464. As there is no settled manufactory here, there is little increase or decrease of the people by acquisition or emigration, and probably what may happen in either case is nearly balanced by the other.

It appears from tab. 4, that the summer season is by much the healthiest at York; autumn the next; then the spring; winter being by far the most fatal. Dr. Percival found much the same to be the case at Manchester. At Chester Dr. Haygarth says November was the most sickly month. It appears that our diseases are chiefly of the inflammatory kind, which physicians know to be the general attendants of the winter and spring months. The disorders of the summer and autumn are more particularly such as arise from putrescency and acrimony, such as slow and remitting fevers, dysenteries, choleras, and the like; those then, being with us the healthiest seasons, show, that we are not subject to putrid diseases. Dr. Wintringham has given an account of the weather and the corresponding diseases at York for sixteen years successively, in his *Commentarium Nosologicum*, to which learned work the curious reader is referred for further satisfaction on this subject.

Among the general causes of our increasing population and healthiness, we may enumerate the introduction of inoculation, which has been the means of saving a number of lives; improvements in the treatment and cure of several disorders, the cool regimen in fevers, the admission of fresh air, the general use of antiseptic medicines and diet, have doubtless had a salutary and extensive influence on the health of mankind, and have much obviated the malignity of some of our most dangerous diseases. To these may be added a general improvement and greater attention to nature in the management of infants.

After the general causes of healthiness, such as are particular, or of a more local nature, come under consideration. In this respect the city of York has been much improved within a few years past. The streets have been widened in many places, by taking down a number of old houses built in such a manner as almost to meet in the upper stories, by which the sun and air were almost excluded in the streets and inferior apartments. They have also been new paved, additional drains made, and, by the present method of conducting the rain from the houses, are become much drier and cleaner than formerly. The erection of the locks, about 4 miles below the city, has been a great advantage to it: for, before this, the river was frequently very low, leaving quantities of sludge and dirt in the very heart of the city, also the filth of the common sewers which it



was unable to wash away. The lock has effectually prevented this for the future, by the river being kept always high, broad, and spacious; and has thus contributed to the salubrity as well as beauty of York.

*IV. Account of a monstrous Birth. In a Letter from John Torlese, Esq., Chief of Anjingo, to the Hon. William Hornbey, Governor of Bombay. Dated April 5, 1780. p. 44.*

As I know you are curious with respect to the productions of nature, I have taken the liberty to inclose you a drawing of a child which a Nair woman was delivered of the 28th of March at midnight, and which lived till the 1st of April in the morning. In the afternoon I went to see it in company with Mr. Hutchenson and Dr. Crozier. It had but 1 body, at the extremity of which were 2 heads, 1 larger than the other. It had 4 hands and arms perfect, 2 legs on 1 side its body, and 1 on the other, which began on the middle of its back, and appeared by nature intended for 2 by its size and from the appearance of the foot, which looked as if 2 had been squeezed or rather mashed together. It had but 1 navel, and 1 anus, but 2 genitals of the female. It was fed during its short existence by hand with goat's milk. It is remarkable, that 1 head would sleep while the other was awake; or 1 would cry and the other not. They both died at the same instant.

*V. Experiments with Chinese Hemp-Seed. By Keane Fitzgerald, Esq. p. 46.*

A few grains of Chinese hemp-seed had been given to me by the late Mr. Elliot, brother to Gen. Elliot, who had formerly resided for some time in China. He told me, the hemp in that country was deemed superior to that of any other, both for fineness and strength, and wished I would try whether it would come to maturity in this kingdom. He gave me between 30 and 40 grs. of seed for the purpose, which I laid by, as I thought, carefully, with intent of sowing them the spring following, which is the usual time of sowing hemp in this country; but I had unluckily forgotten where I laid them, and did not find them till the beginning of last June, by which time I imagined them to be very unfit for vegetation; but as I concluded they would be still more so by keeping them till the succeeding April, I had them sowed the 4th day of that month, and was much surprized to find that 32 of the seeds had vegetated strongly, and grown to an amazing size, several of the plants measuring in height more than 14 feet, and nearly 7 inches in circumference, by the middle of October following, at which time they came into bloom. There were from 30 to 40 lateral branches on a plant; these were set off in pairs, one on each side of the stem pointing horizontally; the others at about 5 or 6 inches distance from them,



pointing in different directions, and so on to the top, the bottom branches of some measuring more than 5 feet, the others decreasing gradually in length towards the top, so as to form a beautiful cone when in flower, which were unluckily nipped by a few nights frost that happened to be pretty sharp towards the end of the month; and the plants began to droop at the beginning of November, at which time I had them pulled up by the roots.

As I was but little acquainted either with the cultivation of the seed, or preparing the plants afterwards for the production of hemp, and as these plants were very different in their size from any I had ever seen, the best method that occurred to me was, that of steeping them in water, where I let them remain for a fortnight, and then placed them in an upright position against a south wall to dry and bleach. On trying whether the hemp could be easily separated from the woody part, I was agreeably surprized to find, that on peeling a few inches longitudinally from the root, the whole rind, from the bottom to the top, not only of the stem but also of all the lateral branches, stripped off cleanly, without breaking any one of them. The toughness of the hemp seemed to be extraordinary, and on drying and beating divides into an infinity of tough fibres. The plants when stripped are quite white, and when the lateral branches are cut off, appear like handsome young poles. They are perforated in the middle, but the perforation is not larger than that of a goose quill, in a stem of more than 2 inches diameter. The woody part seems pretty substantial, and if they should be found of any duration, might be applied to many useful purposes; or if not, I should imagine they would produce plenty of good ashes by burning. The rough hemp that has been peeled from the 32 plants, when thoroughly dried, weighed 3 pounds and a quarter; but I do not think it had come to full maturity, though I can hardly doubt but the plants would have come to perfection if the seed had been sown in the proper season. The summer was remarkably dry, notwithstanding which; though the situation they were placed in was very warm, and the ground not rich, I found, on measuring the plants at different times, that they had grown nearly 11 inches per week.

As the culture of so valuable a kind of hemp, as this promises to produce, appears to be of consequence to a maritime and commercial kingdom, I have applied to the directors of the East-India Company, to give proper orders to their factors and super-cargoes in China, to procure some of the best seed that can be obtained; and send even a small parcel, by each of their returning ships, which they have very obligingly promised; and from what has already appeared, there can be no doubt of its continuing in a state fit for vegetation for a much longer time than is usually required for that voyage.

If the seed should arrive in safety, I can hardly doubt of obtaining the assistance of the society established for the encouragement of arts, manufactures,



commerce; and should expect from their wonted assiduity and liberal disposition of proper rewards for the culture and manufacture of so valuable a commodity, to see it as successfully carried to perfection as several other branches have happily attained by their care and protection; and shall think myself very happy in being any ways instrumental in forwarding so good a purpose.

*VI. On some Scoria from Iron Works, which resemble the Vitrified Filaments described by Sir William Hamilton. By Samuel More, Esq. p. 50.*

In the account given of the eruption of Mount Vesuvius in August, 1779, by Sir Wm. Hamilton, printed in the Philos. Trans., vol. 70, p. 42, et seq. among many other equally curious informations, it is said, "Long filaments of vitrified matter, like spun-glass, were mixed with and fell with the ashes." And in a note annexed it is also said, that "during an eruption of the volcano in the isle of Bourbon in 1766, some miles of country, at the distance of 6 leagues from the volcano, were covered with a flexible capillary yellow glass, some of which were 2 or 3 feet long; with small vitreous globules at a little distance one from the other."

There appeared to me, on reading these passages, an exact similarity between these productions of the 2 volcanos and some scoria I had received from a worthy friend, who is master of one of the largest works in England for smelting iron. In a letter accompanying the specimen, he writes, "I have sent a specimen of some slag, or vitrified cinder, which has by the reverberation of the blast from the Tweer,\* been drawn out while fluid into long cobweb-like threads, sometimes 10 or 12 feet in length, and affixed itself to the beams, &c. of the bellows room."

Whoever has attentively viewed the large furnaces where iron ore is smelted by coke, will readily allow, that they present the most striking resemblance, however diminished, of that most tremendous of all appearances, the eruption of a volcano; and that the most exact pictures hitherto seen of the flowing of the lava from the one, is shown by the running of the slag from the other: this has induced me to send, for the inspection of the R. S., some of the scoria in its capillary state, and with all due deference to the acknowledged abilities of Sir William Hamilton, to submit, whether the fine filaments may not be produced in the eruption of the great furnaces of nature, by means similar to those by which we see them formed in the furnaces of art. Sir William seems to think, "That what he calls the natural spun glass which fell at Ottaiano, as well as that which fell in the Isle of Bourbon in 1766, must have been formed, most probably, by the operation of such a sort of lava as has been just described (that is, perfectly vitrified) cracking, and separating in the air at the time of its

\* The Tweer is that opening through which the air is driven by the bellows into the body of the furnace.



emission from the volcanos, and by that means spinning out the pure vitrified matter from its pores or cells; the wind at the same time carrying off those filaments of glass as fast as they were produced."

That some of the fine filaments found after the eruptions of the volcanos were formed in this manner is not unlikely: but as we see about the iron furnaces the vitrified scoria drawn into fine threads, of very considerable length, by the simple action of the wind from the bellows, is it not very probable, that the far greater part at least of those filaments scattered over the land, and which were found 2 or 3 feet long, were drawn out before the ejection of the lava from the crater by the force of those violent torrents of wind which must be required to support and actuate so intense a fire as at those times fills the body of the mountain? The extreme fineness to which these filaments are reduced, and their brittleness, render it almost impossible to convey them to any distance, preserving at the same time any considerable length of the fibres; these now sent resemble cotton in appearance, but if examined with a microscope will be found in all respects similar to those described by Sir W. Hamilton.

*VII. An Extract of the Register of the Parish of Holy Cross, Salop, being a Third Decade of Years from Michaelmas 1770 to Michaelmas 1780, carefully digested in the following Table. By the Rev. Mr. William Gorsuch, Vicar p. 53.*

		1771	1772	1773	1774	1775	1776	1777	1778	1779	1780		
Baptized	Males	23	20	19	20	18	31	16	18	20	18	203	} 385
	Females	16	18	16	12	23	14	17	27	22	17	182	
Buried	Males	16	19	12	11	13	28	13	12	23	13	160	} 311
	Females	13	20	17	10	6	21	14	15	13	22	151	
Increase												74	

	Died in the 10 years.				Died in the 10 years.		
	Male.	Fem.	Total.		Male.	Fem.	Total.
Under a month	11	11	22	40 to 45	7	4	11
From mo. to 1 yr.	15	23	38	45 .. 50	3	5	8
1 to 2	11	11	22	50 .. 55	11	8	19
2 .. 5	17	15	32	55 .. 60	5	4	9
5 .. 10	7	7	14	60 .. 65	13	9	22
10 .. 15	1	2	3	65 .. 70	3	3	6
15 .. 20	1	2	3	70 .. 75	11	15	26
20 .. 25	4	2	6	75 .. 80	9	7	16
25 .. 30	8	4	12	80 .. 85	12	7	19
30 .. 35	3	4	7	85 .. 90	1	1	2
35 .. 40	8	3	11	90 .. 95	0	4	4

An actual survey was made in 1775, when the number of the inhabitants was found to be 1057: of which there were under ten 287, and above seventy 57, viz. from 70 to 75, males 12 females 10 = 22. From 75 to 80, males 8 females 11 = 19. From 80 to 85, males 8 females 6 = 14. From 85 to 90, males 1 females 1 = 2.



An actual survey was made in the year 1780, when the number of inhabitants were 1113. There remains alive in 1780, under 10 years of age, males 155 females 138 = 293. From 70 to 75, males 6, females 11 = 17. From 75 to 80, males 5, females 8 = 13. From 80 to 85, males 2, females 4 = 6. From 85 to 90, males 2, females 1 = 3.

The number of inhabitants actually surveyed every 5 years for 30 years, as annexed. The increase of 48 persons in the year 1765 was owing to the ingress of 4 numerous families into large houses, which were almost uninhabited for many years before.

In	1755	.....	1049
	1760	.....	1048
	1765	.....	1096
	1770	.....	1046
	1775	.....	1057
	1780	.....	1113

The decrease of 50 persons in the year 1770, was occasioned by the demolishing of 9 houses, in order to open a way to the new stone bridge built over the river Severn.

*VIII. An Experiment proposed for determining, by the Aberration of the Fixed Stars, whether the Rays of Light, in pervading different Media, change their Velocity according to the Law which results from Sir Isaac Newton's Ideas concerning the Cause of Refraction; and for ascertaining their Velocity in every Medium whose refractive Density is known. By Patrick Wilson, A. M., Assistant to Alex. Wilson, M. D., Professor of Practical Astronomy in the University of Glasgow. p. 58.*

On the supposition that the refraction of light is caused by a certain action of gross and sensible bodies on it, Sir Isaac Newton has demonstrated, that the sines of incidence and refraction, when the rays pass out of one medium into another of different density, must always be in a constant ratio. This constancy of the ratio of the sines is agreeable to universal experience, and has been called the law of refraction. On the same grounds he has also demonstrated, that the velocity of the rays must be greater in the more refracting medium in the inverse ratio of the sines. Of this property of refraction however, we have hitherto had no evidence in the way of experiment. The ideas entertained by Sir Isaac from which this property has been deduced, though they confess their great author, by a most beautiful simplicity, and by a very striking agreement with fact, have yet been deemed by some persons as not perfectly authentic. His contemporary Leibnitz and others have attempted demonstrations of the law of refraction from principles very different, and which do not lead to the opinion of the acceleration of light in the more refracting medium. At present it is proposed to point out a method of determining experimentally the law of the variation of the velocity of light, according to the change of the medium. If observations shall shew this law to be agreeable to Sir Isaac's conclusions, we shall then have a very strong additional evidence in favour of his principles. If,



contrary to the most probable issue of the experiment, some unsuspected law should be discovered, we must, according to the rules of induction laid down by that great master in philosophy, so far restrict our general conclusions, and accommodate our ideas to the real condition of things.

The method of experiment at present alluded to, is that of observing the aberration of the fixed stars with a telescope filled with a dense fluid, such as water, or any other equally limpid and of greater refraction, fitted to bring the rays to a focus by the surface of the medium opposed to the object having a proper degree of convexity. Suffice it at this time to suggest a general notion of the instrument; and proceed we now to explain in what manner it can assist us in the present inquiry.

Since aberration, taken in its enlarged sense, depends on the relative velocities of light and of the telescope, if the rays were really to move much faster or much slower in an unusual telescope of this kind, it seems to follow, that the quantity of aberration given in these circumstances, compared with Dr. Bradley's angle, would certainly indicate the new rate of velocity. Such an inference would certainly be just, and it is on these grounds that we propose to inquire into the velocity of the rays, as they move forward in dense media so applied to telescopes. Granting however, for the sake of argument, that light moves down through such an unusual telescope with an increased velocity, suited to the refractive density of the medium, it will by no means happen, that the aberration will be changed on that account. This proposition, which at first view may appear paradoxical, and even contradictory to what has been affirmed above, is however not the less certain, and may serve to show what caution is sometimes requisite in applying general principles to particular cases: for it will be proved, that the aberration in such a telescope will precisely agree with that of Dr. Bradley's only in the case of the rays moving swifter in the watery medium than in air, in the ratio assigned by Sir Isaac Newton, and that this sameness of aberration will itself be a proof of light being so accelerated within the telescope. In the illustrations which follow, the reader is supposed not to be wholly unaccustomed to the distinctions between absolute and relative motion, as this will prevent repetitions and all unnecessary prolixity.

Let ABC (fig. 13, pl. 3,) be the spherical refracting surface of such a telescope as has been described, and let the telescope be supposed to be at rest, or the velocity of light to be infinite with respect to that of the earth, and let GBMF be a line drawn from a star at G, in the pole of the ecliptic, through the centre M of the refracting surface; the image of the star will be formed somewhere, as at F, in the line BF; and here the intersection of the cross wires used in observing must be placed. It is evident, that the star will be seen in its true direction FG; and we must conclude that to be its true direction, because we know that the



ray GBF passes into the medium without being refracted by it, and BMF would be considered as the axis of the telescope.

Now let the spherical refracting surface with its wires, or the unusual telescope, be carried laterally with the motion of the earth towards  $a$ . Conceive GBF to be a line not partaking of this lateral motion, which at any particular moment passes through  $M$ , the centre of convexity. Along this line suppose one of many rays to pass from a star situated in the pole of the ecliptic. Then will all the contemporary light of this pencil of parallel rays be made to converge, so as to meet in a focus somewhere in the unrefracted ray BF. Let  $F$  therefore be the point in absolute space where the image of the star is so formed. Let the parallel motion of the telescope, whose refracting spherical surface is ABC, be in the direction of HF, and take FD to FB as the lateral velocity of the telescope to the velocity of light in air, and join BD: then it is manifest that BD will be the position of a telescope such as Dr. Bradley's, when the image of the star is formed in the axis BD, and that IBG, or its equal FBD, will be the angle of greatest aberration.

Also, the velocity of the rays, as they proceed to the focus  $F$ , after refraction at the surface ABC, being supposed the same as in air, it is evident, that the line DML drawn through  $D$  and the centre of convexity  $M$ , must give the position of the axis of this kind of telescope, when the image of the star is formed there: for, by the hypothesis, the image is formed at  $F$  in absolute space; and since BF is supposed to be to FD, as the velocity of light within the medium, to the lateral velocity of the telescope, the point  $D$  of the axis DL will arrive at  $F$ , when the rays arrive there to form the image. And the observer not knowing, or at present not taking account of, the lateral motion of the telescope, will suppose, that the line LMD, joining the image of the star and the centre of convexity  $M$ , is the true direction of the star; just as before he concluded, that FMBG would be the direction of the star when the lateral motion of the telescope was supposed to be nothing. Hence it is evident, that the intersection of the cross wires, used in observing, must now be placed at  $D$ ; or else, if those be still used that were before supposed to be at  $F$ , the refracting surface ABC, with the line or axis BF, must revolve about the centre  $M$ , till the vertex  $B$  comes to  $L$ , and the cross wires  $F$  to  $D$ .

In like manner, if the velocity of the rays were increased after refraction at the spherical surface in any ratio, as that of DF to EF, the refraction continuing the same, then EMO, drawn through the centre of convexity, would now give the position of the axis of the telescope necessary for receiving the image formed at  $F$ . For the space described by the rays in passing downwards to the focus, in this case and the former being equal, the times of their converging at  $F$  will be reciprocally as the velocities, or as EF to DF. But, on account of the



equable lateral motion of the telescope,  $DF$  and  $EF$  will be as the times of the points  $D$  and  $E$  arriving at  $F$ : therefore, in the last case, the intersection of the cross wires, supposed at  $E$ , will meet the image at  $F$ , and accordingly the star will be seen in the axis.

From what has been said it will appear, that if  $DF$ , fig. 14, be taken to  $EF$ , as the sine of incidence to the sine of refraction peculiar to the medium which fills the telescope; then, from the property of the focus, we shall have this proportion, viz.  $BF : FM :: DF : EF$ . Hence the line  $EMO$ , passing through  $M$ , must be parallel to  $DB$ ; but  $DB$ , as before, denotes the position of Dr. Bradley's telescope, when the aberration of the star is at its maximum, and  $EMO$ , parallel to it, denotes the position of the water telescope, at the same time, on the supposition that the velocity of the rays without and within are as  $EF$  to  $DF$ , or inversely as the sines of incidence and refraction peculiar to water. Here then we discover what must be the law of variation as to the velocity of the rays, provided that the aberration given by such a telescope shall come out the same with that found by Dr. Bradley. It is the very same which follows from the Newtonian principles: for from the manner of observing, the angle of aberration is always determined by the position of the telescope necessary for having the image formed somewhere in the axis.

But supposing that in the course of observing with such a telescope, the aberration should come out different from what has already been ascertained by Dr. Bradley, it may next be inquired how, from the difference given, the velocity of light within the telescope is to be deduced. (Fig. 15.) Imagine then such a telescope actually to give  $FMD$  as the greatest angle of aberration, and let this be supposed greater than that of Dr. Bradley's, which, for example, let be  $FME$ . From what has been already said, the velocity of light corresponding to this last mentioned angle, is deducible from the known refraction of the medium which fills the telescope; and, by construction, the velocity corresponding to  $FMD$ , the angle given, must be to the former, inversely as the tangents of these angles. From this consideration we have the following analogy, for finding the velocity corresponding to whatever difference there may be observed between the two aberrations at present alluded to. The rule in all cases must be; "as the tangent of the observed angle is to the tangent of the Bradleyan angle, so is the velocity of light deducible from the hypothesis of the observed angle being the same with that of Dr. Bradley, to the velocity sought." It has already been shown, how the former of these velocities can be universally ascertained, from the known refraction of the medium which is taken to fill the telescope, and therefore the last term of the above proportion, which is the velocity sought, is thereby given.

In a telescope of this kind it will not have escaped notice, that the ray  $BF$ ,



fig. 14, which, on account of its passing to the focus unrefracted, may be called the axis of the pencil, can never be found in the axis of the telescope  $EO$ , except at the focus  $F$ , where  $D$  and  $F$  meet. That ray however  $OP$ , parallel to  $BG$ , which falls obliquely on the axis of the telescope  $EO$ , will continue to pass along it after refraction, and for that reason it may be called the relative axis of the pencil. This will appear, by considering that the particle of light, which at any moment is refracted at the vertex  $O$  of the spherical surface, is found by hypothesis in the axis a second time, when it meets the contemporary light at the focus. But since the motion both of the axis and of the particle is uniform and rectilinear, the former cannot be found in the latter at 2 different times, without being found in it continually during the whole interval. In like manner, a part of every other ray from the star, which successively falls on the vertex, must move relatively along the axis after refraction: and thus a constant succession of these particles constitute a visual refracted ray, whose relative path must always be in the axis  $OE$ .

All that has been shown concerning the telescope already considered, will receive still further illustration, by tracing the motion of this particular refracted ray till it arrives at the focus. This way of viewing the subject will also render the reasoning more general, and make it apply to telescopes when the dense fluid within is supposed to be confined by object-glasses of any figure. But in order to this, it will be convenient to premise, and briefly to demonstrate, what shall afterwards be referred to by the name of

*Prop. A.*—If any very small body or particle of light, as it moves uniformly in the absolute path  $SB$ , fig. 16, has passed relatively along a part of the line  $CD$ , which advances equably and parallel to itself in the direction  $DK$ ; and if at any instant the absolute path of the particle be changed into any other, as  $BR$ ; then it will still pass relatively along the moving line, provided its velocity now be to its former velocity, as the sine of the angle  $DBF$  to the sine of the angle  $DBR$ ; these being the angles which the moving line  $BD$  makes with  $BF$  and  $BR$ , the absolute path or direction of the particle in the two cases.

The construction of this figure is so simple, that it is unnecessary formally to point it out. Since, by hypothesis, the velocity of the particle along  $BR$  is to its former along  $BF$ , as the sine  $FZ$  to the sine  $RT$ ; or, on account of similar triangles, as  $DF$  to  $IR$ , and, on account of parallels, as  $DF$  to  $DW$ , it follows, that the time of its describing  $BR$  now, is to the time of formerly describing its equal  $BF$ , as  $DW$  to  $DF$ . But the line  $BD$  advancing with a uniform motion, the time of its arriving at  $w$  is to the time of its arriving at  $F$ , also as  $DW$  to  $DF$ . Therefore, when the particle arrives at  $R$ , the point  $D$  of the moving line will have arrived at  $w$ , and  $wRP$  will be its position. Hence the particle at that moment must be found in the intersection  $R$  of this line, with its absolute path  $BR$ . In



the same manner it may be shown, that at any other time the particle will be found in the intersection: therefore, from the time of its direction being changed at B, it must pass relatively along the moving line as before. By a small alteration in the construction it may be shown, that if the absolute path had been so changed at B as to have augmented the angle  $FBD$ , still the particle would have moved relatively along  $DB$ , provided its velocity after had been to its velocity before, as the sine of  $FBD$  the first angle, to the sine of the increased angle.

To apply therefore this proposition to the present investigation, let  $DB$  be conceived as the axis of a telescope perpendicular to the spherical surface of a refracting medium which accompanies it in its lateral motion,  $SB$  the absolute path of a particle of light which had passed relatively along  $DB$  produced, till its arrival at B, and  $BR$  its absolute path within the medium of the telescope. Then it is evident that  $FBD$ , or its equal  $CBS$ , will be universally the angle of incidence, and  $RBD$  the angle of refraction. Hence, by prop. A, that ray of the parallel pencil which is refracted at  $o$ , the vertex of the spherical surface in fig. 14, must still pass relatively along the axis, provided the velocity within the telescope be to its former in air, as the sine of incidence to the sine of refraction. But the image of the star being produced by the meeting of all the contemporary light, will consequently be found in the axis, which, by hypothesis, deviates from the true place of the star by the same quantity as Dr. Bradley's angle; so that in this way of considering the matter, the same thing results which was formerly shown in regard to a telescope so constructed.

By prop. A it is also manifest, that whatever number of refractions that ray which falls on the extremity of the axis suffers, in pervading object-glasses of any figure, or even dense media beyond the object-glass, if bounded by transparent planes to which the axis produced is perpendicular, yet if the velocities and refractions so correspond, still the ray in question will pass relatively along the axis till it meet the rest at the focus: for here the refracted ray in the first medium becomes the incident ray in relation to its path in the 2d, and this in its turn becomes an incident ray in relation to its path in the 3d medium, &c. and therefore, by the prop. A, can never deviate from the moving axis, whatever be the refractive density of the media, or however these are disposed in the order of succession. And since, by Sir Isaac Newton's theorem, the ratio of the sine of incidence to the sine of refraction, in the passage of a ray out of one medium into another, is compounded of the ratio which the former has to the latter, in the passage of that ray out of the first medium into any third, and of the ratio of the former to the latter in the passage of the same ray out of the 3d medium into the 2d, &c. it follows, that if the velocities be related to the degree of refraction as before-mentioned, the ray in the last dense medium will, notwith-



standing any number of previous refractions by glasses, &c. have the same final velocity that would have been acquired on its passing immediately out of air into that medium. This being the case, it appears, that though the intervention of an object-glass may shorten the focal distance of such a telescope, yet it will not displace the image nor alter the rule of inferring the final velocity of the rays in the dense medium from the aberration given; at least when this is supposed to be the same with Dr. Bradley's.

But further, if the aberration of such a telescope should differ from the Bradleyan one, and give, for example, the angle  $OMB$ , fig. 15, still the ray  $PO$ , which falls on  $O$  the vertex, must be considered as an incident ray, which, after refraction, passes along the axis. By prop. A therefore, the velocity of the ray, whatever this may be after refraction, must be to that velocity by which it would have moved relatively in the axis, so inclined to its path, previous to the refraction, inversely as the sines of incidence and refraction. Now this being duly considered, it will be found that the velocity within the medium, corresponding to this supposed aberration, or the absolute velocity within the medium, must be to the velocity within the medium corresponding to the Bradleyan aberration, inversely as the tangents of these two angles: for let  $v$  and  $v$  express the velocities before and after refraction corresponding to the Bradleyan angle, and  $x$  and  $x$  the velocities before and after corresponding to the supposed uncommon angle,  $x$  being the actual velocity after refraction; then, because by prop. A the antecedent is to the consequent, in both cases, in the same ratio, viz. as the sine of refraction to the sine of incidence, it will be  $v : v :: x : x$ , and therefore  $v : x :: v : x$ . But from the nature of the aberration  $v$  must be to  $x$  (this supposititious velocity before incidence) inversely as the tangents of the angles of the two aberrations. This therefore must be the ratio of  $v$  to  $x$ . But  $v$  is given, as before shown; therefore  $x$  the velocity within the medium corresponding to the supposed observed aberration is also given, and by the same rule as was found formerly in the case of the first telescope.

What has been at present advanced is unconnected with any hypothetical notions concerning the rays or the cause of refraction. Light has been considered only as something which moves uniformly from one place to another; and which is always refracted according to a known law. The first of these properties has been put beyond all doubt by the observations of Dr. Bradley and Mr. Molyneux; and it has been long known that the last is quite agreeable to experience.

It has indeed always been taken for granted, that the velocity of the ray which passes through the centre of convexity, represents the common velocity of all the contemporary light of the converging pencil. This may perhaps be reckoned a circumstance of which we have no proof. But it must be considered,



that if the rays of light, after being variously bent towards the focus, were no longer to move with the same common velocity, the image formed at the focus of Dr. Bradley's telescope, would be elongated in the direction of the aberration. Those who have attended to this subject will be at no loss in discerning the reason of this. The extent of that lengthened image would depend on the difference of velocity which would obtain among the converging rays, and would probably increase according to the magnitude of the aperture of the object-glass. But such a phenomenon being contrary to experience, it follows, that the unequal bending of the rays does not give them unequal velocities, while moving in the same medium. This is another property with regard to the motion of light which may be considered as proved experimentally by Dr. Bradley's observations, and which doubtless would have occurred to him if he had had occasion to trace the refraction of a pencil of parallel rays at the object-glass of his telescope.

To conclude: in bringing this question concerning the velocity of light to the issue of an experiment, that fluid would doubtless be most proper for the telescope which absorbs the fewest rays, and possesses the greatest refractive density, and which at the same time is not liable to generate air-bubbles. To compensate for the unavoidable loss of light, which by Mr. Canton's and Dr. Priestley's experiments is found to be considerable in such cases, it perhaps may be necessary to use an achromatic object-glass for the sake of a large aperture, and of such a figure as to shorten the focal distance as much as the observations of such a small angle can admit of. Some contrivance too will be requisite to keep the whole space between the object-glass and the eye-glass always full, notwithstanding the expansions and contractions of the confined fluid by heat and cold, or its waste by evaporation.

It might prove a very considerable abridgment of the necessary apparatus, if this kind of telescope could be connected with the common telescope of a mural quadrant, or zenith sector, and their axes made perfectly parallel by previous observations of a proper terrestrial object. But as there would be some room for apprehending that the exact adjustment of the axes might be affected in raising the telescopes afterwards for celestial observations, this might be examined into by directing them to some star situated in, or very near, the ecliptic, and taking its meridian altitudes at a time of the year when it is in quadrature with the sun, in which case it would have no aberration. But either in this way, or with two separate instruments, the experiment might be made in a few nights, by taking the zenith distance of a proper star, the plane of the instruments being alternately turned different ways in observing, to get the true zenith distance independent of the error of the line of collimation; or the meridian altitude of the pole star may be observed in December above and below the pole, which will



give the apparent distance of the star from the pole at that time as affected by aberration. The error of the line of collimation would not affect the result in this way, being the same in the observation both above and below the pole.\*

\* Though Mr. Wilson did not bring forward this interesting tract till the year 1782, yet it is well known that, so long ago as 1770, his attention was drawn to this subject by a particular view which had occurred to him when reading over Dr. Bradley's admirable paper on aberration, in the *Phil. Trans.* It then struck Mr. Wilson that, in reasoning concerning the relative motion of the ray from the star, no account had been taken of its having finally to pass through the aqueous and vitreous humours of the eye on its way to the retina, in order to produce vision. This unavoidable and ultimate motion of the ray seemed to him to have some relation to the subject.

His idea was, that when looking into Dr. Bradley's sector, we perceived the image of the star in the centre of the field, at that instant a straight line, joining that centre at the centre of the retina, must coincide with the axis of the eye produced: because, by the laws of vision, an object must always appear in the direction of the optical axis and the eye which beholds it. But, as the image could not be seen in this direction unless the ray, when moving in the aqueous and vitreous humours, passed relatively along the axis of the eye, he was led to think that the velocity of the ray, in those dense fluids, was that which was justly deducible from the angle of aberration, shown by Dr. Bradley's sector; and not its velocity in air; as had been hitherto imagined.

Considering now that as the celebrated Romer, by a method wholly different from aberration, had nearly ascertained the velocity of light in the ethereal spaces, it now occurred to Mr. Wilson, that, by comparing the results afforded by these two different methods, it might be determined whether light was accelerated or not in the denser medium according to the ratio resulting from the Newtonian doctrine of refraction. This view, of resorting to the principles of aberration, for deciding experimentally in a question of such high moment to optics and to general physics, arose in Mr. Wilson's mind in the way now stated, and so long ago; as appears by the outlines of it published in the *London Chronicle* of the 4th December 1770, under the signature X.

Not long after this however, on further consideration, he found that some conclusions had been too hastily adopted; a circumstance the less surprising, as it will appear in the sequel that the very same were fallen into, without ever having been corrected, by philosophers and geometricians of the first eminence; though now known to be entirely false. On this account it may be both instructive and entertaining to trace a little the history of this intricate subject, and to shew the steps by which Mr. Wilson was led to a just comprehension of it. This we have been enabled to do by several late communications with him, during his present residence at Hampstead.

While he took it for granted, that the aberration of the axis of the eye must differ from that of Doctor Bradley's sector, yet he became soon sensible that the consequent displacement of the image of the star, as perceived by the eye looking in, would be much more minute than at first he imagined, by reason of the near proximity of the eye to the field of the sector. Still however this displacement seemed sufficient to give rise to phenomena of a very peculiar kind, some symptoms of which might be detected by a very close attention to the image. He supposed the case when the aberration of a star, near the pole of the ecliptic, lay at right angles to a horizontal wire passing through the centre of the field, when the telescope turned in a vertical circle. Then by making the wire gradually to approach the image, this he concluded ought to disappear when at some small distance from the wire; and, when brought to coincide with the wire, it ought to appear visible upon it, instead of being hid behind it. Such indeed would be the necessary consequences of the premises he now went on, by reason of the displacement of the image that would be occasioned by the eye.

But as Dr. Bradley has never mentioned any symptoms of phenomena so very peculiar, Mr.



*IX. Quantity of Rain which fell at Barrowby near Leeds. By George Lloyd, Esq., F. R. S. p. 71.*

This table of rain contains the quantity fallen in each month of 4 successive years, the sums of which for those years are, in 1778, 28 inches; in 1779,

Wilson was led to suspect some fallacy in his present grounds; and that, in reality, the aberration of the axes of the eye and of the sector might not at all differ, and that the ray passed relatively along both, when they lay in the same straight line. For, according to this, the image of the star would appear in its true place in the field, and of course the above phenomena could not exist.

He, on further consideration, found his suspicion was just, by the detection of a most material circumstance, soon to be explained, which from the beginning had entirely escaped him. By taking in this circumstance, and tracing its consequences, the whole discussion, at once, received a new form; and he was enabled fully to demonstrate, by the arguments stated in the present paper, that the aberration of the axis of the eye and that of the telescope must precisely agree, notwithstanding the acceleration of the ray on entering the eye, as resulting from Newton's doctrine of refraction.

Having arrived at this important conclusion, at first so little apprehended, it could not but occur that the same theory would hold true whatever magnitude or deepness was imputed to the eye. Still the aberration of its axis would precisely agree with that of Dr. Bradley's sector, when the ray from the star passed relatively over both. From this it followed immediately, as an identical proposition, that a telescope of any length filled with water, or any dense clear fluid, between the object glass and the wires at the focus, would shew the very same aberration with Dr. Bradley's sector, or any other telescope, having air only within it.

This being demonstrable, according to the Newtonian doctrine of refraction, Mr. Wilson saw that it was immediately applicable to the purpose he had originally in view: because such an agreement between a water and an air telescope, if actually found by observation, would constitute a proof of the acceleration of light in the dense medium, in the ratio assigned by Newton. The reader will perceive that this is the very thing which Mr. Wilson illustrates and proves by his present paper.

In endeavouring to trace the circumstances of the displacement of the image necessarily arising from his former premises, it comes to be considered how far the eye beheld the illumined wires in the field of Doctor Bradley's sector, in their true places, notwithstanding the motion of the earth in its orbit. This opened a question entirely new, namely, whether a terrestrial object, once seen in the axis of a water telescope, steadily and immutably pointed, could ever appear to depart from the axis by any new lateral motion given to both, by the orbital motion, or otherwise. In this inquiry, the same circumstances he had formerly detected clearly pointed out that such a terrestrial aberration was an impossible thing, according to the Newtonian doctrine of refraction; and that the object, once seen in the axis of the water telescope when immutably pointed, would still continue to be seen in the axis, notwithstanding the direction of the orbital motion, relative to the axis, constantly varying according to the time of the day.

The illustration and proofs of the various points now detailed, Mr. Wilson had fully made out before the end of 1772, as can be shown by original letters in his possession, especially from one gentleman,\* of the first eminence as an astronomer and mathematician, who with the greatest liberality and candour honoured, and warmly encouraged him in these researches by his correspondence. It was not till he had arrived at a full understanding of the subject he learned that the late excellent and eminent geometer Abbé Boscovich had proposed a similar experiment to him, and with the same view, but concerning which he had not then made any publication. Afterwards Lalande, in the 4th vol. of his *Astronomy*, published in 1781, pages 687, 688, gave an account of Boscovich's ideas from his own

\* Dr. Maskelyne.



29.05 inches ; in 1780, 22.9 inches ; in 1781, 25.6 inches ; and the mean of the 4 years is 26.4 nearly per year.

letter, said to be dated in 1766. This account is quite conformable to what Boscovich himself gives in his *Opuscula*, first published at Bassano in 1785. In these volumes there is another tract on terrestrial aberration, which topic appears to have come under his attention only a little time before publishing his *Opuscula*, and many years after the same subject had been considered by Mr. Wilson.

It is very remarkable however, by the account given by Lalande, and by Boscovich's own account in the two tracts of his *Opuscula*, that this excellent person proceeds entirely on a radical and confirmed mistake on both points, which overthrows all his conclusions ; though many of them, particularly those relating to terrestrial aberration, are very extraordinary, and justly and beautifully deduced from his erroneous principles ; and had they been true, would have led to most wonderful and important discoveries.

In regard to the first point, Boscovich asserts, and in this Lalande joins him, that the aberration of the axes of the water and air telescope, when the star is seen in the axes of each, must necessarily be different, and that the former would give only 15" aberration of a star in the pole of the ecliptic, instead of 20" the well known aberration of Dr. Bradley's sector, in that case. He then concludes that, were this difference actually found by observation, it would constitute the proof of the acceleration of light in the dense medium. In showing how the aberration of the water telescope should so differ, Boscovich considers the ray at entering, at the axis, as still proceeding down in the water in its former absolute direction, though much accelerated. But by Mr. Wilson's paper it is evident that this cannot possibly happen, on the hypotheses admitted. He has shown that the real or absolute path of the ray, before entering, is inclined to the moving axis, and consequently to the surface of the water. On that account its absolute direction, at the moment of entrance, must be changed by refraction, so as to make a less absolute angle than it did before with the moving axis and the water telescope.

This unavoidable change of the absolute direction of the ray, at entering, is the very important circumstance which Mr. Wilson at last detected, and which delivered him from all his previous misconceptions, and on which the whole reasoning on this subject entirely hinges. It appears however from the year 1776, till 1785, when Mr. Boscovich published his *Opuscula*, that this important circumstance had never once occurred to him ; in consequence of which it is now well known that all his conclusions, in the two tracts abovementioned, concerning the water telescope, and terrestrial aberration, are quite erroneous. Boscovich there considers only one effect produced on the ray at entering the water telescope, namely the acceleration of its velocity. But Mr. Wilson considers a second and simultaneous effect produced on the ray by refraction in consequence of its known oblique incidences, namely the unavoidable change of its former absolute direction. Accordingly the scope of his paper is to show how these two different but concomitant effects must, according to the Newtonian doctrine of refraction, precisely counteract one another, so as to make the aberration of the water and air telescopes to agree, when the star is seen in the axis of both. This conclusion is the very reverse of that of Boscovich. From never having attended to this second effect produced in the ray, as changing its former absolute direction, Boscovich necessarily concluded the possibility of terrestrial aberration ; and so deduced many curious things from it in his *opuscula*, all of which are illusions. But whoever reasons about this point, and takes into account this second effect on the ray, must immediately perceive that a terrestrial aberration, in the case of the water telescope, is an impossible thing.

In the 2d vol. of the *Philos. Trans.*, of the Edinburgh Royal Society, there is a most ingenious paper, on the Motion of Light, by a philosopher and geometer of great eminence, which points out Boscovich's delusion concerning this terrestrial aberration. This he does solely by taking in this



*X. Of an improved Thermometer. By Mr. James Six. p. 72.*

Attempting some time before to ascertain the greatest degree of heat and cold that happened in the atmosphere each day and night, or during the course of 24 hours, Mr. S. experienced the inconvenience which attends thermometers commonly used for that purpose; viz. the necessity of the observer's eye being on the instrument the very instant the mercury stands at the highest or lowest degree: for, since the time when that may happen is utterly uncertain, if it be not immediately noticed, it can never after be known. The sultry heat of the summer's days, and freezing cold of the winter's nights, which is commonly most severe at a late unseasonable hour, render it very unpleasant to be abroad in the open air, though it is absolutely necessary for the thermometer to be placed in such a situation. Ingenious men of our own country, as well as foreigners, have, it seems, long ago, endeavoured to remedy this inconvenience; and several thermometers of different constructions have been invented for that purpose. Van Swinden describes one, which he says was the first of the kind, made on a plan communicated by Mr. Bernoulli to Mr. Leibnitz. Mr. Kraft, he also tells us, made one nearly like it. A description of those by Lord Charles Cavendish and Mr. Fitzgerald may be seen in the Philos. Trans. vol. 50, p. 501, and vol. 51, p. 820. Though much ingenuity appears in the invention of those curious instruments, Mr. S. thought that a thermometer might be constructed more conveniently to answer the purpose, and show accurately the greatest degree of heat and cold which happened in the observer's absence. Mr. S. then gives a description of another thermometer, rather of a complex form; and then adds, thus far our thermometer resembles in some respects those of Mr. Bernoulli and Lord Charles Cavendish; but the method of showing how high the mercury had risen in the observer's absence, the essential property of an instrument of this kind, is wholly different from theirs, and effected in the following manner. Within the small tube of the thermometer, above the surface of the mercury on either side, immersed in the spirit of wine, is placed a small index, so fitted as to pass up and down as occasion may require: that surface of the mercury which rises carries up the index with it, which index does not return with the mercury when it descends; but, by remaining fixed, shows distinctly, and very accurately, how high the mercury had risen, and consequently what degree of heat or cold had happened. Towards evening, says Mr. S. I usually visit my thermometer, and see at one view, by the index on the left

circumstance of the unavoidable change of the absolute direction of the ray at entering the water telescope. The author there thinks that he was the first who ever detected that important circumstance; not then adverting that the same was fully pointed out, and reasoned from as the leading principle of Mr. Wilson's present paper, published in the London Philos. Trans., 5 years before the time the paper in question was read at Edinburgh in 1788.



side, the cold of the preceding night; and by that on the right, the heat of the day. These I minute down, and then apply a small magnet to that part of the tube against which the indexes rest, and move each of them down to the surface of the mercury: thus, without heating, cooling, separating, or at all disturbing the mercury, or moving the instrument, may this thermometer, without a touch, be immediately rectified for another observation. When I wish to put the thermometer out of my hand, without hanging it up, I have a stand to place it on; for if the mercury presses against the index, while the instrument lies in an horizontal position, it is in danger of passing by it, which is avoided by keeping the thermometer in a position nearly vertical.

*XI. On the Parallax of the Fixed Stars. By Mr. Herschel. F. R. S. p. 82.*

To find the distance of the fixed stars has been a problem which many eminent astronomers have attempted to solve; but about which, after all, we remain in a great measure still in the dark. Various methods have been pursued without success, and the result of the finest observations has hardly given us more than a distant approximation, from which we may conclude, that the nearest of the fixed stars cannot be less than 40 thousand diameters of the whole annual orbit of the earth distant from us. Trigonometry, by whose powerful assistance the mathematician has boldly ascended into the planetary regions, and measured the diameters and orbits of the heavenly bodies, for want of a proper base, can here be but of little service; for the whole diameter of the annual orbit of the earth is a mere point when compared to the immense distance of the stars. Now, as it is not in our power to enlarge this base, we can only endeavour to improve the instruments by which we measure its parallax.

There are two things requisite for measuring extremely small angles with accuracy. First, that the instrument we use for this purpose, be it quadrant, sector, or micrometer, should be divided and executed with sufficient exactness; and, secondly, that the telescope, by which the observations are to be made, should have an adequate power and distinctness. On the first head, the great improvements by instrument-makers have hardly left us any thing to desire: we can now measure seconds with almost as much facility and truth as former observers could measure minutes; nor does it seem impossible to go still further, and divide instruments that would show thirds with sufficient accuracy. It is in the latter, or optical part, we find the greatest difficulty. To see a single second of a degree with precision, requires a telescope of very great perfection; therefore, supposing the mechanical part of an apparatus well executed, it will still be necessary to try how far the power of our telescope will enable us to ascertain with confidence the division or number of seconds it points out. If on trial we find that our instrument will give us the same measure within the second, every



time the experiment is repeated, we may pronounce it capable of measuring seconds; if otherwise, it will remain to be examined, whether the fault lies in the mechanical or optical part.

Let us now suppose that the parallax of the fixed stars does not amount to a single second, yet still the case is by no means desperate; and though the difficulty of measuring seconds will soon suggest to us what extraordinary powers and distinctness of the telescope, and accuracy of the micrometer, are required to measure thirds; this ought by no means to discourage us in the attempt. Could we measure angles, much smaller than seconds, might we not hope to find the parallax of some of the fixed stars at least to amount to several thirds? On the other hand, if it should appear indeed that, even with such improved methods of measurement, we could not reach the remote situation of such almost infinitely distant suns, we might still derive a valuable approximation towards truth from such repeated observations, even though they should not be attended with all the success we expected from them. On this assurance, Mr. H. endeavoured to take such a method for attempting the investigation of the parallax of the stars as to avail himself of the improvements he had already made, and was still in hopes of making, in his telescopes.

The next thing necessary to be considered in this undertaking was, the manner of putting it into execution. The method pointed out by Galileo, and attempted by Hook, Flamsteed, Molineux, and Bradley, of taking distances of stars from the zenith that pass very near it, though it failed with regard to parallax, has been productive of the most noble discoveries of another nature. At the same time it has given us a much juster idea of the immense distance of the stars, and furnished us with an approximation to the knowledge of their parallax that is much nearer the truth than we ever had before. Dr. Bradley, in a letter to Dr. Halley on the subject of a new discovered motion of the fixed stars, says, "I believe I may venture to say, that in either of the two stars last mentioned ( $\gamma$  Draconis and  $\eta$  Ursæ majoris) it (the annual parallax) does not amount to 2". I am of opinion, that if it were 1" I should have perceived it in the great number of observations that I made, especially on  $\gamma$  Draconis; which agreeing with the hypothesis (without allowing any thing for parallax) nearly as well when the sun was in conjunction with, as in opposition to, this star, it seems very probable, that the parallax of it is not so great as one single second." Phil. Trans. n. 406. As it is not known that any thing more decisive has been done on the subject, it will not be amiss to see how far this method of finding the parallax has really been successful. The instrument that was used on this occasion, was the same as the present zenith sectors, which can hardly be allowed sufficient to show an angle of 1 or even 2 seconds with accuracy; yet, on account of the great number of observations, and above all the great sagacity of



the observer, we will admit that if the parallax had amounted to 2 seconds he would have perceived it. The star on which these observations were made, is marked of the 3d magnitude in the catalogue of Ptolemy; in Tycho Brahe's of the 3d; in the Prince of Hesse's of the 3d; in Hevelius's between the 3d and 2d; in Flamsteed's of the 2d; and now appears as a very bright star of the 3d, or a small star of the 2d magnitude; therefore its parallax is probably considerably less than that of a star of the first magnitude. Several authors, who have touched on this subject, seem to have overlooked this distinction; and from Dr. Bradley's account of the parallax of  $\gamma$  Draconis, have concluded the parallax of the stars in general not to exceed 1"; but this appears by no means to follow from the doctor's observations. It is rather evident that, for aught we know to the contrary, the stars of the first magnitude may still have a parallax of several seconds; and probably this is as accurate a result as that method is capable of giving, at least in latitudes where there is not a star of the first magnitude that passes directly through the zenith.\*

\* De La Lande, in his excellent book of Astronomy, says, that the parallax of the fixed stars has been proved to be absolutely insensible (liv. 16, § 2782.) He reports the observations of Tycho Brahe, Picard, Hook, and Flamsteed, and concludes (§ 2778) from the discovery of the aberration by Dr. Bradley (which it seems he also allows to be the most decisive on the subject) that now the question about parallax is resolved. In giving us the opinion which the doctor had of the result of his own observations with regard to the annual parallax, De La Lande only mentions "M. Bradley pense que si elle (la parallaxe) eût été seulement de 1", il l'auroit apperçue dans le grand nombre d'observations qu'il avoit faites, surtout de  $\gamma$  du Dragon." But if we also take in those lines on which Dr. Bradley seems to lay the greatest stress, viz. "I believe I may venture to say, that in either of the two stars last mentioned it does not amount to 2 seconds;" and if we allow for the magnitude of the stars on which the observations were made, I think I have fairly stated the full amount of all the actual proofs we have of the smallness of the annual parallax. Now, since it has escaped the finest observations of Bradley, it is not likely that it should come up to the full quantity to which it might amount without being perceived; and therefore the doctor might think it highly probable, "that it is not so great as one single second;" and his opinion, as well as De La Lande's, who believes it to be absolutely insensible, are perfectly consistent with all the observations that have hitherto been made; though the actual proofs, which are the subject of our present inquiry, do not extend so far. Against the parallax of Sirius, De La Lande (§ 2781) mentions "forty-five meridian altitudes taken by Dr. Bevis,\* with the eight-feet mural quadrant of the Royal Observatory at Greenwich, none of which differed 3 or 4" from the mean altitude." Now if they differed 3 or 4" from the mean, we may suppose they differed 6 or 8" from each other; and that observations, subject to so many causes of error as I shall presently enumerate, and which differed so much from each other, cannot give the least evidence either for or against a parallax, will need no proof. Refraction alone, which is liable to such changes at the meridian altitude of Sirius, notwithstanding the most careful observations of the barometer and thermometer should be made to ascertain its quantity, would, with me, remain an unanswerable argument against the validity of such observations in a subject of this critical nicety.—Orig.

\* These observations were not made by Dr. Bevis, but extracted from the registers of the Royal Observatory at my desire, and calculated by myself, and sent in a letter by Dr. Bevis to Paris.—Nevil Maskelyne.



In general, the method of zenith distances labours under the following considerable difficulties. In the first place, all these distances, though they should not exceed a few degrees, are liable to refractions; and I hope to be pardoned, says Mr. H. when I say that the real quantities of these refractions, and their differences, are very far from being perfectly known. Secondly, the change of position of the earth's axis arising from nutation, precession of the equinoxes, and other causes, is so far from being completely settled, that it would not be very easy to say what it exactly is at any given time. In the third place, the aberration of light, though best known of all, may also be liable to some small errors, since the observations from which it was deduced laboured under all the foregoing difficulties. I do not mean to say, that our theories of all these causes of error are defective; on the contrary, I grant that we are for most astronomical purposes sufficiently furnished with excellent tables to correct our observations from the above-mentioned errors. But when we are on so delicate a point as the parallax of the stars; when we are investigating angles that may perhaps not amount to a single second, we must endeavour to keep clear of every possibility of being involved in uncertainties; even the 100th part of a second becomes a quantity to be taken into consideration.

I shall now deliver the method I have taken, and show that it is free from every error to which the former is liable, and is still capable of every improvement the telescope and mechanism of micrometers can furnish. Let  $oe$  (fig. 1, pl. 4,) be two opposite points of the annual orbit, taken in the same plane with two stars  $a$ ,  $b$ , of unequal magnitudes. Let the angle  $aob$  be observed when the earth is at  $o$ : and let the angle  $aeb$  be also observed when the earth is at  $e$ . From the difference of these angles, if any should be found, we may calculate the parallax of the stars, according to a theory that will be delivered hereafter. These two stars, for reasons that will soon appear, ought to be as near each other as possible, and also to differ as much in magnitude as we can find them.

Galileo, I believe, was the first who suggested this method; but in the manner he mentions it in his 3d dialogue of the *Systema Cosmicum*, it would be exposed to all the difficulties we have enumerated, and would wish to avoid; for he does not observe that the two stars should be so near each other as thus to preclude the influence of every cause of error. This method has also been mentioned by other authors; and we find that Dr. Long observed the double star which is the first of Aries in Ptolemy's catalogue; that in the head of Castor; the middle one in the sword of Orion; and that in the breast of Virgo, with telescopes of 14 and 17 feet, and "was persuaded they would be found always to appear the same." But when the theory of parallax shall be explained, it will be seen that every one of these stars are totally improper for the purpose; for the stars of  $\gamma$  Arietis are near  $10''$  distant from each other, and are also equal



in magnitude. In  $\alpha$  Geminorum the stars, though near enough, do not sufficiently differ in magnitude to show any parallax. The stars in the Nebula of Orion, on account of their extreme smallness or distance, are still more improper than any; and those of  $\gamma$  Virginis are equal in magnitude.

I do not find that anything else has been done on the subject. Galileo justly remarks, that such observations ought to be made with the best telescopes, and on this occasion he mentions the power of his own, which enlarged the disc of the sun a thousand times, from which we find it magnified about 32 times; but we can hardly think his or even Dr. Long's, whose power might probably be 60 or 70, sufficient for that purpose. What would Galileo say, if he were told that our present opticians make instruments that enlarge the disc of the sun above 40,000 times? What would even Cassini say, if he were to view the first star of Aries, which appeared to him as split in two, through a telescope that will show  $\eta$  Coronæ borealis and  $h$  Draconis to be double stars?

But to proceed, I shall now prove that this method, if stars properly situated, such as I have found, are taken, is free from all the errors occasioned by refraction, nutation, precession of the equinoxes, changes of the obliquity of the ecliptic, and aberration of light; and that the annual parallax, if it even should not exceed the 10th part of a second, may still become visible, and be ascertained at least to a much greater degree of approximation than it ever has been done. It will also appear, from the great number of observations I have already made on several double stars, especially  $\epsilon$  Bootis, that we can now with much greater certainty affirm the annual parallax to be exceedingly small indeed; and that there is a great probability of succeeding still further in this laborious but delightful research, so as to be able at last to say, not only how much the annual parallax is not, but how much it really is.

Let there be 2 stars at a distance from each other, not exceeding 5 seconds; suppose them to be observed at an altitude of  $20^\circ$ ; and let them be so situated with respect to each other, that one of them may be  $20''$ , and the other  $20''$  and  $5''$  high: then the whole effect of mean refraction at that altitude, by Dr. Maskelyne's excellent tables, will be  $2' 35''.5$  for  $20^\circ$ , and  $2' 35''.4888$  for  $20^\circ 5''$ . The difference is  $0''.0111$ . Now, in the first place, we have nothing to do with the refraction itself, since the real altitude of the stars is not in question. In the next place, we also have no concern with the difference of refraction between the two stars, though no more than the .0111th part of a second, because the real distance between the two stars is not required. It follows then, that these observations can only be affected by the difference of the difference; that is, by an alteration in the quantity of refraction occasioned by the change of heat and cold, or weight of the atmosphere, and pointed out to us by the rise and fall of the barometer and thermometer. Let us then see what this difference of the



difference may amount to. Suppose a change of  $22^{\circ}$  of Fahrenheit's thermometer, that is, from the freezing point to the moderate air of a summer's night, and a difference of an inch in the height of the barometer; these two causes both conspiring, which does not often happen, may occasion an alteration of .00096th part of a second in 5, at an altitude of  $20^{\circ}$ ; but this, being less than the thousandth part of a second, may safely be rejected as a quantity altogether insensible.

Since it may not be always convenient to view those stars at the altitude of  $20^{\circ}$ , it remains to see what effect different altitudes may have: let us then make the most unfavourable supposition, that they may one time be seen in a horizontal position, having before been seen vertical. In this case, as the whole difference of refraction in a difference of  $5''$  of altitude is no more than .0111, provided they are observed not lower than  $20^{\circ}$ , and the whole difference of the difference of refraction is only .0009; the sum .012, when both conspire, not exceeding much the 100th part of a second, may still be rejected as insensible. Let us also examine how near the horizon it may be safe to observe such stars. At  $10^{\circ}$ , for instance, the refraction is  $5' 14''.6$ ; the difference for  $5''$  is .0388; the joint effect of the changes in the barometer and thermometer is .0034; the sum of the whole together amounts to .0422, which is less than half the 10th of a second: now this may either be taken into consideration, or such low observations may be avoided, as being by no means necessary, and but ill suiting the high powers a telescope proper for this purpose ought to bear.

The change of position of the earth's axis I consider as an unsurmountable obstacle to taking the parallax of stars by the method of zenith distances: for though refraction is much reduced in the zenith, this change is there no less sensible than in other parts of the heavens; but as this will always affect our two stars exactly alike, we are entirely freed from this embarrassment. The aberration of light can have no influence of the least consideration on our two stars, as a mere inspection of the tables will show. In a whole degree, its effects, when greatest, amount but to  $\frac{3}{10}$  of a second, and consequently in  $5''$  to no more than .0005, or the 2000th part of a second.

Observations of the relative distance of the two stars that make up a double star being thus cleared of every impediment, are capable of being continually improved by every degree of perfection the telescope may acquire: we can chuse stars that may be viewed sufficiently high to be clear of the vapours that swim near the horizon, and consequently employ the greatest powers our instruments are capable of. From experience I can also affirm, that the stars will bear a much higher degree of magnifying than other celestial objects. Too much has hitherto been taken for granted in optics: every natural philosopher is ready enough to allow the necessity of making experiments, and tracing out the steps



of nature; why this method should not be more pursued in the art of seeing does not appear. Theories are only to be used when proper data are assigned; but the data are carefully to be re-examined, when new improvements may widely alter the result of former experiments. Thus, we are told, that we gain nothing by magnifying too much. I grant it; but shall never believe I magnify too much till by experience I find, that I can see better with a lower power. Nor is even that sufficient: a lower power may show more of the object; it may show it brighter, nay even distincter, and therefore on the whole better; and yet the greater power may, in a particular case, be preferable: for if the object is so small as not to be at all visible with the lower power, and I can, by magnifying more, obtain a view of it, though neither so bright nor distinct as I could wish, is it not evident, that here this power is preferable to the former?

The naturalist does not think himself obliged to account for all the phenomena he may observe; the astronomer and optician may claim the same privilege. When we increase the power we lessen the light in the inverse ratio of the square of the power; and telescopes will, in general, discover more small stars, the more light they collect; yet with a power of 227 I cannot see the small star near the star following  $\alpha$  Aquilæ, when, by the same telescope, it appears very plainly with the power of 460: now, in the latter case, the power being more than double, the light is less than the fourth part of the former. In such particular cases I generally suspect my own eyes, and have recourse to those of my friends. I had the pleasure of showing this star to Dr. Watson, junior, who soon discovered the small star, which accompanies the other, with the power of 460; but saw nothing of it with 227, though the place where to look for it had been pointed out to him by the higher power. The experiment has been too often repeated to be doubtful, and has also been confirmed by others of nearly the same nature: for instance, the smallest of the 2 that accompany the star near  $\kappa$  Aquilæ, the small star near  $\mu$  Herculis, and the small star near  $\alpha$  Lyræ, are invisible with my power of 227, but visible with the same aperture when the power is 460. Also the small stars near Flamsteed's 24th of Aquila, the smaller of 2 near  $\sigma$  Coronæ, the small star near the star south of  $\epsilon$  Aquilæ, the small star near the second  $\alpha$  Persei, the small star near the star which accompanies Flamsteed's 10th sub pede et scapula dextra Tauri, the small star near  $\beta$  Delphini, and the small star near the pole star, are all much brighter and stronger, and therefore much sooner seen with 460, than with 227.

Great power may also, in particular circumstances, be favourable, even with an excess of aberration. When two stars are so close together as to make the scale for measuring the distance of their centres too small, if, by magnifying much, we can enlarge that distance, we may gain a considerable advantage, provided the centres, or apparent bodies of the stars, remain distinct enough for the



purpose of these measures. The appearance of  $\alpha$  Lyræ in my Newtonian reflector with a power of 460 is represented in fig. 2; with 2010 in fig. 3; with 3168 in fig. 4; and with 6450 in fig. 5. Now in all these figures we see, that the centres are still distinct enough to measure their distances with sufficient truth; or if any little error should be introduced by the magnitude of the central point, it will be more than sufficiently balanced by the largeness of the scale. In this manner, with a power of 3168, I have obtained a scale of no less than 10 inches  $\frac{6}{10}$  for the distance of the centres of the two stars of  $\alpha$  Geminorum; and as we know these centres to be but a few seconds distant, it is plain how great an advantage we gain by such an enlarged scale.

These experiments have but very lately pointed out to me a method of making a new micrometer, on a construction entirely different from any that are now in use, which I have been successful enough to put in practice, and by which I have already begun to determine the distance of the centres of some of the most remarkable double stars to a very great degree of accuracy.\*

The powers that may be used on various double stars are different, according to their relative magnitudes:  $\epsilon$  Bootis, for instance, will not bear the same power as  $\alpha$  Geminorum, nor would it be difficult to assign a reason for it; but as I here shall merely confine myself to facts, it will be sufficient in general to mention, that two stars, which are equal, or nearly so, will bear a very high power: with  $\alpha$  Geminorum I have gone as far as 3168; but with the former only to 2010. The difficulty of using high powers is exceedingly great; for the field of view takes in less than the diameter of the hair or wire in the finder, and the effect of the earth's diurnal motion is so great, that it requires a great deal of practice to find the object, and manage the instrument. It appears to me very probable, that the diurnal motion of the earth will be the greatest obstacle to our progress in magnifying, unless we can introduce a proper mechanism to carry our telescopes in a contrary motion.

Though opticians have proved that 2 eye-glasses will give a more correct image than one, I have always, from experience, persisted in refusing the assistance of a 2d glass, which is sure to introduce errors greater than those we would correct. Let us resign the double eye-glass to those who view objects merely for entertainment, and must have an exorbitant field of view. To a philosopher this is an unpardonable indulgence. I have tried both the single and double eye-glass of equal powers, and always found that the single eye-glass had much the superiority in point of light and distinctness. With the double eye-glass I could not see the belts on Saturn, which I very plainly saw with the single one. I would however except all those cases where a large field is absolutely necessary, and where power joined to distinctness is not the sole object of our view.

\* For a description of this micrometer see a subsequent paper.



The application of the different powers of a telescope in general is of some consequence; and in answer to those who may think I have strained or overcharged mine, I must observe, that a single glance at the subsequent  $\delta$  Draconis,  $\eta$  Coronæ, and the star near  $\mu$  Bootis, with a power of 460, showed them to me as double stars; when, in 2 former reviews of the heavens, I had twice set them down in my journal as single stars, where I used only the power of 222 and 227, and in all probability should never have found them double, had I not looked with a higher power.

We are to remember, that it is much easier to see an object when it is pointed out to us, than when it falls in our way unexpectedly, especially if of such a nature as to require some attention to be seen at all; but to say no more of other advantages of high powers, it is evident, that in the research of the parallax of the fixed stars they are absolutely necessary. If we would distinctly perceive and measure or estimate extremely small quantities, such as a 10th of a second, it appears, that when we use a power of 460, this 10th of a second will be no more in appearance than  $46''$ , and even with a power of 1500 will be but  $2' 30''$ , which is a quantity not much more than sufficient to judge well of objects and distinguish them from each other, such as a circle from a square, triangle, or polygon.\*

It has been observed, that objects become indistinct when the principal optic pencil at the eye becomes less than the 40th or 50th part of an inch in diameter. In the experiments that have been made on this subject it appears to me, that the indistinctness which is ascribed to the smallness of the optical pencil may be owing to very different causes: at least it will be easy to bring contrary experiments of extremely small pencils, not at all affected by this inconvenience; for instance, it is well known, that microscopes, consisting of a single lens or globule, are remarkable for distinctness. We also know, that they have been made so small as to magnify above 10,000 times.† From this we may infer that their apertures, and consequently the diameters of the optic pencil at the eye, could not exceed the 2500th part of an inch. I am therefore inclined to believe, that we must look for distinctness in the perfection of the object-speculum or object-glass of a telescope; and if we can make the first image in the focus of a speculum almost as perfect as the real object, what should hinder our magnifying but the want of light? Now, if the object has light sufficient, as the stars most undoubtedly have, I see no reason why we should limit the powers of our instruments by any theory. Is it not best to have recourse to experiments

\* By a set of experiments, made in the year 1774, I found, that I could discover or perceive a bright object, such as white paper, against the sky-light, when it subtended an angle of  $35''$ ; but could only distinguish it to be a circle, and no other figure, when it appeared under an angle of  $2' 24''$ .

† See Padre Della Torre's Method, &c. Scelta di Opusculi.



to find how far our endeavours, to render the first image perfect, have been successful.

As soon as I was fully satisfied that, in the investigation of parallax, the method of double stars would have many advantages above any other, it became necessary to look out for proper stars. This introduced a new series of observations. I resolved to examine every star in the heavens with the utmost attention and a very high power, that I might collect such materials for this research as would enable me to fix my observations on those that would best answer my end. The subject has already proved so extensive, and still promises so rich a harvest, to those who are inclined to be diligent in the pursuit, that I cannot help inviting every lover of astronomy to join with me in observations that must inevitably lead to new discoveries. I took some pains to find out what double stars had been recorded by astronomers; but my situation permitted me not to consult extensive libraries, nor indeed was it very material: for as I intended to view the heavens myself, Nature, that great volume, appeared to me to contain the best catalogue on this occasion. However, I remembered that the star in the head of Castor, that in the breast of the Virgin, and the first star in Aries, had been mentioned by Cassini as double stars. I also found the Nebula in Orion was marked in Huygens' *Systema Saturnium* as containing 7 stars, 3 of which (now known to be 4) are very near together. With this small stock I began, and in the course of a few years' observations have collected the stars contained in my catalogue. I find, with great pleasure, that a very excellent observer, whom I have the honour to call my friend,\* has also, though unknown to me, met with 3 of those stars that will be found in my catalogue: and on this occasion I also beg leave to observe, that the Astronomer Royal, when I was at Greenwich last May, with his usual politeness, showed me, among other objects,  $\alpha$  Herculis as a double star, which he had discovered some years ago. The Rev. Mr. Hornsby also, when I had the pleasure of seeing him at Oxford, in a conversation on the subject of the stars of the first magnitude that have a proper motion, mentioned  $\pi$  Bootis as a double star. It is a little hard on young astronomers to be obliged to discover over-again what has already been discovered; however, the pleasure that attended the view when I first saw these stars has made some amends for not knowing they had been seen before.

If I should mention, in my list of observations, a few that may be found difficult to be verified by other telescopes, I must beg the indulgence of the observers. I hope it will sufficiently appear, that I have guarded against optical delusions; and every astronomer, I make no doubt, will find, by those observa-

\* Phil. Trans., for the year 1781, double stars discovered in 1779, at Frampton-house, Glamorganshire, by Nat. Pigott, Esq., F. R. S., &c.



tions that fall within the compass of his instruments, and attention to circumstances necessary to the right management of them, that I have had all along truth and reality in view, as the sole object of my endeavours; and therefore he will be inclined to give some credit to what he does not immediately perceive, when he finds himself successful where he takes the proper precautions so necessary in delicate observations, even with the best instruments. I have been in some doubt in what manner to communicate these observations. My first view was to have methodized them properly; but I find them so extensive that there is but little probability that one person should be able to bring them to a conclusion, for which reason I have now resolved to give them unfinished as they are, that every person who is inclined to engage in this pursuit may become a fellow-labourer.

In settling the distances of double stars I have occasionally used 2 different ways. Those that are extremely near each other may be estimated by the eye, in measures of their own apparent diameters. For this purpose their distance should not much exceed 2 diameters of the larger, as the eye cannot so well make a good estimation when the interval between them is greater. This method has often the preference to that of the micrometer: for instance, when the diameter of a small star, perhaps not equal to half a second, is double the vacancy between the two stars. Here a micrometer ought to measure 10ths of seconds at least, otherwise we could not, with any degree of confidence, rely on its measures; nay, even then, if the stars are situated in the same parallel of declination and near the equator, their quick motion across the micrometer makes it extremely difficult to measure them, and in that case an estimation by the eye is preferable to any other measure; but this requires not a little practice, precaution, and time, and yet with proper care it will be found that this method is capable of great exactness. Let 2 small circles be drawn, either equal or unequal, at a distance not exceeding twice the diameter of the larger: let these be shown to several persons in the same light and point of view. Then, if every one of them will separately and carefully write down his estimation of the interval between them, in the proportion of either of their diameters, it will be found on a comparison, that there will seldom be so much as a quarter of a diameter difference among all the estimations. If this agreement takes place with so many different eyes, much more may we expect it in the estimations of the same eye, when accustomed to this kind of judgment.

I have divided the double stars into several different classes. In the first, I have placed all those which require indeed a very superior telescope, the utmost clearness of air, and every other favourable circumstance to be seen at all, or well enough to judge of them. They seemed to me on that account to deserve a separate place, that an observer might not condemn his instrument or his eye if he should not be successful in distinguishing them. As these are some of the



finest, most minute, and most delicate objects of vision I ever beheld, I shall be happy to hear that my observations have been verified by other persons, which I make no doubt the curious in astronomy will soon undertake. I should observe, that since it will require no common stretch of power and distinctness to see these double stars, it will therefore not be amiss to go gradually through a few preparatory steps of vision, such as the following: when  $\eta$  Coronæ borealis, one of the most minute double stars, is proposed to be viewed, let the telescope be some time before directed to  $\alpha$  Geminorum, or if not in view to either of the following stars,  $\zeta$  Aquarii,  $\mu$  Draconis,  $\epsilon$  Herculis,  $\alpha$  Piscium, or the curious double-double star  $\epsilon$  Lyræ. These should be kept in view for a considerable time, that the eye may acquire the habit of seeing such objects well and distinctly. The observer may next proceed to  $\xi$  Ursæ majoris, and the beautiful treble star in Monoceros's right fore-foot; after these to  $i$  Bootis, which is a fine miniature of  $\alpha$  Geminorum, to the star preceding  $\alpha$  Orionis, and to  $\eta$  Orionis. By this time both the eye and the telescope will be prepared for a still finer picture, which is  $\eta$  Coronæ borealis. It will be in vain to attempt this latter if all the former, at least  $i$  Bootis, cannot be distinctly perceived to be fairly separated, because it is almost as fine a miniature of  $i$  Bootis as that is of  $\alpha$  Geminorum. If the observer has been successful in all these, he may then, at the same time, try  $h$  Draconis, though I question whether any power less than 4 or 500 will show it to be double; but all the former I have seen very well with 227.

To try the stars of unequal magnitudes it will be expedient to take them in some such order as the following:  $\alpha$  Herculis,  $\omega$  Aurigæ,  $\delta$  Geminorum,  $h$  Cygni,  $\epsilon$  Persei, and  $b$  Draconis; from these the observer may proceed to a most beautiful object,  $\epsilon$  Bootis which I have closely attended these 2 years as very proper for the investigation of the parallax of the fixed stars.

It appears, from what has been said, that these double stars are a most excellent way of trying a telescope; and as the foregoing remarks have suggested the method of seeing how far the power and distinctness of our instruments will reach, I shall add the way of finding how much light we have. The observer may begin with the pole star and  $\alpha$  Lyræ; then go to the star south of  $\epsilon$  Aquilæ, the treble star near  $h$  Aquilæ, and last of all to the star following  $o$  Aquilæ. Now, if his telescope has not a great deal of good distinct light, he will not be able to see some of the small stars that accompany them.

In the 2d class of double stars, I have put all those that are proper for estimations by the eye or very delicate measures of the micrometer. To compare the distances with the apparent diameters, the power of the telescope should not be much less than 200, as they will otherwise be too close for the purpose. The instrument ought also to be as much as possible free from rays that surround a star in common telescopes; and should give the apparent diameters of a double



star, perfectly round and well defined, with a deep black division between them, as in fig. 6, which represents  $\alpha$  Geminorum as I have often seen it with a power of 460. It will be necessary here to take notice, that the estimations made with one telescope cannot be applied to those made with another: nor can the estimations made with different powers, though with the same telescope, be applied to each other. Whatever may be the cause of the apparent diameters of the stars, they are certainly not of equal magnitude with the same powers in different telescopes, nor of proportional magnitude with different powers in the same telescope. In my instruments I have ever found less diameter in proportion the higher I was able to go in power, and never have I found so small a proportional diameter as when I magnified 6450 times;\* therefore if we would wish to compare any such observations together, with a view to see whether a change in the distance has taken place, it should be done with the very same telescope and power, even with the very same eye-glass or glasses; for others, though of equal power and goodness, would most probably give different proportional diameters of the stars.

In the 3d class I have placed all those double stars that are more than 5 but less than 15'' asunder; and for that reason, if they should be used for observations on the parallax of the fixed stars, they ought to be considered as quite free from the effects of refraction, &c. In the same manner that the stars in the 1st and 2d classes will serve to try the goodness of the most capital instruments, these will afford objects for telescopes of inferior power, such as magnify from 40 to 100 times. The observer may take them in this or the like order:  $\zeta$  Ursæ majoris,  $\gamma$  Delphini,  $\gamma$  Arietis,  $\pi$  Bootis,  $\gamma$  Virginis,  $\iota$  Cassiopeæ,  $\mu$  Cygni. And if he can see all these, he may pass over into the 2d class, and direct his instrument to some of those that were pointed out as objects for the very best telescopes, where I suppose he will soon find the want of superior power.

The 4th, 5th, and 6th classes contain double stars that are from 15 to 30'', from 30'' to 1', and from 1' to 2' or more asunder. Though these will hardly be of any service for the purpose of parallax, I thought it not amiss to give an account of such as I have observed; they may perhaps answer another very important end, which also requires a great deal of accuracy, though not quite so much as the investigation of the parallax of the fixed stars. I will just mention it, though foreign to my present purpose. Several stars of the first magnitude have already been observed, and others suspected, to have a proper motion of their own; hence we may surmise, that our sun, with all its planets and comets, may also have a motion towards some particular part of the heavens, on account of a greater quantity of matter collected in a number of stars and

\* See the measures of the diameter of  $\alpha$  Lyræ. Catalogue of double stars, 5th class.—Orig.



their surrounding planets there situated, which may perhaps occasion a gravitation of our whole solar system towards it. If this surmise should have any foundation, it will show itself in a series of some years; as from that motion will arise another kind of hitherto unknown parallax,\* the investigation of which may account for some part of the motions already observed in some of the principal stars; and for the purpose of determining the direction and quantity of such a motion, accurate observations of the distance of stars that are near enough to be measured with a micrometer, and a very high power of telescopes, may be of considerable use, as they will undoubtedly give us the relative places of those stars to a much greater degree of accuracy than they can be had by transit instruments or sectors, and thus much sooner enable us to discover any apparent change in their situation occasioned by this new kind of systematical parallax, if I may be allowed to use that expression, for signifying the change arising from the motion of the whole solar system.

I shall now endeavour to deliver a theory of the annual parallax of double stars, with the method of computing from it what is generally called the parallax of the fixed stars, or of single stars of the first magnitude, such as are nearest to us. It may be observed, that the principles on which I have founded the following theory are of such a nature, that they cannot be strictly demonstrated, in consequence of which they are only proposed as postulata, which have so great a probability in their favour, that they will hardly be objected to by those who are in the least acquainted with the doctrine of chances.

*General Postulata.*—1. Let the stars be supposed, one with another, to be about the size of the sun.†

2. Let the difference of their apparent magnitudes be owing to their different distances, so that a star of the 2d, 3d, or 4th magnitude, is 2, 3, or 4 times as far off as one of the first.‡

\* See the note in the Rev. Mr. Mitchell's paper on the Parallax of the Fixed Stars. Phil. Trans. vol. 57, p. 252.—Orig.

† See Mr. Mitchell's Inquiry into the probable Parallax and Magnitude of the Fixed Stars, Phil. Trans. vol. 57; and Dr. Halley on the Number, Order, and Light, of the Fixed Stars, Phil. Trans. vol. 31.—Orig.

‡ The apparent magnitude is here taken in a stricter sense than is generally used; and by it is rather meant the order into which the stars ought to be distinguished than that into which they are commonly divided; for as the order of the magnitudes is here to denote the different relative distances, we are to examine carefully the degree of light each star is accurately found to have: and considering then that light diminishes in the inverse ratio of the squares of the distances, we ought to class the stars accordingly. An allowance ought also perhaps to be made for some loss that may happen to the light of very remote stars in its passage through immense tracts of space, most probably not quite destitute of some very subtle medium. This conjecture is suggested to us by the colour of the very small telescopic stars, for I have generally found them red, or inclining to red; which seems to indicate, that the more feeble and refrangible rays of the other colours are either stopped by the way, or at least diverted from their course by accidental deflections.—Orig.



In fig. 7, let  $OE$  be the whole diameter of the earth's annual orbit; and let  $a, b, c$ , be 3 stars situated in the ecliptic, in such a manner that they may be seen all in one line  $oabc$ , when the earth is at  $o$ . Let the line  $oabc$  be perpendicular to  $OE$ , and draw  $PE$  parallel to  $co$ . Then, if  $oa, ab, bc$ , are equal to each other,  $a$  will be a star of the 1st magnitude,  $b$  of the 2d, and  $c$  of the 3d. Let us now suppose the angle  $oae$ , or parallax of the whole orbit of the earth, to be  $1''$  of a degree: then we have  $PEa = oae = 1''$ : and, because very small angles, having the same subtense  $OE$ , may be taken to be in the inverse ratio of the lines  $oa, ob, oc$ , &c. we shall have  $obE = \frac{1}{2}''$ ,  $ocE = \frac{1}{3}''$ , &c.\* Now, when the earth is removed to  $E$ , we shall have  $PEb = Ebo = \frac{1}{2}''$ , and  $PEa - PEb = aEb = \frac{1}{2}''$ ; that is, the stars  $a, b$ , will appear to be  $\frac{1}{2}''$  distant. We also have  $PEc = ECo = \frac{1}{3}''$ , and  $PEa - PEc = aEc = \frac{2}{3}''$ ; that is, the stars  $a, c$ , will appear to be  $\frac{2}{3}''$  distant, when the earth is at  $E$ . Now, since we have  $bEP = \frac{1}{2}''$ , and  $cEP = \frac{1}{3}''$ , therefore  $bEP - cEP = bEc = \frac{1}{2}'' - \frac{1}{3}'' = \frac{1}{6}''$ ; that is, the stars  $b, c$ , will appear to be only  $\frac{1}{6}''$  removed from each other, when the earth is at  $E$ .

From what has been said, we may gather the following general expression, to denote the parallax that will become visible in the change of distance between the two stars, by the removal of the earth from one extreme of its orbit to the other. Let  $P$  express the total parallax of a fixed star of the first magnitude,  $M$  the magnitude of the larger of the two stars,  $m$  the magnitude of the smaller,† and  $p$  the partial parallax to be observed by the change in the distance of a double star; then will  $p = \frac{m - M}{Mm} P$ ; and  $p$ , being found by observation, will give us  $P = \frac{pMm}{m - M}$ . An example or two will explain this sufficiently. Suppose a star of the first magnitude should have a small star of the 12th magnitude near it; then will the partial parallax we are to expect to see be  $\frac{12 - 1}{12 \times 1} P$ ; or  $\frac{11}{12}$  of the total parallax of a fixed star of the first magnitude; and if we should, by observation, find the partial parallax between 2 such stars to amount to  $1''$ , we shall have the total

\* This proves what I have before remarked on the parallax of  $\gamma$  Draconis; for that star, (admitting it to be a star of between the 2d and 3d magnitude, which ought to be ascertained by experiments, as mentioned in the note above) by the postulata, will have its place assigned somewhere between  $b$  and  $c$ , and therefore its parallax will be between  $\frac{1}{2}$  and  $\frac{1}{3}$  of the parallax of a star of the first magnitude. And if Dr. Bradley thought that he should have perceived a parallax in  $\gamma$  Draconis, if at most it had amounted to  $2''$ , it follows, that the angle  $oae$  may nearly amount to 4 or  $5''$  for any thing we can conclude to the contrary from those observations.—Orig.

† As  $M$  and  $m$  are here taken to express the relative distances of the stars, in measures whereof the distance of the nearer star is taken as unity, those who think the postulata on which these estimations are built cannot be granted, may still use the following formulæ, if instead of the magnitudes  $M, m$ , they put their own estimations of the relative distances of the stars, according to any other method whatever they may think it most eligible to adopt; for the apparent magnitude of stars is here only proposed as the most probable means we have of forming any conjectures about their relative distances.—Orig.



parallax  $P = \frac{1 \times 1 \times 12}{12 - 1} = 1''.0909$ . If the stars are of the 3d and 24th magnitude, the partial parallax will be  $\frac{24 - 3}{3 \times 24} = \frac{21}{72} P = \frac{7}{24} P$ ; and if by observation,  $p$  is found to be a 10th of a second, the whole parallax will come out  $\frac{.1 \times 3 \times 24}{24 - 3} = 0''.3428$ .

It will be necessary to examine some different situations. Suppose the stars, being still in the ecliptic, to appear in one line, when the earth is in any other part of its orbit between  $o$  and  $E$ ; then will the parallax still be expressed by the same algebraic form, and one of the maxima will still lie at  $o$ , the other at  $E$ ; but the whole effect will be divided into 2 parts, which will be in proportion to each other as radius — sine to radius + sine of the stars distance from the nearest conjunction or opposition.

When the stars are any where out of the ecliptic, situated so as to appear in one line  $oabc$  at rectangles to  $oE$ , the maximum of parallax will still be expressed by  $\frac{m - M}{Mm} P$ ; but there will arise another additional parallax in the conjunction and opposition, which will be to that which is found  $90^\circ$  before or after the sun, as the sine ( $s$ ) of the latitude of the stars seen at  $o$  is to radius ( $R$ ); and the effect of this parallax will be divided into 2 parts; half of it lying on one side of the large star, the other half on the other side of it. This latter parallax also will be compounded with the former, so that the distance of the stars in the conjunction and opposition will then be represented by the diagonal of a parallelogram, of which the two semi-parallaxes are the sides; a general expression for which will be  $\frac{m - M}{2Mm} P \times \sqrt{(\frac{ss}{RR} + 1)}$ : for the stars will apparently describe 2 ellipses in the heavens, whose transverse axes will be to each other in the ratio of  $M$  to  $m$  (fig. 8), and  $Aa$ ,  $Bb$ ,  $Cc$ ,  $Dd$ , will be cotemporary situations. Now, if  $bQ$  be drawn parallel to  $Ac$ , and the parallelogram  $bqBQ$  completed, we shall have  $bQ = \frac{1}{2}CA - \frac{1}{2}ca = \frac{1}{2}Cc = \frac{1}{2}p$ , or semi-parallax  $90^\circ$  before or after the sun, and  $Bb$  may be resolved into, or is compounded of,  $bQ$  and  $bq$ ; but  $bq = \frac{1}{2}BD - \frac{1}{2}bd =$  the semi-parallax in the conjunction or opposition. We also have  $R : s :: bQ : bq = \frac{ps}{2R}$ ; therefore the distance  $Bb$  or  $Dd = \sqrt{[(\frac{p}{2})^2 + (\frac{ps}{2R})^2]}$ ; and by substituting the value of  $p$  into this expression we obtain  $\frac{m - M}{2Mm} P \times \sqrt{(\frac{ss}{RR} + 1)}$ , as above. When the stars are in the pole of the ecliptic,  $bq$  will become equal to  $bQ$ , and  $Bb$  will be  $.7071P \frac{m - M}{Mm}$ .

Hitherto we have supposed the stars to be all in one line  $oabc$ ; let them now be at some distance, suppose  $5''$  from each other, and let them first be both in the ecliptic. This case is resolvable into the first; for imagine the star  $a$ , fig. 9, to stand at  $x$ , and in that situation the stars  $x$ ,  $b$ ,  $c$ , will be in one line, and their



parallax expressed by  $\frac{m - M}{Mm} P$ . But the angle  $aex$  may be taken to be equal to  $aox$ ; and as the foregoing form gives us the angles  $xeb$ ,  $xec$ , we are to add  $aex$ , or  $5''$  to  $xeb$ , and we shall have  $aEb$ . In general, let the distance of the stars be  $d$ , and let the observed distance at  $E$  be  $D$ ; then will  $D = d + p$ , and therefore the whole parallax of the annual orbit will be expressed by  $\frac{DMm - dMm}{m - M} = P$ .

Suppose the two stars now to differ only in latitude, one being in the ecliptic, the other, for instance,  $5''$  north, when seen at  $o$ . This case may also be resolved by the former; for imagine the stars  $b$ ,  $c$ , fig. 7, to be elevated at rectangles above the plane of the figure, so that  $aob$ , or  $aoc$ , may make an angle of  $5''$  at  $o$ : then, instead of the lines  $oabc$ ,  $ea$ ,  $eb$ ,  $ec$ ,  $ep$ , imagine them all to be planes at rectangles to the figure; and it will appear, that the parallax of the stars in longitude must be the same as if the small star had been without latitude. And since the stars  $b$ ,  $c$ , by the motion of the earth from  $o$  to  $E$ , will not change their latitude, we shall have the following construction for finding the distance of the stars  $ab$ ,  $ac$ , at  $E$ , and from thence the parallax  $P$ . Let the triangle  $ab\beta$ , fig. 10, represent the situation of the stars;  $ab$  is the subtense of  $5''$ , that being the angle under which they are supposed to be seen at  $o$ . The quantity  $b\beta$  by the former theorem is found  $\frac{m - M}{Mm} P$ , which is the partial parallax that would have been seen by the earth's moving from  $o$  to  $E$ , had both stars been in the ecliptic; but on account of the difference in latitude it will now be represented by  $a\beta$ , the hypotenuse of the triangle  $ab\beta$ : therefore, in general, putting  $ab = d$ , and  $a\beta = D$ , we have  $\frac{\sqrt{(DD - dd) \times Mm}}{m - M} = P$ . Hence  $D$  being taken by observation, and  $d$ ,  $M$ , and  $m$ , given, we obtain the total parallax.

If the situation of the stars differs in longitude as well as latitude, we may resolve this case by the following method. Let the triangle  $ab\beta$ , fig. 11, represent the situation of the stars,  $ab = d$  being their distance seen at  $o$ ,  $a\beta = D$  their distance seen at  $E$ . That the change  $b\beta$  which is produced by the earth's motion will be truly expressed by  $\frac{m - M}{Mm} P$ , may be proved as before, by supposing the star  $a$  to have been placed at  $\alpha$ . Now let the angle of position  $ba\alpha$  be taken by a micrometer,\* or by any other method that may be thought sufficiently exact; then, by solving the triangle  $ab\alpha$ , we shall have the longitudinal and latitudinal differences  $a\alpha$  and  $b\alpha$  of the two stars. Put  $a\alpha = x$ ,  $b\alpha = y$ , and it will be  $x + b\beta = aq$ , whence  $D = \sqrt{\left(x + \frac{m - M}{mM} P\right)^2 + yy}$ ; and  $\frac{\sqrt{(D^2 - y^2)} - x}{m - M} = P$ .

\* The position of a line passing through the two stars, with the parallel of declination of the largest of them, may be had by the micrometer I invented for this purpose in the year 1779, of which a description has been given in a former paper; whence, by spherical trigonometry, we easily deduce their position  $ba\alpha$  fig. 11, with regard to the ecliptic,—Orig.



If neither of the stars should be in the ecliptic, nor have the same longitude or latitude, the last theorem will still serve to calculate the total parallax whose maximum will lie in  $\epsilon$ . There will also arise another parallax, whose maximum will be in the conjunction and opposition, which will be divided, and lie on different sides of the large star; but as we know the whole parallax to be exceedingly small, it will not be necessary to investigate every particular case of this kind; for, by reason of the division of the parallax, which renders observations taken at any other time, except where it is greatest, very unfavourable, the forms would be of little use.

To finish this theory, I shall only add a general observation on the time and place where the maxima of parallax will happen. When 2 unequal stars are both in the ecliptic, or, not being in the ecliptic, have equal latitudes, north or south, and the larger star has most longitude, the maximum of the apparent distance will be when the sun's longitude is  $90^\circ$  more than the stars, or when observed in the morning; and the minimum when the longitude of the sun is  $90^\circ$  less than that of the star, or when observed in the evening. When the small star has most longitude, the maximum and minimum, as well as the time of observation, will be the reverse of the former. When the stars differ in latitudes, this makes no alteration in the place of the maximum or minimum, nor in the time of observation; that is to say, it is immaterial whether the largest star has the least or the most latitude of the two stars.

*XII. Catalogue of Double Stars. By Mr. Herschel, F. R. S. p. 112.*

*Introductory Remarks.*—The following catalogue contains not only double stars, but also those that are treble, double-double, quadruple, double-treble, and multiple. The particulars I have given of them are comprehended under the following general heads. 1. The names of the stars and number in Flamsteed's catalogue; or, if not contained there, such a description of their situation as will be found sufficient to point them out. 2. The comparative size of the stars. On this occasion I have used the terms equal, a little unequal, pretty unequal, considerably unequal, very unequal, extremely unequal, and excessively unequal, as expressing the different gradations to which I have endeavoured to affix always the same meaning. 3. The colours of the stars as they appeared to me when I viewed them. Here I must remark, that different eyes may perhaps differ a little in their estimations. I have, for instance, found, that the little star which is near  $\alpha$  Herculis, by some to whom I have showed it has been called green, and by others blue. Nor will this appear extraordinary when we recollect that there are blues and greens which are very often, particularly by candle-light, mistaken for each other. The situation will also affect the colour a little, making a white star appear pale red when the altitude is not sufficient to clear it of the



vapours. It is difficult to find a criterion of the colours of stars, though I might in general observe that Aldebaran appears red, Lyra white, and so on; but when I call the stars garnet, red, pale red, pale rose-colour, white inclining to red, white, white inclining to blue, bluish white, blue, greenish, green, dusky, I wish rather to refer to the double stars themselves to explain what is meant by those terms.

4. The distances of the stars are given several different ways. Those that are estimated by the diameter can hardly be liable to an error of so much as one quarter of a second; but here must be remembered what I have before remarked on the comparative appearance of the diameters of stars in different instruments. Those that are measured by the micrometer, I fear, may be liable to an error of almost a whole second: and if not measured with the utmost care, to near 2". This is however to be understood only of single measures; for the distance of many of them that have been measured very often in the course of 2 years observations, can hardly differ so much as half a second from truth, when a proper mean of all the measures is taken. As I always make the wires of my micrometer outward tangents to the apparent diameter of the stars, all the measures must be understood to include both their diameters; so that we are to deduct the 2 semi-diameters of the stars if we would have the distance of their centres. What I have said concerns only the wire micrometers, for my last new micrometer is of such a construction, that it immediately gives the distance of the centres; and its measures, as far as in a few months I have been able to find out, may be relied on to about  $\frac{1}{10}$  of a second, when a mean of 3 observations is taken. When I have added inaccurate, we may suspect an error of 3 or 4". Exactly estimated may be taken to be true to about  $\frac{1}{8}$  part of the whole distance; but only estimated, or about, &c. is in some respect quite undetermined; for it is hardly to be conceived how little we are able to judge of distances when, by constantly changing the powers of the instrument, we are as it were left without any guide at all. I should not forget to add, that the measure of stars, when one is extremely small, must claim a greater indulgence than the rest, on account of the difficulty of seeing the wires when the field of view cannot be sufficiently enlightened.

5. The angle of position of the stars I have only given with regard to the parallel of declination, to be reduced to that with the ecliptic as occasion may require. The measures always suppose the large star to be the standard, and the situation of the small one is described accordingly. Thus, in fig. 12, AB represents the apparent diurnal motion of a star in the direction of the parallel of declination AB; and the small star is said to be south preceding at *mn*, north preceding at *op*, south following at *qr*, and north following at *st*. The measure of these angles, I believe, may be relied on to 2°, or at most 3°, except when men-



tioned inaccurate, where an error amounting to  $5^\circ$  may possibly take place. In mere estimations of the angle, without any wires at all, an error may amount to at least  $10^\circ$ , when the stars are near each other.

6. The dates, when I first perceived the stars to be double, treble, &c. are marked in the margin of each star. To shorten the work as much as possible, I have put L for the large star; s for the small star; w for white; r for red; d for dusky; n for north; s for south; and have also occasionally used other abbreviations that will be easily understood. It may be seen, that this catalogue is yet in a very imperfect state, many of the stars not having even the principal elements of distance and position determined with any degree of accuracy; but having already mentioned the reason why I give it imperfect as it is, I can only add that my endeavours will not be wanting soon to remove those defects. However, since this can only be a work of some time, we may hope, in the meanwhile, that many lovers of the science will turn their thoughts to the same subject.

### CATALOGUE OF DOUBLE STARS.

#### *First Class.*

1.  $\epsilon$  Bootis. Flamst. 36. Ad dextrum femur in perizomate.

Sept. 9, 1779.—Double. Very unequal. L red-dish; S blue, or rather a faint lilac. A very beautiful object. The vacancy, or black division between them, with 227 is  $\frac{3}{4}$  diameter of S; with 460,  $1\frac{1}{4}$  diameter of L; with 932, near 2 diameters of L; with 1159, still further; with 2010, extremely distinct,  $2\frac{3}{4}$  diameters of L. These quantities are a mean of 2 years observations. Position  $31^\circ 34'$  n preceding.

2.  $\xi$  Ursæ majoris. Fl. 53. In dextro posteriore pede.

May 2, 1780.—Double. A little unequal. Both w and very bright. The interval with 222 is  $\frac{2}{3}$  diameter of L; with 227, 1 diameter of L; with 278, near  $1\frac{1}{2}$  diameter of L. Position  $53^\circ 47'$  s following.

3.  $\sigma$  Coronæ borealis, Fl. 17.

Aug. 7.—Treble. The 2 nearest pretty unequal; the 3d very faint with powers lower than 460. The 2 nearest both w; the 3d d. Interval of the 2 nearest with 227, full  $1\frac{1}{4}$  diameter of L; with 460, 2 diameters of L. Position  $77^\circ 32'$  n preceding. Distance of the 3d from L  $24''$  by exact estimation. Position  $25^\circ$  n following by estimation.

4. In constellatione Draconis, Fl. 16.

Aug. 8.—Double. It is the star to which a line drawn from  $\nu$  through  $\mu$ , points at nearly the same distance from  $\mu$  as  $\mu$  from  $\nu$ . Considerably unequal. L w; S w inclining to r. With 222, 1 diameter of L; with 278,  $1\frac{1}{2}$  diameter of L. Position  $24^\circ 0'$  s following. There is a 3d star, at some distance, preceding.

5.  $\sigma$  Cassiopeæ, Fl. 8. In dextro cubito.

Aug. 31.—Double. It is the star at the vertex of a telescopic isosceles triangle turned to the south. Very unequal. L w a little inclining to r; S d. With 222,

near 1 diameter of L; with 460,  $1\frac{1}{2}$  diameter of L. Position  $60^\circ 28'$  n preceding.

6. Quæ infra oculum Lyncis, Fl. 12.

Oct. 3.—A curious treble star. Two nearest pretty unequal. L w; S w inclining to rose colour. With 227, about  $\frac{1}{2}$  diameter; with 460, full  $\frac{3}{4}$  diameter of s. Position  $88^\circ 37'$  s preceding. The 1st and 3d considerably unequal; 2d and 3d pretty unequal. The 3d pale r. Distance from the 1st  $9'' 23'''$ ; too difficult to be extremely exact. Position with regard to the 1st  $32^\circ 33'$  n preceding.

7.  $b$  Draconis, Fl. 39. Trium in recta, in prima inflexione colli, borea.

Oct. 3.—A minute double star. Extremely unequal, the small star being a fine lucid point. L w; S inclining to r. With 227,  $\frac{3}{4}$  diameter of L; with 460, full  $1\frac{1}{2}$  diameter of L; with 932 (extremely fine) full 2 diameters of L. Position  $77^\circ 8'$  n following. A 3d star at some distance; dusky r. Position  $63^\circ 22'$  n following.

8.  $\epsilon$  Draconis, Fl. 63. In quadrilatero inflexionis primæ.

Oct. 3.—A very minute double star. Excessively unequal; the small star can only be seen when the air is perfectly clear. L w; S d. With 227, less than 1 diameter of L; with 278, not a diameter of L. Position  $63^\circ 14'$  n preceding. A pretty large 3d star at about 3 or  $4'$ . Position of this 3d star with  $\epsilon$   $88^\circ 16'$  n following.

9. In cauda Lyncis media, Fl. 39.

Nov. 24.—Double. Very unequal. L w; S inclining to r. With 227, extremely close; with 460, at least  $\frac{1}{2}$  diameter of S. A very fine object. Position  $25^\circ 51'$  s preceding. A proper motion is suspected in one of the stars.

10. In sinistro anteriore pede Monocerotis, Fl. 11.

Feb. 15, 1781.—A curious treble star; may appear double at first sight; but with some attention we see



that one of them again is double. The first, or single star, is the largest; the other two are both smaller, and almost equal, but the preceding of them is rather larger than the following. They are all w. The two nearest with 227, 1 diameter of the preceding, or nearly  $1\frac{1}{4}$  of the following; with 460,  $1\frac{1}{4}$  diameter of the preceding. Position of the two nearest  $11^{\circ} 32'$  s following. For an account of the single star, see the 2d class. As perfect as I have seen this treble star with 460, it is one of the most beautiful sights in the heavens; but requires a very fine evening.

11. In constellatione Cancrī, Fl. 11.

Mar. 13.—Double. Considerably unequal. Both pale r. With 227, 1 full diameter of L; with 460, about  $1\frac{3}{4}$  diameter of L. Position  $85^{\circ} 10'$  n preceding.

12. *d* Serpentis, Fl. 59. In Cauda.

July 17.—Double. Very unequal. L reddish w; S fine blue. With 227, 1 full diameter of L; with 278,  $1\frac{1}{3}$  diameter of L. Position  $44^{\circ} 33'$  n preceding.

13. In constellatione Aquilæ, near Fl. 37.

July 25.—A curious treble star. It is the last star of a telescopic trifolium n following *k*, similar to that in the hand of Aquarius. The two nearest very unequal; the 3d star excessively small, and not visible with 227. The two nearest with 460, no more than  $\frac{1}{2}$  diameter of L; the farthest about 7 or 8".

14. In constellatione Aquilæ, Fl. 24.

July 30.—Double. In Harris's maps it is the star in the elbow of Antinous. Excessively unequal; the small star is but just visible with 227; but with 460 it is pretty strong. L pale r; S d. With 227, 1 full diameter of L; with 460,  $1\frac{1}{2}$  diameter of L. Position  $72^{\circ} 0'$  s following.

15. *i* Bootis, Fl. 44.

Aug. 17.—Double. In Harris's maps it is marked *i*, but has no letter in Fl. Atlas. Considerably unequal. Both w. With 227 they seem almost to touch, or at most  $\frac{1}{4}$  diameter of S asunder; with 460,  $\frac{1}{2}$  or  $\frac{3}{4}$  diameter of S. This is a fine object to try a telescope, and a miniature of  $\alpha$  Geminorum. Position  $29^{\circ} 54'$  n following.

16.  $\gamma$  Coronæ borealis, Fl. 2.

Sept. 9.—Double. A little unequal. They are whitish stars. They seem in contact with 227, and though I can see them with this power, I should certainly not have discovered them with it; with 460, less than  $\frac{1}{4}$  diameter; with 932, fairly separated, and the interval a little larger than with 460. I saw them also with 2010, but they are so close that this power is too much for them, at least when the altitude of the stars is not very considerable; with 460 they are as fine a miniature of *i* Bootis as that is of  $\alpha$  Geminorum. Position  $59^{\circ} 19'$  n following.

17. In constellatione Bootis, near Fl. 51.

Sept. 10.—Double. It is a star near  $\mu$  not marked in Flamsteed's catalogue. Considerably unequal. Both dusky w inclined to r. The interval with 460 is  $\frac{3}{4}$  diameter of S. The position of the small star is turned towards  $\mu$  a little following the line which joins L to  $\mu$  Bootis. See  $\mu$  Bootis in the 6th class.

18. In constellatione Coronæ borealis.

Sept. 10.—Double. It is the smallest of 2 tele-

scopic stars between  $\theta$  and  $\delta$ , not contained in Fl. cat. Equal. Both d. With 460, about  $1\frac{3}{4}$  diameters. Position  $21^{\circ} 0'$  n following.

19. *h* Draconis, near Fl. 20.

Sept. 10.—One of the most minute of all the double stars I have hitherto found. It is the small telescopic star near the preceding *h* Draconis. Considerably unequal. Both dusky w inclining to r. With 460, they seem in contact; I have however had a very good view of a small dark division between them. Position (by exact estimation)  $25$  or  $30^{\circ}$  s preceding. They are too minute for any micrometer I have. It is in vain to look for them if every circumstance is not favourable. The observer as well as the instrument must have been long enough out in the open air to acquire the same temperature. In very cold weather, an hour at least will be required; but in a moderate temperature, half an hour will be sufficient.

20. In dextro humero Orionis, Fl. 52.

Oct. 1.—Double. A little unequal. Both w a little inclining to pale r. With 227,  $\frac{1}{4}$  diameter; with 460,  $\frac{1}{2}$  diameter. Position  $69^{\circ} 41'$  s preceding.

21. *c* Trianguli, near Fl. 12 and 13.

Oct. 8.—Double. It is the most north of a small telescopic trapezium of unequal stars. Extremely unequal. With 460,  $\frac{3}{4}$  diameter of L. Position (by estimation)  $55$  or  $60^{\circ}$  n preceding.

22.  $\eta$  Orionis, Fl. 33. Duorum præcedentium  $13^{\text{am}}$  ( $\omega$ ) antedens.

Oct. 22.—Double. Considerably unequal. L w; S w; inclining to blue. With 227, they seem almost in contact; with 460,  $\frac{1}{2}$  diameter of S. Position  $61^{\circ} 23'$  n following. A very pleasing object and easily seen.

23. In posterioribus femoribus Canis minoris.

Nov. 21.—A most minute double star. It is the small telescopic star following Procyon. A little unequal. Both w. With 278,  $\frac{1}{8}$  of a diameter of S; with 460, near  $\frac{1}{4}$  of a diameter of S. They are closer than  $\eta$  Coronæ, because their diameters, by which they are estimated, are smaller. Position  $27^{\circ} 21'$  s following. To see this very minute double star well, Procyon should be near its meridian altitude. There is a small telescopic star preceding the double star. Distance  $1' 59'' 39'''$  from centre to centre.

24.  $\zeta$  Cancrī, Fl. 16.

Nov. 21.—A most minute treble star. It will at first sight appear as only a double star, but with proper attention, and under favourable circumstances, the preceding of them will be found to consist of 2 stars, which are considerably unequal. The largest of these is larger than the single star; and the least of the 2 is less than the single star. The 1st and 2d (in the order of magnitude) pretty unequal. The 2d and 3d pretty unequal. The 2 nearest both pale r or r. With 278, but just separated; with 460,  $\frac{1}{4}$  diameter of S. Position  $86^{\circ} 32'$  n following. For measures relating to the 3d or single star, see  $\zeta$  Cancrī in the 3d class of double stars.

#### Second Class of Double Stars.

1.  $+$   $\alpha$  Geminorum, Fl. 66. In capite præcedentis II<sup>i</sup>.



April 8, 1778.—Double. A little unequal. Both w. The vacancy between the 2 stars, with a power of 146, is 1 diameter of S; with 222, a little more than 1 diameter of L; with 227,  $1\frac{1}{2}$  diameter of S; with 460, near 2 diameters of L; (see fig. 6) with 754, 2 diameters of L; with 932, full 2 diameters of L; with 1536, very fine and distinct, 3 diameters of L; with 3168, the interval extremely large, and still pretty distinct. Distance by the micrometer  $5''.156$ . Position  $32^{\circ} 47' n$  preceding. These are all a mean of the last 2 years observations, except the first with 146.

2.  $\dagger \alpha$  Herculis, Fl. 64. In capite.

Aug. 29, 1779.—A beautiful double star. Very unequal. L r; S blue inclining to green; the colours with every power the same. The interval with 222,  $1\frac{3}{4}$  diameter of L; with 227, above 2 diameters of L; with 932, above 3 diameters of L. Distance  $4''.966$ . All a mean of 2 years observations. A single measure with my last new micrometer, from centre to centre,  $4'' 34'''$ . Position  $30^{\circ} 35' s$  following.

3.  $* \epsilon$  Herculis, Fl. 75. Trium in sinistro femore, tertia.

Aug. 29.—Double. Pretty unequal. Both w. With 227,  $1\frac{1}{4}$  diameter of L; with 460, 2 diameters of S. Distance  $2''.969$ . Position  $30^{\circ} 21' n$  preceding. The measures a mean of 2 years observations.

4.  $* p$  Serpentarii, Fl. 70. Tres has sequitur, quasi supra mediam.

Aug. 29.—Double. Considerably unequal. L w; S inclining to r. With 227,  $1\frac{2}{3}$  diameter of L; with 460, much above 2 diameters of L. Position  $9^{\circ} 14' s$  following. Mean of 2 years observations.

5 et 6.  $* \epsilon$  Lyra, Fl. 4 and 5.

Aug. 29.—A very curious double-double star. At first sight it appears double at some considerable distance, and by attending a little we see that each of the stars is a very delicate double star. The first set consists of stars that are considerably unequal. The stars of the 2d set are equal, or the preceding of them rather larger than the following. The colour of the stars in the first set L very w; S a little inclining to r. In the 2d set both w. The interval between the stars of the unequal set, with a power of 227, is full 1 diameter of L; with 460, near  $1\frac{1}{2}$  diameter of L; with 932, full  $1\frac{1}{2}$  diameter; with 2010,  $2\frac{1}{3}$  diameters. The interval between the equal set with a power of 227 is almost  $1\frac{1}{2}$  diameter of either; with 460, full  $1\frac{3}{4}$  diameter; with 932, 2 diameters; with 2010,  $2\frac{1}{2}$  diameters. These estimations are a mean of 2 years observations. Position of the unequal set  $56^{\circ} 0' n$  following. Position of the equal set  $72^{\circ} 57' s$  following.

7.  $* \zeta$  Aquarii, Fl. 55. Trium in manu dextra præcedens.

Sept. 12.—Double. Equal, or the preceding rather the larger. Both w. With 227,  $1\frac{1}{4}$  diameter; with 449,  $1\frac{1}{3}$  diameter; with 460, 2 diameters; with 910, near 2 diameters; with 932,  $2\frac{1}{3}$  diameters; with 2010, pretty distinct; but too tremulous to estimate. With my 20-feet reflector, power 600, full 2 diameters, very distinct. Position  $71^{\circ} 39' n$  following. Distance  $4''.56$ , mean of 2 years observations.

8.  $\zeta$  Coronæ borealis, Fl. 7.

Oct. 1.—Double. Considerably unequal. L fine w; S w inclining to r. With 222, almost 3 diameters of L. Distance  $5''.468$ . Position  $23^{\circ} 51' n$  preceding, mean of 2 years observations.

9.  $\lambda$  Orionis, Fl. 39. In capite nebulosa.

Oct. 7.—Quadruple, or rather a double star and 2 more at a small distance. The double star considerably unequal. L w; S pale rose colour. With 222,  $1\frac{1}{2}$  diameter of L; with 449, above 2 diameters of L. Distance  $5''.833$ , a mean of all the measures. Position  $45^{\circ} 14' n$  following. As every one of the 4 stars is perfectly distinct, it is evident that the whole appeared nebulous to Flamsteed for no other reason than because his telescope had not sufficient power to distinguish them.

10 and 11.  $\sigma$  Orionis, Fl. 48. Ultiman cinguli præcedit ad austrum.

Oct. 7.—A double-treble star, or 2 sets of treble stars, almost similarly situated. Preceding set. The 2 nearest equal; the 3d larger and, compared with either of the former 2, pretty unequal. The 2 nearest with 222, about 2 diameters. Position of the following star of the 2 nearest with the 3d  $66^{\circ} 35' s$  preceding. Position of the 2 nearest, by exact estimation, 2 or  $3^{\circ} n$  following or  $s$  preceding the following set. The 2 nearest very unequal. The larger of the 2 and the farther considerably unequal. L w; S blueish. The 2 nearest with 222, about  $2\frac{1}{4}$  diameters of L; the 2 farthest  $43'' 12'''$ . Position of the 2 nearest  $5^{\circ} 5' n$  following. Position of the 2 farthest  $29^{\circ} 4' n$  following. A pretty object with 227.

12.  $\alpha$  Piscium, Fl. ultima. In nodo duorum linorum.

Oct. 19.—Double. Considerably unequal. Both w. With 222, not quite 2 diameters of L; with 460, about 3 diameters of L. Distance  $5''.123$  mean measure. Position  $67^{\circ} 23' n$  preceding.

13.  $\mu$  Draconis, Fl. 21. In lingua.

Oct. 19.—Double. Equal. Both w. With 227,  $1\frac{1}{2}$  diameter; with 460,  $2\frac{1}{2}$  diameters. Distance  $4''.354$  mean measure. Position  $37^{\circ} 38' s$  preceding or  $n$  following.

14.  $\omega$  Aurigæ, Fl. 4.

Oct. 30.—Double. Very unequal. L w; S r. With 227, almost 2 diameters of L; with 460, full 3 diameters of L. Position  $82^{\circ} 37' n$  preceding.

15.  $\psi$  Cygni, Fl. 24. In ala dextra.

Nov. 2.—Double. Extremely unequal; the small star a mere point. L w; S r. With 227, near  $1\frac{1}{4}$  diameter of L; with 278, near  $1\frac{1}{2}$  diameter of L; with 460, 2 diameters of L. Position  $89^{\circ} 32' n$  preceding.

16.  $\xi$  Cephei, Fl. 17. In pectore.

Nov. 7.—A fine double star. Considerably unequal. L w inclining to r; S dusky grey. With 222, nearly 2 diameters of L. Single measure  $5''.00$ . Position  $20^{\circ} 18' n$  preceding.

17.  $*$  In sinistro anteriore pede Monocerotis, Fl. 11.

Dec. 5.—Double. With 222, about  $1\frac{1}{2}$  diameter. Position (taken Oct. 20, 1781) with the farther of the other 2 stars  $31^{\circ} 38' s$  following. See the 10th star in the first class.



18.  $\xi$  Bootis, Fl. 37.

April 9, 1780.—Double. Very unequal. L pale r, or nearly r. S garnet, or deeper r than the other. With 222,  $1\frac{1}{2}$  diameter of L, with 460, full 3 diameters of L. Distance  $3'' 23'''$  single measure. Position  $65^\circ 53'$  n following.

19.  $g$  Serpentarii, Fl. 5.

May 2.—Double. It is a star in the body of Cancer, and the double star is at the angular point of the 3 telescopic  $g$ 's making a rectangle. Pretty unequal. Both w. With 227,  $1\frac{1}{2}$  diameter of L. Position  $82^\circ 10'$  s preceding.

20 and 21.  $\xi$  Libræ, Fl. ultima.

May 23, 1780.—Double double. The first set very unequal. L fine w. With 227, nearly 2 diameters of L.\* By the micrometer  $6'' 23'''$ , but too large a measure. Position  $1^\circ 23'$  n following. The other set both small and obscure. With 227, perhaps 5 or 6 of their diameters asunder.

22.  $\epsilon$  Persei, Fl. 45. In sinistro genu.

Aug. 2, 1780.—Double. Extremely unequal. L w; S d. With 222,  $2\frac{1}{2}$  diameters of L. Position  $81^\circ 28'$  s following, a little inaccurate. A 3d star near at about  $1\frac{1}{2}$  or  $1\frac{3}{4}$  min.

23. In constellatione Serpentarii, near Fl. 11.

Aug. 7.—Double. It is the smaller and preceding of 2 in the finder. Pretty unequal. L pale r; S dusky r. With 222, about  $1\frac{1}{4}$  diameter of L; with 278, about  $1\frac{1}{2}$  diameter of L; with 460, above 2 diameters of L. Position  $46^\circ 24'$  n preceding. A little inaccurate.

24. In constellatione Aquarii, Fl. 107. In sequenti flexu  $4^a$  ad A.

Aug. 23.—Double. In Harris's maps it is marked  $i$ . Unequal. With 227, 2 diameters; with 460, about 3 diameters.

25.  $k$  Cygni, Fl. 52.

Sept. 8.—Double. Extremely unequal. L w inclining to r; S d. and extremely faint; with 227,  $2\frac{1}{2}$  diameters of L; with 460, about 4 diameters of L, or more. Position  $31^\circ 3'$  n following.

26. In constellatione Orionis, near Fl. 42. In longo ensis.

Oct. 23.—Double. It is the most north of 3 telescopic stars in a line at the end of a cluster near  $c$ . Extremely unequal. L w; S d. With 278,  $1\frac{3}{4}$  diameter of L. Position  $26^\circ 5'$  n following.

27.  $\delta$  Geminorum, Fl. 55. In inguine sinistro sequentis IF.

March 13, 1781.—Double. Extremely unequal. L w inclining to r; S r. With 227, about  $2\frac{1}{2}$  full diameters of L; with 460, 4 or 5 diameters. Position  $85^\circ 51'$  s preceding.

28. In constellatione Aquilæ, near Fl. 54.

July 23.—Double. It is a star following  $\alpha$ . Excessively unequal. The small star is not visible with 227, nor with 278. It is visible with 460; but not without attention. Distance with 460, about 4 or 5 diameters of L. Position, by very exact estimation,  $36^\circ 28'$  n preceding.

\* In a future collection this set will be found as a treble star of the first class, the large white star, with a power of 460 and 932, appearing to be 2 stars.—Orig.

29. In constellatione Aquilæ, near Fl. 63. In medio capite.

July 31.—Double. It is the star at the vertex of a telescopic isosceles triangle near  $\tau$ . Extremely unequal. Both r. With 460, 2 diameters of L. Position  $75^\circ 48'$  n preceding.

30.  $\zeta$  Sagittæ, Fl. 8. Trium in arundine sequens.

Aug. 23.—Double. Extremely unequal. The small star brighter with 460 than with 227 or with 278; with 460, between 4 or 5 diameters of L; with 278,  $2\frac{1}{2}$  diameters of L. Distance  $5'' 27'''$  inaccurate. Position  $34^\circ 10'$  n preceding.

31. In constellatione Draconis, Fl. 56.

Sept. 6.—Double. A little unequal. Both w. With 460, near 3 diameters. Distance  $5'' 7'''$ .

32. In constellatione Sagittæ, near Fl. 4.

Sept. 7.—Double. It is the star north following  $\epsilon$ . L pale r; S d. Distance  $5'' 3'''$  inaccurate.

33.  $\beta$  Orionis, Fl. 19. In sinistro pede splendida.

Oct. 1.—Double. Extremely unequal. L w; S inclining to r. With 227,  $2\frac{1}{4}$  or  $2\frac{1}{2}$  diameters of Rigel. With 460, more than 3 diameters of L. Distance  $5'' 27'''$ . Position  $65^\circ 12'$  s preceding. The small star not wanting apparent magnitude is better to be seen with my power of 227 than with 460.

34. Trianguli, Fl. 6.

Oct. 8.—Double. It is marked  $b$  in the small triangle of Harris's maps. Very unequal. L pale r or reddish w; S blueish r. With 227, full  $1\frac{1}{4}$  diameter of L; with 460, full  $1\frac{1}{2}$  diameter of L. Position  $4^\circ 23'$  n following. A pretty object, somewhat resembling  $\alpha$  Herculis, but smaller, and not so bright.

35. In constellatione Trianguli, near Fl. 6.

Oct. 8.—Double. It is the star following  $\epsilon$ . Equal. Both dusky w. With 460, about  $2\frac{1}{2}$  diameters.

36. In constellatione Eridani, Fl. 32.

Oct. 22.—Double. Considerably unequal. L reddish w; S blue. Distance  $4'' 19'''$ . Position  $73^\circ 23'$  n preceding.

37. In capite Monocerotis.

Oct. 22.—Double. It is one of a cluster of 6 telescopic stars, arranged in pairs.

38. In constellatione Bootis.

Dec. 24.—Double. It is the most north and largest of 3 in a line, s following Fl. 15. Considerably unequal, L w; S inclining to r. Distance  $5'' 10'''$ . Position  $83^\circ 5'$  s preceding.

#### Third Class of Double Stars.

1.  $\dagger$   $\theta$  Orionis, Fl. 41. Trium contiguarum in longo ensis media.

Nov. 11, 1776.—Quadruple. It is the small telescopic Trapezium in the Nebula. Considerably unequal. The most southern star of the following side of the Trapezium is the largest; the star in the opposite corner is the smallest; the remaining 2 are nearly equal. L pale r; the star preceding L inclined to garnet; following L inclined to garnet; opposite to L d. With 460, the stars are all full, round, and well-defined. The 2 stars in the preceding side distance  $8''.780$ ; in the southern side,  $12''.812$ ; in the following side  $15''.208$ ; in the northern side,  $20''.396$ .

2.  $\zeta$  Ursæ majoris, Fl. 59. Trium in cauda media.



Aug. 17, 1779.—Double. Considerably unequal. L w; S w; inclining to pale rose colour. Distance 14".5 by 2 years observations, not a mean but that which I suppose nearest the truth. Position  $56^{\circ} 46'$  s following.

3.  $\alpha$  Cassiopeæ, Fl. 24. In cingulo.

Aug. 17.—Double. Very unequal. L fine w; S fine garnet, both beautiful colours. Distance 11".275 mean measure. Position  $27^{\circ} 56'$  n following.

4. In extremitate pedis Cassiopeæ, Fl. 55.  $\delta$  Ptolemæi.

Aug. 17.—Double. Extremely unequal. L w; S blueish r. Distance 7".5 single measure. Position  $10^{\circ} 37'$  s following. †

5.  $\gamma$  Andromedæ, Fl. 57. Supra pedem sinistrum.

Aug. 25.—Double. Very unequal. L reddish w; S fine light sky-blue, inclining to green. Distance 9".254 a mean of 2 years observations. Position  $19^{\circ} 37'$  n. following. A most beautiful object.

6.  $\beta$  Cephei, Fl. 8. In cingulo ad dextrum latus.

Aug. 31.—Double. Very unequal. L blueish w; S garnet. Distance 13".125. Position  $15^{\circ} 28'$  s preceding.

7.  $\alpha$  Scorpii, Fl. 8. Trium in fronte, lucidarum, borea.

Sept. 19.—Double. Very unequal. L whitish r; S r. Distance 14".375. Position  $64^{\circ} 51'$  n following.

8.  $\pi$  Bootis, Fl. 29.

Sept. 20.—Double. Pretty unequal. L w; S w, inclining to r. Distance 6".171. Position  $6^{\circ} 28'$  s following.

9.  $\gamma$  Arietis, Fl. 5. Quæ in cornu duarum præcedens.

Sept. 27.—Double. Equal, or if any difference the following is the larger. Distance 10".172, a mean of 2 years observations. L w, inclining a little to r; S w. Position  $86^{\circ} 5'$  n preceding.

10.  $\gamma$  Delphini, Fl. 12. Borea sequentis lateris, quadrilateri.

Sept. 27.—Double. Nearly equal, the following a little larger. Both w. Distance 11".822, being a mean of the measures taken in Sept. Oct. Nov. and Dec. 1779. As I suspect a motion in one of these stars, I thought it best not to join other observations in that measure. Position  $4^{\circ} 9'$  n preceding.

11.  $\alpha$  Bootis, Fl. 17. Trium in sinistro manu præcedens.

Sept. 27.—Double. Very unequal. L w; S d. Distance 12".503, a mean of the observations in 1779, 80, 81. Position about  $30^{\circ}$  s preceding.

12.  $\alpha$  Orionis, Fl. 44. Trium contiguarum in ense austrina.

Oct. 7.—Treble. It is the following or larger of the 2's. One is L; the other 2 are extremely small. L w; the other 2 both dusky r. Distance of the nearest 12".5. Distance of the farthest 48" 31". Position of the nearest  $43^{\circ} 51'$  following. Position of the farthest  $11^{\circ} 19'$  s following.

† In a future collection this will be found as a treble star of the first class; the large star having a small one preceding, easily seen with 400 and 932.—Orig.

13 and 14.  $\alpha$  Orionis, Fl. 44. Trium contiguarum in ense austrina.

Oct. 7.—Double treble. It is the preceding or smallest of the 2's. The preceding set, forming a triangle, consists of 3 equal stars. All dusky r. Distance of the 2 nearer, with 227, about 3 diameters. The following set, forming an arch, consists of 3 stars of different sizes. The middle star is the largest; that to the south is also pretty large; and the 3d is very small. L w; l w; S pale r. Distance 36".25.

15.  $\mu$  Cygni, Fl. 78.

Oct. 19.—Double. Considerably unequal. L w; S blueish. Distance 6".927 mean measure. Position  $26^{\circ} 15'$  s following.

16.  $\alpha$  In constellatione Delphini, Fl. 1.

Nov. 15.—Double. It is the star south preceding. A little unequal. Both w. Distance 12".5. Position  $9^{\circ} 42'$  s preceding.

17. In extremitate caudæ Lacertæ, near Fl. 1.

Nov. 20.—Double. Considerably unequal. L w; S d; inclining to r. Distance 15" 43" inaccurate. Position  $76^{\circ} 16'$  s preceding.

18.  $\gamma$  Virginis, Fl. 29. De quatuor in ala sinistra, sequens.

Jan. 21, 1780.—Double. Equal. Both w. Distance 7".333 mean measure. Position  $40^{\circ} 44'$  s following.

19.  $\zeta$  Cancræ, Fl. 16.

April 5.—Double. Considerably unequal. L pale r; S pale r. Distance 8".046 mean measure. Position  $88^{\circ} 16'$  s preceding. See the 24th in the first class.

20. In constellatione Bootis.

June 25.—Double. Draw a line through  $\pi$  and  $\zeta$  to the small star under the right foot, and erecting a perpendicular towards the left foot of equal length, the end of it will mark out this double star. Pretty unequal. Both r. Distance 7" 36" full measure. Position  $59^{\circ} 32'$  n preceding.

21. In constellatione Equulei, Fl. 1.

Aug. 2.—Double. Considerably unequal. L w; S much inclining to r. Distance 9".375 mean measure. Position  $5^{\circ} 39'$  n following. A 3d small star follows at some distance.

22. Quæ infra oculum Lyncis, Fl. 12.

Aug. 7.—Double. With 222, about 3 diameters of L. Considerably unequal. L w; S pale r. Distance 9" 23", not extremely accurate. Position  $32^{\circ} 33'$  n preceding. See the 6th star in the first class.

23. In constellatione Cassiopeæ, Fl. 34.

Aug. 8.—Double. It is one of 2 telescopic stars, and is marked  $\phi$  in Harris's maps. Extremely unequal. L pale r; S d. Distance about 12" or more.

24.  $\theta$  Sagittæ, Fl. 17.

Aug. 8.—Treble. The 2 nearest extremely unequal. L pale r; S d. Third star pale r. Distance of the 2 nearest 11" 8". Distance of the 2 largest 57" 49".

25.  $\alpha$  In constellatione Serpentarii, Fl. 39.

Aug. 24.—Double. It is the more south and larger of 2 in the finder. Very unequal. L w; S



inclining to blue. Distance  $10'' 2'''$ , a little inaccurate. Position  $87^\circ 14'$  n preceding.

26. \* In constellatione Cerberi 1. Hevelii 1<sup>a</sup>. Fl. Herculis 95.

Sept. 8.—Double. It is the star in the leaf nearest to Hercules's face and hand. Equal. Preceding w. Following blueish w. Distance  $6'' 6'''$ . Position  $4^\circ 9'$  s preceding or n following.

27. In constellatione Navis, near Fl. 3.

Feb. 15, 1781.—Double. It is a star between  $\gamma$  Canis majoris and  $\xi$  Navis. Equal. Distance about  $15''$ .

28. In constellatione Navis, near Fl. 9.

Feb. 15.—Double. It is one of 2 telescopic stars under Monoceros. Distance about  $8''$ .

29. In naribus Monocerotis, Fl. 8. ::

Feb. 15.—Double. Distance about  $12''$ .

30. \* In constellatione Leonis, Fl. 54. Duarum supra dorsum sequens.

Feb. 21.—Double. Considerably unequal. L brilliant w; S ash-colour, or greyish w. Distance  $7'' 6'''$  mean measure. Position  $9^\circ 14'$  s following.

31. In constellatione Herculis.

May 20.—Double. Over  $\epsilon$  :: Equal. Both very small. Distance about  $10''$ .

32. In constellatione Aquilæ, Fl. 11.

July 25.—Double. It is the more south of 2 near  $\epsilon$  and  $\zeta$ . Excessively unequal. S hardly visible with 227, but pretty strong with 460. Distance about  $7''$ .

33. In constellatione Aquilæ, near Fl. 7 and 8.

July 30.—Double. It is a star preceding the 2 small stars north of  $k$  and  $l$ . Unequal. L w; S blueish w. Distance  $11'' 35'''$  inaccurate, but not much.

34. In constellatione Aquarii, Fl. 94.

Aug. 20.—Double. Between  $\psi$  and  $\omega$  towards  $\delta$ . Very unequal. Distance  $13'' 45'''$ . L pale r; S d.

35. In Constellatione Serpentarii, Fl. 54.

Aug. 21.—Double. It is the preceding of 2 stars in the head. Excessively unequal. L reddish w; S d. Distance about  $8''$ .

36. In constellatione Persei.

Sept. 14.—Double. A little south of  $\gamma$ . Considerably unequal. L w; S w, inclining to r. Distance  $11'' 53'''$ , rather full measure.

37 and 38. In constellatione Persei, near Fl. 38. ‡

Sept. 24.—Double-double. South preceding the first  $\alpha$ . The equal set with 227, about 4 or 5 diameters. The unequal set about 5 or 6 diameters. Near this last set is also a 3d star forming an obtuse angle with the stars of this set. Distance about  $10''$ .

39.  $\alpha$  Persei, Fl. 40.

Sept. 24.—Double. It is the 2d or more northern  $\alpha$ . Extremely unequal. L w; S d. With 227, S is hardly visible; with 460, it appears at first sight. Distance  $14'' 59'''$ , inaccurate on account of the obscurity of S.

40. In constellatione Herculis, near Fl. 87.

Oct. 10.—Double. Of 3 stars, forming an obtuse angle, whereof Fl. 87, (a star south of  $\mu$ ) is at the angular point, that towards Ramus Cereb. Extremely

unequal. L w; S d. Distance  $10'' 20'''$ . Position  $19^\circ 37'$  s following.

41. \*  $\epsilon$  Herculis, Fl. 43.

Oct. 10.—Double. Equal. Preceding star w. A little inclined to r. Following w. Distance  $11'' 43'''$ . Position  $88^\circ 23'$  n following.

42. In constellatione Trianguli.

Oct. 10.—Double. It is a star north following  $\delta$ . Unequal. L reddish. S blueish. Both d. Distance about 6 or  $7''$ .

43. In sinistro anteriore pede Monocerotis.

Oct. 20.—Double. It is the more south of 2 telescopic stars preceding the treble star. Extremely unequal. L w; S d. Position  $23^\circ 39'$  n preceding.

44. In ore Monocerotis.

Oct. 20.—Double. Considerably unequal. L w; S r. Distance  $12'' 30'''$ . Position  $60^\circ 14'$  n following.

45. In constellatione Tauri, near Fl. 10.

Oct. 22.—Double. It is near the star sub pede et scapula dextra. Extremely unequal. L pale r; S d. Position  $35^\circ 33'$  s preceding.

46. In constellatione Monocerotis.

Oct. 22.—Double. It is the star following the tip of the ear.

#### Fourth Class of Double Stars.

1.  $\alpha$  Ursæ minoris, Fl. 1. Stella Polaris.

Aug. 17, 1779.—Double. Extremely unequal. L w; S r. Distance  $17'' 15'''$ . Position  $66^\circ 42'$  s preceding.

2. \*  $\gamma$  Lyræ, Fl. 20. Duarum contiguarum ad ortum a testa, borea.

Aug. 29.—Double. Considerably unequal. L w; S r. Distance  $25'' 42'''$ . Position  $31^\circ 51'$  s preceding. Three other stars in view.

3. Fl. 64. Sagittarii.

Sept. 19.—Double. It is the preceding star of two. Extremely unequal. Distance about  $25''$ .

4.  $\alpha$  Persei, 1. Hevelii 9. In dextro brachio.

Sept. 20.—Double. Very unequal. L r; S blue. Distance  $26''$ , very inaccurate. Position  $20^\circ 3'$  n preceding.

5. In constellatione Arietis, Fl. 33. Quatuor inform. sup. dors. præc.

Sept. 27.—Double. It is the first in the head of the fly. L w; S d. Considerably unequal. Distance  $25'' 32'''$  inaccurate. Position  $87^\circ 14'$ .

6. †  $\theta$  Serpentis, Fl. 63. In extremitate Caudæ.

Oct. 17.—Double. Equal. Both w. Distance  $19'' 375$ .

7.  $\psi$  Draconis, Fl. 31. Prima ad  $\psi$ .

Oct. 19.—Double. Pretty unequal. L w; s pale r. Distance  $28'' 14'''$ .

8. \*  $\zeta$  Piscium, Fl. 86. Trium in lino lucidarum sequens.

Oct. 19.—Double. Pretty unequal. L w; S w inclining to blue. Distance  $22''.187$ , not very accurate. Position  $22^\circ 37'$  n following.

9. \* Prima ad  $\psi$  Piscium, Fl. 74. Trium in pinna costarum præcedens.

Oct. 30.—Double. Distance  $27''.5$ . Position about  $80^\circ$  s following. An obscure star also within  $1\frac{1}{2}$  min.

10.  $\alpha$  Tauri, Fl. 59. Australis sequentis lateris quadrilateri, in cervice.

‡ Mr. Bryant of Bath first observed these stars.—Orig.



Oct. 30.—Double. Distance  $18''.75$ , very inaccurate.

11.  $\gamma$  Cygni, Fl. 17.

Nov. 20.—Double. Very unequal. L w; S dusky r. Distance  $24'' 52'''$ .

12.  $\ast \psi$  Aquarii, Fl. 91.

Nov. 26.—Double. It is the first of 3  $\psi$ 's. Unequal. Distance  $23'' 5'''$ , pretty accurate.

13. In constellatione Leonis, Fl. 83.

April 6, 1780.—Double. It is a small star north preceding  $\tau$ . A little unequal. Both inclining to r. Distance  $29'' 5'''$ . Position  $54^\circ 55'$  s following.

14. In constellatione Aquilæ, Fl. 57.

Aug. 2.—Double. It is the preceding of 2, near the south end of Antinous's bow. A little unequal. L w; S w, inclining to r. Distance  $29'' 28'''$ , pretty accurate. Position  $81^\circ 55'$  s preceding.

15. In dextra aure Camelopardali. I. Hevelii ultima.

Aug. 2.—Double. A little unequal. L reddish w; S reddish w. Distance  $20'' 5'''$ .

16. In constellatione Cassiopeæ, Fl. 31.

Aug. 2.—Double. It is marked with the letter A in Harris's maps. Distance about  $20''$  or more.

17.  $\ast$  Cor Caroli, Fl. 12. Canum Venaticorum.

Aug. 7.—Double. Very unequal. L w; S inclining to r. Distance  $20'' 0'''$ , inaccurate. Position  $41^\circ 47'$  s preceding.

18.  $\ast$  In constellatione Cygni, Fl. 61.

Sept. 20.—Double. It is a star preceding  $\tau$ . Pretty unequal. L pale r; S r; or L r; S garnet. Distance  $16'' 7'''$ . Position  $36^\circ 28'$  n following.

19. In constellatione Aurigæ, Fl. 14.

Sept. 24.—Double. It is the preceding star of a cluster of stars that precede  $\phi$  and  $\chi$ . Very unequal. L reddish w; S d. Distance  $16'' 8'''$ , a little inaccurate. Position  $37^\circ 38'$  s preceding.

20.  $\ast$  Draconis, Fl. 47.

Oct. 3.—Double. Very unequal. L pale r; S dusky r. Distance  $26'' 39'''$ . Position  $90^\circ$  n preceding or following, by exact estimation.

21.  $\zeta$  Orionis, Fl. 50. Trium in cingulo sequens.

Oct. 10.—Double. Very unequal. L w; S d. Distance about  $25''$ . Position  $83^\circ 25'$  n following, very inaccurate.

22.  $f$  Cygni, Fl. 63. ::

Oct. 27.—Double. Extremely unequal. L fine w; S d. Distance  $18'' 11'''$ .

23.  $3^a$  ad  $\omega$  Cygni, Fl. 46. In genu dextro.

Oct. 27.—Double. Considerably unequal. L reddish w; S d. Distance within  $30''$ . Position  $7^\circ 23'$  n preceding.

24.  $3$  ad  $\omega$  Cygni, Fl. 46 adjacens in genu dextro.

Oct. 27.—Treble. Very unequal, and extremely unequal. L fine garnet; S r; smallest d. All within  $30''$ . Position of the brighter of the two small stars  $44^\circ 19'$  n preceding. Position of the faintest — preceding.

25. In constellatione Ceti, Fl. 66.

Dec. 23.—Double. It is a star near the place of the periodical star  $\alpha$ . Distance  $16''.875$ , a little inaccurate.

26. In constellatione Navis, Fl. 19. ::

Feb. 15, 1781.—Double. It is a star under the ham of Monoceros's right-foot. Distance about  $25''$ .

27. In constellatione Comæ Berenices, Fl. 24.

Feb. 28.—Double. Considerably unequal. L whitish r; S blueish r. Mean distance  $18'' 24'''$ . Position  $3^\circ 28'$  n preceding.

28. In constellatione Geminorum.

March 13.—Double. It is near  $\gamma$  towards  $\zeta$  Tauri. A little unequal. Both r. Distance  $19'' 41'''$ . Position  $57^\circ 0'$  f preceding.

29.  $h$  Ursæ majoris, Fl. 23. Duarum in collo sequens.

April 25.—Double. Extremely unequal. L reddish w; S d. Distance with 460,  $19'' 26'''$ . Position  $3^\circ 14'$  n preceding.

30. In constellatione Lynceis, Fl. 43. Præcedens ad boream.

May 26.—Double. It is the eye or nose of Leo minor. Unequal. Distance  $24'' 53'''$  inaccurate.

31. In constellatione Cephei, near Fl. 27.

May 27.—Treble. It is a star near  $\delta$ . Distance of the nearest about  $20''$ .

32.  $\ast$  In constellatione Serpentarii, Fl. 61.

July 15.—Double. It is a star near  $\gamma$ . A little unequal. L w; S grey. Distance  $19'' 4'''$ , inaccurate. Position almost directly following.

33. In constellatione Aquilæ.

July 19.—Treble. It is the first of 2 stars preceding  $v$ . Distance of the 2 nearest  $21'' 59'''$ , inaccurate.

34. In constellatione Aquilæ, near Fl. 64.

July 25.—Double. It is near a star preceding  $\theta$ . Equal distance about  $30''$ .

35.  $\beta$  Delphini, Fl. 6. Austrina præcedentis lateris quadrilateri.

Aug. 1.—Double. Extremely unequal. Hardly visible with 227; pretty strong with 460. Distance  $25'' 54'''$ , rather narrow measure. Position  $78^\circ$  n preceding, by exact estimation.

36.  $\beta$  Serpentis, Fl. 28. In eductione colli.

Aug. 13.—Double. Extremely unequal. L w; S extremely faint. Distance  $24''$ , pretty exactly estimated. Position 3 or  $4^\circ$  s preceding, too obscure for measuring.

37.  $\delta$  Equuloi, Fl. 7. Duarum in ore sequens.

Aug. 13.—Double. Excessively unequal. S hardly visible with 227; but with 460, visible at first sight. L w; S d. Distance  $19'' 32'''$ . S, too obscure to be very accurate. Position  $11^\circ 39'$  n following.

38. In constellatione Aquarii, Fl. 24.

Aug. 14.—Double. It is the star in the cheek or hair of the neck. Very unequal. L w; S d. Distance  $25''$ , very inaccurate.

39. In constellatione Cygni.

Oct. 1.—Double. It is a star north following  $\sigma$ . Extremely unequal. L w; S d. Distance  $18''$  exact estimation. Position  $30^\circ 28'$  s following.

40.  $\alpha$  Trianguli, Fl. 10.

Oct. 8.—Double. It is the preceding of 3 telescopic stars. Unequal. Distance  $17'' 19'''$ , pretty accurate.



41.  $\mu$  Herculis, Fl. 86.

Oct. 10.—Double. Excessively unequal. The small star is not visible with 227, nor with 278. I saw it very well with 460. L inclined to pale r; S d. Distance, by pretty exact estimation, 18". Position, by very exact estimation,  $36^\circ$  s preceding.

42. In constellatione Herculis.

Oct. 10.—Double. It is a star just by v. Considerably unequal. L inclined to r; S inclined to blue. Distance 18" 19". Position  $4^\circ$  58' n preceding.

43.  $\lambda$  Eridani, Fl. ultima. In origine fluvii.

Oct. 22.—Double. It is the middle of 3 telescopic stars. Very unequal. L w; S r.

44. In constellatione Tauri, near Fl. 4.

Dec. 22.—Double. It is a small telescopic star south following s. Extremely unequal. L w; S d.

*Fifth Class of Double Stars.*

1.  $\delta$  Herculis, Fl. 11. In sinistro humero.

Aug. 9, 1779.—Double. Extremely unequal. L w; S inclining to r. Distance 33".75. Position  $7^\circ$  28' s following.

2.  $\ast$   $\zeta$  Lyræ, Fl. 6.

Aug. 29.—Double. Pretty unequal. L w; S w inclining to pale rose colour. Distance 41" 58", perhaps a little inaccurate. Position  $62^\circ$  18' s following, a little inaccurate.

3.  $\ast$   $\beta$  Lyræ, Fl. 10. Duarum in jugimento borea.

Aug. 29.—Quadruple. All w. First and 2d considerably unequal. First and 3d very unequal. First and 4th very unequal. The 2d a little inclining to r. The 3d and 4th more inclining to r. Distance of the 1st and 2d 43" 57". Position  $60^\circ$  28' s following, a little inaccurate.

4.  $\delta$  Cephei, Fl. 27. Sequitur tiam.

Aug. 31.—Double. Considerably unequal. L reddish w; S blueish w. Distance 38" 18", a bright object.

5.  $\dagger$   $\beta$  Cygni, Fl. 6. In ore.

Sept. 12.—Double. Considerably unequal. L pale r; S a beautiful blue. The estimation of the colours the same with 227 and 460. Distance 39" 32", pretty accurate. Position  $36^\circ$  28' n following.

6.  $\ast$   $\nu$  Scorpii, Fl. 14. Duarum adjacentium boreæ frontis, borea.

Sept. 19.—Double. Very unequal. Both w. Distance 38" 20", pretty accurate. Position  $69^\circ$  28' n preceding.

7.  $\mu$  Sagittarii, Fl. 13. In summo arcu, borealis.

Sept. 19.—Treble. Two small stars near on each side. L w; S both r. Distance of the nearest about 30". Position—preceding, the other—following.

8.  $\ast$  Herculis, Fl. 7. In dextri brachii ancone.

Sept. 20.—Double. A little unequal. L r; S garnet; or L pale r; S r. When the stars are low the first estimation of the colours will take place. Distance 39" 59". Position  $79^\circ$  37' n following. Has a 3d star.

9.  $\iota$  Bootis, Fl. 21.—Trium in sinistra manu, media.

Sept. 27.—Double. Very unequal. L w; S d. Distance 37" 56. This is not a mean of the mea-

asures; for I suspect a motion in one of the stars, which another year or two may show. Position  $52^\circ$  51' n following.

10.  $\ast$   $\delta$  Orionis, Fl. 34. Trium in cingulo præcedens.

Oct. 6.—Double. Considerably unequal. L w; S blueish r. Distance 52".968 full measure. Position  $88^\circ$  10' n preceding.

11.  $\dagger$   $\nu$  Draconis, Fl. 24 and 25. In ore duplex.

Oct. 19.—Double. A little unequal. L pale r; S pale r. Distance 54" 48". Position  $44^\circ$  19' n preceding.

From the right ascension and declination of these stars in Flamsteed's catalogue we gather, that in his time their distance was 1' 11".418; their position  $44^\circ$  23' n preceding; their magnitude equal or nearly so. The difference in the distance of the 2 stars is so considerable, that we can hardly account for it otherwise than by admitting a proper motion in either one or the other of the stars, or in our solar system; most probably neither of the 3 is at rest.

12.  $\ast$   $\lambda$  Arietis, Fl. 9. In vertice.

Oct. 30.—Double. Considerably unequal. L pale r; S dusky garnet. Distance 36" 44", a little inaccurate. Position 42' 0' n following.

13.  $\phi$  Tauri, Fl. 52. Borea sequentis lateris quadrilateri in Cervice.

Oct. 30.—Double. Distance 55".625, inaccurate.

14. In constellatione Monocerotis.

Dec. 5.—Multiple. It is a spot over the right fore-foot; 4 or 5 small stars within 1 minute.

15.  $c$  Ursæ majoris, Fl. 16.

May 2, 1780.—Double. Very unequal. L whitish r; S d. Distance with 460, 48" 59". Position  $79^\circ$  51' s preceding.

16.  $\sigma$  Piscium, Fl. 76. Duarum in ore piscis sequentis borealior.

Aug. 3.—Double. Extremely unequal. L pale r; S dusky r. Distance 48".125, pretty accurate. Position  $15^\circ$  28' n preceding.

17.  $\pi$  Andromedæ, Fl. 29. In dextro humero.

Aug. 25.—Double. Extremely unequal. L w; S blueish. Distance 34" 12", inaccurate.

18.  $\alpha$  Cassiopeæ, Fl. 18. In pectore.

Aug. 31.—Double. Extremely unequal. L pale r; S d. Distance 52".812. Position  $5^\circ$  26' n preceding.

19.  $\lambda$  Herculis, Fl. 20. In dextro brachio.

Sept. 4. Double. Extremely unequal. L reddish w; S r distance 41" 49", a little inaccurate. Position  $19^\circ$  30' s preceding.

20.  $e$  Pegasi, Fl. 1.

Sept. 8.—Double. Very unequal. L pale r; S d; Distance 37" 5", pretty accurate. Position  $38^\circ$  19' n preceding.

21.  $\tau$  Aurigæ, Fl. 29.

Sept. 26.—Double, about 30".

22.  $\lambda$  Aurigæ, Fl. 15.

Sept. 30.—Multiple. Two, within about 30".

23. In constellatione Orionis.

Oct. 10.—Double. It is a star following f. Distance about 40".



24. In constellatione Ceti, Fl. 37.

Oct. 12. Double. It is a star between  $\eta$  and  $\theta$  towards the north. Distance  $42''.812$ , inaccurate.

25.  $\tau$  Orionis, Fl. 20, supra talem in tibia.

Oct. 23.—Double. Very unequal. Distance about  $30''$ .

26.  $h$  Leonis, Fl. 6.

Feb. 21, 1781.—Double. Very unequal. L r; S d. Distance  $36'' 9'''$ . Position  $12^\circ 55'$  n following.

27. In constellatione Libræ, near Fl. 31.

May 24. Double. The most south of 3 small stars in the finder. Equal, or the preceding rather the larger. Both w inclining to pale r. Distance  $44'' 12'''$ , a little inaccurate. Position  $40^\circ 17'$  s following.

28. In constellatione Cephei.

May 27.—Double. It is a star near  $\beta$ . Extremely unequal. Distance about  $30''$ .

29.  $\nu$  Serpentis, Fl. 53. Post dextrum femur Serpentarii.

July 16.—Double. Unequal. Distance about  $35''$ .

30. In constellatione Serpentarii, Fl. 53.

July 19.—Double. It is a star between  $\alpha$  and  $\beta \frac{1}{3}$  of the way from  $\alpha$ . Very unequal. L w; S inclining to r. Distance  $32'' 21'''$ , narrow measure.

31. In constellatione Aquilæ.

July 19.—Double. It is the star next but one preceding  $\delta$ . Very unequal. L r; S d. Distance about  $30''$ .

32.  $\alpha$  Andromedæ.

July 21.—Double. Extremely unequal. The small star better with 460 than with 227. L w; S d. Distance  $55'' 32'''$ , rather narrow measure. Position  $10^\circ 37'$  s preceding.

33.  $h$  Aquilæ, Fl. 15.

July 25.—Double. Unequal. Both pale r. Distance  $33'' 53'''$ , inaccurate.

34. In constellatione Aquilæ; A Fl. 28.

July 25.—Double. It is one of 2 stars near A. Distance about  $35''$ .

35. In constellatione Aquilæ.

July 25.—Double. It is a star near that which follows  $\epsilon$ . Very unequal. Distance about  $40''$ .

36.  $\sigma$  Scuti, Fl. 2, in constellatione Aquilæ.

July 30.—Double. Very unequal. L pale r; S d. Distance  $42'' 44'''$ , a little inaccurate.

37.  $\zeta$  Coronæ, Fl. 18.

Sept. 21.—Treble. Very unequal. L w; S both r. Dist. of the nearest about  $50''$ ; the farthest  $1\frac{1}{2}$  min.\*

38. In Constellatione Herculis, Fl. 23.

Sept. 21.—Double. It is the star between  $\nu$  and  $\xi$  Coronæ, the largest of a telescopic triangle. Distance  $36'' 27'''$ , rather narrow measure. L w; S w; inclining to r.

39.  $\alpha$  Lyræ, Fl. 3. In testa fulgida.

Sept. 24.—Double. Excessively unequal. By moonlight I could not see the small star with 278, and saw it with great difficulty with 460; but in the absence of the moon I have seen it very well with 227. L fine brilliant w; S dusky. Distance  $37'' 13'''$ . Position  $26^\circ 46'$  s following.

\* In a future collection the small star at the obtuse angular point will be found as a double star of the 2d or 3d class.

Oct. 22. Having often measured the diameters of many of the principal fixed stars, and having always found that they measured less and less the more I magnified, I fixed on this fine star for taking a measure with the highest power I have yet been able to apply, and on the largest scale of my new micrometer I could conveniently use. With a power of 6450 (determined by experiments on a known object at a known distance) I looked at this star for at least a quarter of an hour, that the eye might adapt itself to the object; having experimentally found, that the aberration by this means will appear less and less, and, in the telescope I used on this occasion with powers from 460 to 1500, will often quite vanish, and leave a very well-defined circular disc for the apparent diameter of the stars. The diameter of  $\alpha$  Lyræ, by this attention, appeared perfectly round, and occasionally separated from rays that were flashing about it. From the very brilliant appearance of the star with this great power, and a pretty accurate rough calculation founded on its apparent brightness, when observed with the naked eye with 227, with 460, with 6450, I surmise, that it has light enough to bear being magnified at least a hundred thousand times with no more than 6 inches of aperture, provided we could have such a power, and other considerations would allow us to apply it. When I had as good a view as I expected to have, I took its diameter with my new micrometer on a scale of 8 inches and  $4428$  ten thousandth to  $1''$  of a degree, and found it subtended an angle of  $0''.3553$ . I had no person at the clock; but suppose the time of its passing through the field of my telescope, which in this great power is purposely left undefined, and as large as possible, was less than 3 seconds.

40.  $\nu$  Lyræ, Fl. 8.

Sept. 24.—Treble. Extremely unequal. L w; S both d. One n preceding, the other s following. Distance of the following star  $56'' 47'''$ , a little inaccurate. Position of the same  $28^\circ 27'$  s following.

41. A Persei, Fl. 43.

Sept. 24.—Double. Unequal. L w. Distance about  $50''$ .

42. In constellatione Lyræ.

Sept. 25.—Double. It is a small star just by  $\eta$ . A little unequal. Both r. Distance  $38'' 8'''$ . Position  $26^\circ 18'$  n following.

43. In constellatione Cygni, Fl. 76.

Oct. 1.—Double. It is the 3d star from  $\rho$  towards  $\nu$ . Unequal. Distance  $48''$  by exact estimation. Position—preceding.

44. In constellatione Cygni, Fl. 69.

Oct. 1.—Treble. Very unequal. L w; S both reddish. Position—preceding.

45. In constellatione Cygni.

Oct. 1.—Double. It is the more south of 2 telescopic stars following  $\tau$ . Very unequal. L w; S d. Distance  $44''$  by exact estimation. Position—following.

46.  $c$  Cygni, Fl. 16.  $1^a$  ad  $c$ .

Oct. 5.—Double. It is the star next following  $\theta$ . Almost equal. Both pale r. Distance  $30''$ , by pretty exact estimation.

47.  $c$  Cygni, Fl. 26.  $2^a$  ad  $c$ .

Oct. 8.—Double. Very unequal. L reddish w; S



S dusky r. Distance 39" by pretty exact estimation.

48. \* In constellatione Piscium.

Oct. 8.—Double. It is a telescopic star just by  $\theta$  southwards. Both d. Distance about 45".

49. \* In constellatione Arietis, Fl. 30.

Oct. 15.—Double. It is a small star over the ram's back. Nearly equal. Distance 31" 6", inaccurate.

50.  $\gamma$  Leporis, Fl. 13. In posterioribus pedibus austrina.

Oct. 22.—Double. Considerably unequal. Distance about 40".

51. In constellatione Sagittæ.

Nov. 23.—Double. It is a star north following  $\epsilon$ . Extremely unequal. Distance 32" 48". L r; S blue.

#### *Sixth Class of Double Stars.*

1.  $\alpha$  Ceti, Fl. 68. In pectore nova.

Oct. 20, 1777.—Double. Very unequal. L garnet. S dusky. Distance mean of some very accurate measures 1' 44".213; mean of other very accurate measures 1' 53".032. As I can hardly doubt the motion of this star, I have given the mean of the most accurate measures separately; and hope in a few years time to be able to give a better account of it.

2.  $\alpha$  Serpentarii, Fl. 67.

Aug. 29, 1779.—Double. Distance about 1½ min.

3.  $\delta$  Lyræ, Fl. 11.

Aug. 29, 1779.—Double. Extremely unequal. L w; S d. Distance about 4', pretty exact estimation.

4.  $\alpha$  Capricorni, Fl. 5.

Sept. 19.—Double. Very unequal. L r; S d. Distance about 1½ min. Position—s preceding.

5. In constellatione Arietis, Fl. 41, supra dorsum.

Sept. 27.—Double. It is the star in the body of the fly. Distance 2' 5" 35".

6.  $\epsilon$  Capricorni, Fl. 39. Duarum in eductione caudæ præced.

Sept. 27.—Double. Unequal. L pale r. Distance about 1½ min.

7. \*  $\tau$  Tauri, Fl. 94. In eductione cornu borei.

Oct. 6.—Double. Distance 1' 11" 25", pretty accurate.

8.  $\kappa$  Tauri, Fl. 56 and 57.

Oct. 6.—Double. At a considerable distance.

9. \*  $\zeta$  Geminorum, Fl. 43. In sinistro genu sequentis  $\Pi^i$ .

Oct. 7.—Double. Very unequal. L reddish w; S dusky r. Distance 1' 31" 52", rather full measure. Position 81° 14' n preceding.

10.  $\alpha$  Cygni, Fl. 31. Duarum in dextro pede sequens.

Nov. 2.—Double. Considerably unequal. L pale r. S blue. It is the following star of the two  $\alpha$ 's that are close together. Distance 1' 39" 57". Position 87° 14' s preceding.

11. \*  $\alpha$  Leonis, Fl. 32. In corde.

Nov. 14.—Double. Very unequal. L w; S d. Distance 2' 48" 20". Position 30° 5' n preceding.

12. \*  $\tau$  Leonis, Fl. 84. Quasi in cubito.

April 6.—Double. Considerably unequal. L r; S inclining to blue. Distance 1' 22" 42". Position 75° 21' s following.

13.  $\alpha$  Leonis, Fl. 95. In extremitate caudæ.

April 6.—Double. Extremely unequal. L reddish w; S d. Distance about 1½ min. Position about 80° n following.

14.  $\kappa$  Serpentis, Fl. 58. In cauda.

June 19, 1780.—Double. Extremely unequal. L pale r; S d. Distance 1' 21" 2". Position 9° 7' s following.

15. In constellatione Bootis, near Fl. 6.

June 25.—Double. It is a telescopic star near that which forms a rectangle with  $\alpha$  and  $\kappa$ . Distance about 2'.

16.  $\delta$  Bootis, Fl. 49. In dextro humero.

July 23.—Double. Considerably unequal. Distance about 2½ min. L reddish w. S w. Position 5° 46' n following.

17.  $\mu$  Bootis, Fl. 51. In baculo recurvo.

July 30, 1780.—Double. Unequal. Distance 2' 8", exact estimation. Position 80° 25' s following. L reddish w. S pale r. See the 17th star of the first class.

18.  $\nu$  Coronæ, Fl. 21.

July 30.—Double. Very unequal. L r; S garnet. At some considerable distance. Position about 80° n following.

19.  $\kappa$  Persei.

Aug. 2.—Multiple. An astonishing number of small stars all within the space of a few minutes. I counted not less than 40 within my small field of view.

20.  $\mu$  Persei, Fl. 51. Duarum in dextro poplite sequens.

Aug. 2.—Double. Very unequal. L w. Distance about 1½.

21.  $\kappa$  Pegasi, Fl. 44.

Aug. 23.—Double. Distance about 2½ min.

22. In constellatione Draconis, I. Hevelii 69.

Aug. 7.—Double. It is the star between  $\alpha$  Draconis and the tail of Ursa major. Distance about 3½ min.

23. In naribus Lyncis.

Aug. 7.—Double. Distance about 2'.

24.  $\delta$  Cassiopeæ, Fl. 4.

Aug. 12.—Treble. Two are large. Distance about 2'. A 3d is obscure. Distance about 1½ min. They form almost a right angle.

25. In constellatione Cassiopeæ, Fl. 3.

Aug. 18.—Double. Distance about 2½ min.

26.  $\epsilon$  Sagittæ, Fl. 11.

Aug. 19.—Double. Very unequal. L r; S r inclining to blue. Distance 1' 31" 53". Position 8° 32' s following.

27. In constellatione Aquilæ.

Aug. 24.—Double. A star north of  $\theta$ . Distance about 1'.

28.  $\beta$  Capricorni, Fl. 9. Trium in sequente cornu austrina.

Aug. 26.—Double. Considerably unequal. Distance about 3'. Position—preceding.



29.  $\epsilon$  Capricorni, Fl. 11. Trium in rostro præcedens.
- Aug. 26.—Double. Distance about  $2\frac{1}{2}$ .
30.  $\alpha$  Aurigæ, Fl. 13. In humero sinistro.
- Sept. 8.—Double. Extremely unequal. L w; S d. Dist.  $2' 49'' 8'''$ . Position  $61^\circ 23'$  s following. With a power of 227, and my common micrometer, the diameter of this star measured  $2''.5$  The circumference was remarkably well defined.
31.  $d$  Tauri, Fl. 88. In sinistro cubito.
- Sept. 24.—Double. Distance  $1' 10''.625$ . A little inaccurate.
32.  $\lambda$  Cygni, Fl. 54.
- Sept. 20.—Double. Extremely unequal. L blueish w; S d. Distance about  $1'$ . Position  $12^\circ 42'$  s following.
33. In constellatione Cygni, Fl. 32.
- Sept. 20.—Double. Distance about  $2'$ .
34.  $\theta$  Aurigæ, Fl. 37. In dextro carpo.
- Sept. 26.—Double. Distance about  $2\frac{1}{3}$ .
35. In constellatione Camelopardali, Fl. 13.
- Sept. 26.—Double. It is the star over the goat's-head. Distance about  $2'$ .
36. In constellatione Camelopardali, Fl. 10.
- Sept. 30.—Double. Distance about  $1\frac{1}{2}$ .
37.  $c$  Draconis, Fl. 46. In flexura colli.
- Oct. 3.—Double. Distance 3 or  $4'$ . A rich spot.
38.  $c$  Draconis, Fl. 64 or 65.
- Oct. 3.—Double. Distance about  $2'$ .
39.  $\alpha$  Orionis, Fl. 58. In dextro humero lucida rutilans.
- Oct. 10.—Double. Extremely unequal. L r but not deep; S d. Distance  $2' 41'' 46'''$ . Position  $62^\circ 18'$  s following.
40.  $\gamma$  Leporis, Fl. 13.
- Feb. 21, 1781.—Double. Distance about  $2\frac{1}{2}$ .
41.  $\rho$  Cancræ 5 ad  $\rho$ , Fl. 67.
- Feb. 21. Double. Very unequal. L reddish w; S d. Distance  $1' 35'' 59'''$ . Position  $50^\circ 33'$  n preceding.
42.  $\beta$  Geminorum, Fl. 78. In capite sequentis  $\Pi^1$ .
- Mar. 13.—Multiple. Extremely unequal. The nearest distance  $1' 56'' 45'''$ , rather full measure. Position  $24^\circ 28'$  n following, not extremely accurate. This is the smallest. The next distance  $3' 17'' 19'''$ , pretty accurate. Position  $15^\circ 56'$  n following.
43.  $\theta$  Virginis, Fl. 51. De quatuor ultima et sequens.
- May 14.—Double. Extremely unequal. L w; S d. Distance  $1' 3'' 53'''$ , inaccurate. Position  $24^\circ 55'$  n preceding.
44.  $\iota$  Libræ, Fl. 24.
- May 24.—Double. Very unequal. L w; S dusky r. Distance  $59'' 4'''$ , not accurate. Position  $22^\circ 31'$  s following.
45. In constellatione Andromedæ.
- July 21.—Double. It is a star near  $\epsilon$  towards  $\alpha$ . L r. Distance about  $1\frac{1}{2}$ .
46.  $\alpha$  Aquilæ, Fl. 53.
- July 23.—Double. Extremely unequal. L w; S d. Dist.  $2' 23'' 18'''$ . Position  $64^\circ 44'$  n preceding.
47. In constellatione Aquilæ, near Fl. 35.
- July 25.—Double. It is one of the preceding stars of a small quartile near  $c$ , not very near.
48. In constellatione Aquilæ, near Fl. 35.
- July 25.—Double. It is also one of the preceding stars of a small quartile near  $c$ , not very near.
49. In constellatione Aquilæ.
- July 26.—Double. The following star of a trapezium near  $l$ .
50. In constellatione Aquilæ.
- July 26.—Double. The following star of a trapezium near  $l$  not near.
51. In monte Mænali Heveliana.
- Aug. 5.—Double. It is a star near the middle. The following of 2, not very near.
52. In constellatione Bootis
- Aug. 17.—Double. It is a star between  $e$  and  $f$ . Distance above  $1'$ . Unequal.
53. In constellatione Bootis.
- Aug. 17.—Double. It is a star more south than  $i$ . Distance above  $1'$ .
54. In constellatione Serpentarii.
- Aug. 21.—Double. It is a star more south than  $o$ . Distance  $75''$ , exact estimation.
55. In constellatione Cassiopeæ, Fl. 2.
- Sept. 6.—Double. It is a star near  $e$ . L r. Distance within  $2\frac{1}{2}$ .
56.  $\theta$  Lyræ, Fl. ultima.
- Sept. 25.—Double. Very unequal. L w; S inclining to r. Distance about  $1\frac{1}{2}$  min. Position—n following.
57. In constellatione Cygni, Fl. 79.
- Oct. 1.—Double. It is the 5th star from  $\epsilon$  to  $\nu$ . Unequal. L w; S pale r. Dist.  $1' 40''$  estimation.
58. In constellatione Aquarii, Fl. 4.
- Oct. 5.—Double. It is the most south of two in the arrow of Antinous. Distance above  $1'$ .
59. In constellatione Cygni, near Fl. 28.
- Oct. 5.—Double. It is a star near  $b$ . Distance  $73''$ , exact estimation.
60. In constellatione Cygni.
- Oct. 8.—Double. It is a star near the second  $c$ . Considerably unequal. L w; S d. Distance  $88''$ , exact estimation.
61. In constellatione Piscium, near Fl. 7.
- Oct. 8.—Treble. It is a star preceding  $b$ . They form a triangle, each side of which is about  $1'$ .
62.  $\alpha$  Piscium, Fl. 8. In ventre.
- Oct. 8.—Double. Distance near  $2'$ .
63. In constellatione Sagittæ.
- Oct. 12.—Double. It is near the star north following  $\epsilon$ . Extremely unequal. L w inclining to r; S d. Distance  $1' 30'' 56'''$ . Position  $4^\circ 9'$  s preceding. A 3d star in the same direction, at a little more than twice the distance. A 4th star in view.
64. In constellatione Eridani.
- Oct. 22.—Double. It is the small star near  $\nu$ . Distance about  $1\frac{3}{4}$ .
65. In capite Monocerotis.



Oct. 22.—Multiple. It is one star with at least 12 around it, all within the field of my telescope.

66.  $\alpha$  Tauri, Fl. 87. Splendida in austrina oculo.

Dec. 19.—Double. Extremely unequal. L r; S d. Distance  $1' 27'' 45'''$ , position  $52^\circ 58'$  n following. With 460, the apparent diameter of this

star, when on the meridian, measured  $1'' 46'''$ , a mean of 2 very complete observations, they agreed to  $6'''$ ; with 932, it measured  $1'' 12'''$ , also a mean of 2 excellent observations; they agreed to  $8'''$ . The apparent disc was perfectly well defined with both powers.

*Postscript to the Catalogue of Double Stars.*—Since having delivered my paper on the parallax of the fixed stars, in which I refer to the above catalogue of double stars, I have received the 4th volume of the *Acta Academiae Theodoro Palatinæ*, which contains a most excellent Memoir of Mr. Mayer's, "*De novis in Cœlo sidereo Phænomenis*;" where I see that the idea of ascertaining the proper motion of the stars by means of small stars that are situated at no great distance from large ones, has induced that gentleman before me to look out for such small stars. In the course of that undertaking he has discovered a good many double stars, of which he has given us a pretty large list, some of them the same with those in my catalogue. My view being the annual parallax, required stars much nearer than those that would do for Mr. Mayer's purpose; therefore I examined the heavens with much higher powers, and looked out chiefly for such as were exceedingly close.

The above catalogue contains 269 double stars, 227 of which, to my present knowledge, have not been noticed by any person. I hope they will prove no inconsiderable addition to the general stock, especially as in that number there are a great many which are out of the reach of Mr. Mayer's and other mural quadrant or transit instruments. It can hardly be expected, that a power of 70 or 80 would be sufficient to discover those curious stars that are contained in the first class of my catalogue; so that it is not strange they should have entirely escaped Mr. Mayer's notice. We see that it is not for want of his looking at those stars; for we find he has frequently observed  $\zeta$  Cancræ, the star near Procyon, and the star in Monoceros, without perceiving the small stars near them, which I have pointed out. Nor is it only in the first class that his telescope wanted power, light, and distinctness; for the small stars that are near  $\beta$  Orionis,  $\beta$  Serpentis,  $\zeta$  Orionis,  $e$  Pegasi,  $\alpha$  Lyræ,  $\alpha$  Andromedæ,  $\mu$  Sagittarii,  $\alpha$  Aquilæ,  $\eta$  Pegasi,  $\delta$  Lyræ,  $\iota$  Libræ,  $\kappa$  Piscium,  $\alpha$  Tauri, and many more, have escaped his discovery, though he has given us the places of other more distant small stars not far from them, and therefore must have had them frequently in the field of view of his telescope. In settling the relative situations of very close double stars, neither Mr. Mayer's instruments, nor his method, were adequate to the purpose. It is well known, that whenever we employ time as a measure, the results cannot be very accurate; because a mistake of no more than a 10th part of a second in time will produce an error of a whole second; and a half in measure, so that his *an* must be extremely defective. Nor could



his micrometer give the declination much better, unless the telescope had borne a power of at least 4 or 500. When the angle of position is but small, such as 3, 4, 5, or 6 degrees, and the distance of the stars not above a few seconds, it is evident that a micrometer must be able to measure 10ths of a second at least, to give even a tolerable exactness of position. On the contrary, the position being measured with such a micrometer as I have constructed for the purpose, we may thence deduce the declination, with great confidence, true to a quarter of a 10th of a second for every second of the distance of the stars.

Mr. Mayer's account of  $\alpha$  Geminorum, for instance, gives a difference of  $0^s.7$  of time in AR, of  $3''.8$  in declination, and of 1 to 6 in magnitude or degree of light of the stars. These quantities reduced to my notation, and compared with my measures of the same star, give

Mr. Mayer's	}	Distance $9''.635$ from centre to centre	Mine	{	$5''.156$ diameters included.
		Position $23^\circ 14'$ n preceding			$32^\circ 47'$ n preceding.
		Magnitude extremely unequal.			A little unequal.

To account for this difference, I ascribe Mr. Mayer's error in distance to his method of measuring by time. The error of position follows always from an observation of the declination taken with the common micrometer, when it is deduced from an erroneous AR. In my measures the distance and position are independent of each other, which I consider as no small advantage of my cross-hair micrometer. The error in the magnitudes of the stars I ascribe to the want of power in Mr. Mayer's telescope, which did not separate the stars far enough for him to judge accurately of their size; otherwise he would soon have found, that instead of 5, there is hardly so much as 1 single degree of difference in their magnitudes. See fig: 6 for a representation of those stars with my power of 460.

I do not mean to depreciate Mr. Mayer's method, the excellence of which is well known; and with some stars of my 3d, all those of the 4th, 5th, and 6th classes, as well as with those still farther distant, to which he has applied it with admirable skill, and "*magno labore, multisque nocturnis vigiliis*," as he very justly expresses himself, a better can hardly be wished for; but with stars of the 2d class which generally differ no more than 1, 2, or 3 tenths of a second of time in AR, and can never differ more than 4 tenths, the insufficiency of measuring by time is obvious. In regard to the declination, it is also no less evident, that it is much more accurate to take an angle, which may be had true to  $2$  or  $3^\circ$  at most, than to measure its tangent, which in stars of the 2d class is generally no more than 2, 3, or  $4''$  of a degree, and can never exceed 5. I do not so much as mention the stars of the 1st class: they must certainly, as to



sense, pass the meridian at the same instant of time. Their distance has even eluded the attacks of my smallest silk-thread micrometer armed with an excellent power of 460; but I shall soon apply my last new instrument to them, not without hopes of success. Now, though I have hitherto not been able to express the distance of the stars of the first class, otherwise than by the proportion it bears to their apparent diameters, I think it a very great point gained, that one of my instruments at least (*viz.* the cross-hair micrometer) has laid hold of them: for their angle of position, I think, is within a very small quantity as well determined as it is in those of the 2d class. This simple but most useful instrument can, by actual measure, discover beyond a doubt a motion in 2 stars that are very close together, though it should amount to no more than a 10th part of a second of a degree, provided that motion be in such a direction that the effect of it be thrown on the angle of position; wherein, with some of the stars of the first class, it would occasion an alteration of 10, 20, 30, or more degrees.

I have marked all those stars in my catalogue which have been observed by Mr. Mayer, and other astronomers, with an asterisk (\*) affixed to the number, that they may be known; those with the mark of a dagger (†) have been observed by different astronomers before Mr. Mayer. Among the stars which are not marked, will be found several that have been observed by Mr. Mayer; but, on comparing them together, it will be seen, that they are observations of different small stars; for instance, Mr. Mayer (*Act. Acad.* vol. 4, p. 296) observed a small star near Rigel at the distance of  $1^m 0^s.5$  AR in time, and  $2' 55''.2$  in difference of declination north preceding Rigel. In my 2d class (the 34th star) we also find Rigel; but the small star I have observed is one which has not been seen by Mr. Mayer, and is at a distance of no more than  $6'' 27'''$ . Position  $68^\circ 12'$  south preceding; and so on with other stars.

I have used the expression double-star in a few instances of the 6th class in rather an extended signification: the example of Flamsteed, however, will sufficiently authorize my application of the term. I preferred that expression to any other, such as comes, companion, or satellite; because, in my opinion, it is much too soon to form any theories of small stars revolving round large ones, and therefore I thought it adviseable carefully to avoid any expression that might convey that idea. I am very well persuaded that Flamsteed, who first used the word comes, meant it only in a figurative sense. I shall not fail to take the first opportunity of looking out for those of Mr. Mayer's double-stars which I have not in my catalogue, amounting to 31; and also for one I find mentioned in *La Connoissance des Temps* for 1783, discovered by Mr. Messier.



*XIII. Description of a Lamp-Micrometer, and the Method of using it. By Mr. William Herschel, F. R. S. p. 163.*

The great difficulty of measuring very small angles, such as hardly amount to a few seconds, is well known to astronomers. Since I have been engaged in observations on double stars, I have had so much occasion for micrometers that would measure exceeding small distances exactly, that I have continually been endeavouring to improve these instruments.

The natural imperfections of the parallel wire micrometer, in taking the distance of very close double stars, are the following. When 2 stars are taken between the parallels, the diameters must be included. I have in vain attempted to find lines sufficiently thin to extend them across the centres of the stars, so that their thickness might be neglected. The single threads of the silk-worm, with such lenses as I use, are so much magnified, that their diameter is more than that of many of the stars. Besides, if they were much less than they are, the power of deflection of light would make the attempt to measure the distance of the centres this way fruitless: for I have always found the light of the stars to play upon those lines, and separate their apparent diameters into 2 parts. Now since the spurious diameters of the stars thus included, to my certain knowledge, are continually changing according to the state of the air, and the length of time we look at them, we are, in some respect, left at an uncertainty, and our measures taken at different times, and with different degrees of attention, will vary on that account. Nor can we come at the true distance of the centres of any 2 stars, one from another, unless we could tell what to allow for the semi-diameters of the stars themselves; for different stars have different apparent diameters, which, with a power of 227, may differ from each other, as I have experienced, as far as 2 seconds. The next imperfection, is that which arises from a deflection of light on the wires when they approach very near to each other; for if this be owing to a power of repulsion lodged at the surface, it is easy to understand that such powers must interfere with each other, and give the measures larger in proportion than they would have been, if the repulsive power of one wire had not been opposed by a contrary power of the other. Another very considerable imperfection of these micrometers is a continual uncertainty of the real zero. I have found, that the least alteration in the situation and quantity of light will affect the zero, and that a change in the position of the wires, when the light and other circumstances remain unaltered, will also produce a difference. To obviate this difficulty, whenever I took a measure that required the utmost accuracy, my zero was always taken immediately after, while the apparatus remained in the same situation it was in when the measure was taken; but this enhances the difficulty, because it introduces an additional observation. The next imperfection, which is none of the smallest, is that



every micrometer hitherto used requires either a screw, or a divided bar and pinion, to measure the distance of the wires or divided image. Those who are acquainted with works of this kind are but too sensible how difficult it is to have screws that shall be perfectly equal in every thread or revolution of each thread; or pinions and bars that shall be so evenly divided as perfectly to be depended on, in every leaf and tooth, to perhaps the 2, 3, or 4 thousandth part of an inch; and yet, on account of the small scale of those micrometers, these quantities are of the greatest consequence; an error of a single thousandth part inducing in most instruments a mistake of several seconds. The last and greatest imperfection of all is, that these wire micrometers require a pretty strong light in the field of view: and when I had double stars to measure, one of which was very obscure, I was obliged to be content with less light than is necessary to make the wires perfectly distinct; and several stars on this account could not be measured at all, though otherwise not too close for the micrometer.

The instrument I am going to describe, which I call a lamp-micrometer, is free from all these defects, and has also to recommend it, the advantage of a very enlarged scale. The construction of it is as follows.

ABGCFE (fig. 1, pl. 5,) is a stand 9 feet high, on which a semi-circular board qhogg is moveable upward or downward, in the manner of some fire-screens, as occasion may require, and is held in its situation by a peg p put into any one of the holes of the upright piece AB. This board is a segment of a circle of 14 inches radius, and is about 3 inches broader than a semi-circle, to give room for the handles rd, ep, to work. The use of this board is to carry an arm L, 30 inches long, made to move on a pivot at the centre of the circle, by means of a string, which passes in a groove on the edge of the semi-circle pgohq; the string is fastened to a hook at o (not expressed in the figure being at the back of the arm L,) and, passing along the groove from oh to q, is turned over a pulley at q, and goes down to a small barrel e, within the plane of the circular board, where a double-jointed handle ep commands its motion. By this contrivance we see the arm L may be lifted up to any altitude from the horizontal position to the perpendicular, or be suffered to descend by its own weight below the horizontal to the reverse perpendicular situation. The weight of the handle p is sufficient to keep the arm in any given position; but if the motion should be too easy, a friction spring applied to the barrel will moderate it at pleasure.

In front of the arm L a small slider, about 3 inches long, is moveable in a rabbet from the end L towards the centre, backward and forward. A string is fastened to the left side of the little slider, and goes towards L, where it passes round a pulley at m, and returns under the arm from mn, towards the centre, where it is led in a groove on the edge of the arm, which is of a circular form, upward to a barrel (raised above the plane of the circular board) at r, to which



the handle *rd* is fastened. A second string is fastened to the slider, at the right side, and goes towards the centre, where it passes over a pulley *n*, and the weight *w*, which is suspended by the end of this string, returns the slider towards the centre, when a contrary turn of the handle permits it to act.

*a* and *b* are 2 small lamps, 2 inches high,  $1\frac{1}{2}$  in breadth, by  $1\frac{1}{4}$  in depth. The sides, back, and top, are made so as to permit no light to be seen, and the front consists of a thin brass sliding door. The flame in the lamp *a* is placed  $\frac{3}{16}$  of an inch from the left side,  $\frac{3}{16}$  from the front, and half an inch from the bottom. In the lamp *b* it is placed at the same height and distance measuring from the right side. The wick of the flame consists only of a single very thin lamp-cotton thread; for the smallest flame being sufficient, it is easier to keep it burning in so confined a place. In the top of each lamp must be a little slit, lengthways, and also a small opening in one side near the upper part, to permit air enough to circulate to feed the flame. To prevent every reflection of light, the side opening of the lamp *a* should be to the right, and that of the lamp *b* to the left. In the sliding door of each lamp is made a small hole with the point of a very fine needle just opposite the place where the wicks are burning, so that when the sliders are shut down, and every thing dark, nothing shall be seen but two fine lucid points of the size of 2 stars of the 3d or 4th magnitude. The lamp *a* is placed so, that its lucid point may be in the centre of the circular board where it remains fixed. The lamp *b* is hung to the little slider which moves in the rabbet of the arm, so that its lucid point, in a horizontal position of the arm, may be on a level with the lucid point in the centre. The moveable lamp is suspended on a piece of brass fastened to the slider by a pin exactly behind the flame on which it moves as a pivot. The lamp is balanced at the bottom by a leaden weight, so as always to remain upright, when the arm is either lifted above, or depressed below, the horizontal position. The double-jointed handles *rd*, *ep*, consist of light deal rods, 10 feet long, and the lowest of them may have divisions, marked on it near the end *p*, expressing exactly the distance from the central lucid point, in feet, inches, and tenths.

From this construction we see, that a person at a distance of 10 feet may govern the 2 lucid points, so as to bring them into any required position south or north preceding or following, from 0 to 90°, by using the handle *p*, and also to any distance from  $\frac{6}{16}$  of an inch to 5 or 6 and 20 inches, by means of the handle *d*. If any reflection or appearance of light should be left from the top or sides of the lamps, a temporary screen, consisting of a long piece of paste-board, or a wire frame covered with black cloth, of the length of the whole arm, and of any required breadth, with a slit of half an inch broad in the middle, may be affixed to the arm by 4 bent wires, projecting an inch or 2 before the



lamps, situated so that the moveable lucid point may pass along the opening left for that purpose.

Fig. 2 represents part of the arm *L*, of a larger size; *s* the slider; *m* the pulley, over which the cord *xtyz* is returned towards the centre; *v* the other cord going to the pulley *n* of fig. 1; *r* the brass piece moveable on the pin *c*, to keep the lamp upright. At *r* is a wire rivetted to the brass piece, on which is held the lamp by a nut and screw. Fig. 3, 4, represent the lamps *a*, *b*, with the sliding doors open, to show the situation of the wicks. *w* is the leaden weight, with a hole *d* in it, through which the wire *r* of fig. 2 is to be passed, when the lamp is to be fastened to the slider *s*. Fig. 5 represents the lamp *a* with the sliding door shut; *l* the lucid point; and *ik* the openings at the top, and *s* at the sides for the admission of air.

Every ingenious artist will soon perceive that the motions of this micrometer are capable of great improvement by the application of wheels and pinions, and other well known mechanical resources; but, as the principal object is only to be able to adjust the 2 lucid points to the required position and distance, and to keep them there for a few minutes, while the observer goes to measure their distance, it will not be necessary to say more on the subject.

I am now to show the application of this instrument. It is well-known to opticians, and others, who have been in the habit of using optical instruments, that we can with one eye look into a microscope or telescope, and see an object much magnified, while the naked eye may see a scale on which the magnified picture is thrown. In this manner I have generally determined the power of my telescopes; and any one who has acquired a facility of taking such observations will very seldom mistake so much as 1 in 50 in determining the power of an instrument, and that degree of exactness is fully sufficient for the purpose.

The Newtonian form is admirably adapted to the use of this micrometer; for the observer stands always erect, and looks in a horizontal direction, though the telescope should be elevated to the zenith. Besides, his face being turned away from the object to which his telescope is directed, this micrometer may be placed very conveniently, without causing the least obstruction to the view: therefore, when I use this instrument, I put it at 10 feet distance from the left eye, in a line perpendicular to the tube of the telescope, and raise the moveable board to such a height, that the lucid point of the central lamp may be on a level with the eye. The handles, lifted up, are passed through 2 loops fastened to the tube, just by the observer, so as to be ready for his use. I should observe, that the end of the tube is cut away, so as to leave the left-eye entirely free to see the whole micrometer.

Having now directed the telescope to a double star, I view it with the right eye, and at the same time with the left see it projected on the micrometer: then,



by the handle *p*, which commands the position of the arm, I raise or depress it so as to bring the 2 lucid points to a similar situation with the 2 stars; and, by the handle *d*, I approach or remove the moveable lucid point to the same distance of the 2 stars, so that the 2 lucid points may be exactly covered by, or coincide with the stars. A little practice in this business soon makes it easy, especially to one who has already been used to look with both eyes open.

What remains to be done is very simple. With a proper rule, divided into inches and 40th parts, I take the distance of the lucid points, which may be done to the greatest nicety, because, as observed before, the little holes are made with the point of a very fine needle. The measure thus obtained is the tangent of the magnified angle under which the stars are seen, to a radius of 10 feet; therefore, the angle being found, and divided by the power of the telescope, gives the real angular distance of the centres of a double star. For instance, Sept. 25, 1781, I measured  $\alpha$  Herculis with this instrument. Having caused the 2 lucid points to coincide exactly with the stars centre on centre, I found the radius, or distance of the central lamp from the eye, 10 feet 4.15 inches; the tangent or distance of the 2 lucid points 50.6 fortieth parts of an inch; this gives the magnified angle  $35'$ , and dividing by the power 460, which I used, we obtain  $4''\ 34'''$  for the distance of the centres of the 2 stars. The scale of the micrometer at this very convenient distance, with the power of 460 (which my telescope bears so well on the fixed stars that for near a twelvemonth past I have hardly used any other) is above a quarter of an inch to a second; and by putting on my power of 932, which in very fine evenings is extremely distinct, I obtain a scale of more than half an inch to a second, without increasing the distance of the micrometer; whereas the most perfect of my former micrometers, with the same instrument, had a scale of less than the 2000th part of an inch to a second.

The measures of this micrometer are not confined to double stars only, but may be applied to any other objects that require the utmost accuracy, such as the diameters of the planets or their satellites, the mountains of the moon, the diameters of the fixed stars, &c. For instance, Oct. 22, 1781, I measured the apparent diameter of  $\alpha$  Lyræ; and judging it of the greatest importance to increase my scale as much as convenient, I placed the micrometer at the greatest convenient distance, and (with some trouble, for want of longer handles, which might easily be added) took the diameter of this star by removing the 2 lucid points to such a distance as just to inclose the apparent diameter. When I measured my radius, it was found to be 22 feet 6 inches. The distance of the 2 lucid points was about 3 inches; for I will not pretend to extreme nicety in this observation, on account of the very great power I used, which was 6450. From these measures we have the magnified angle  $38'\ 10''$ : this divided by the



power gives  $0''.355$  for the apparent diameter of  $\alpha$  Lyræ. The scale of the micrometer, on this occasion, was no less than  $8.443$  inches to a second, as will be found by multiplying the natural tangent of a second with the power and radius in inches. Nov. 1781, I measured the diameter of the new star; but the air was not very favourable, for this singular star was not so distinct with 227 that evening as it generally is with 460: therefore, without laying much stress on the exactness of the observation, I shall only report it, to exemplify the use of the micrometer. My radius was 35 feet 11 inches. The diameter of the star, by the distance of the lucid points, was 2.4 inches, and the power I used 227: hence the magnified angle is found  $19'$ , and the real diameter of the star  $5''.022$ . The scale of this measure .474 millesimals of an inch, or almost half an inch to a second.

*XIV. A Paper to obviate some Doubts concerning the Great Magnifying Powers used. By Mr. Herschel, F. R. S. Addressed to Sir Joseph Banks. p. 173.*

SIR, I have the honour of laying before you the result of a set of measures I have taken in order to ascertain once more the powers of my Newtonian 7-feet reflector. The method I have formerly used, and which I still prefer to that which I have now been obliged to practise, requires very fine weather and a strong sun-shiny day; but my impatience to answer the requests of Sir Joseph Banks would not permit me to wait for so precarious an opportunity at this season of the year. The difference in all the powers, as far as 2010, will be found to be in favour of those I have mentioned; and, I believe, a much greater concurrence could not well be expected, where different methods of ascertaining them are used. The variation in the 2 highest powers is more considerable than I was aware of; but still may easily be shown to be a necessary consequence of the difference in the methods. However, if on comparing together the methods, it should be thought that the power 5786 is nearer the truth than 6450, I shall readily join to correct that number. The manner in which I have now determined the powers is as follows: I took one of the eye lenses which magnifies least, and measured its solar focus by the sun's rays as exactly as I could 5 times, which proved to be 1.01, 1.04, 1.09, 1.01, 1.05, in half-inch measure, a mean of which is 1.04. The sidereal focus of my 7-feet speculum is 170.4 in the same measure. Thence, dividing 170.4 by 1.04, we find that the telescope will magnify 163.8 times when that lens is used. This power being found, I applied the same lens as a single microscope to view with it a certain object, which was a drawn brass wire fastened so as not to turn on its axis or change its position; for these wires are seldom perfectly round, or of an even size, and it is therefore necessary to use this caution to prevent errors: then, with a fine pair of compasses, I took 4 independent measures of the image of the brass wire,



which was thrown on a sheet of paper exactly  $8\frac{1}{2}$  inches from the lens, the eye being always as close to the lens as possible. I viewed the same wire, exactly in the same manner, with every one of the lenses, and measured the pictures on the paper. When I came to the higher powers, the wire was exchanged for another, 4.37 times thinner than the former, as determined by comparing the proportion of their images 54 to  $235\frac{3}{4}$ , taken by the same lens.

When the images of these wires are obtained, the power of the telescope, with every one of the lenses, becomes known by one plain analogy: viz. as the image of the wire by the first lens ( $77\frac{3}{4}$ ) is to the power it gives to the telescope (163.8,) so is the image of the wire by the 2d lens (119,) to the power it will give to the same telescope (250.7.) The particulars of all the measures are as follow:

Powers as they have been called in my papers.	Images of a wire thrown on a paper in hundredths of half inches.	A mean of the 4 measures.	Powers as they come out by this method.
146 .....	77.. 78 .. 78 .. 78 .....	$77\frac{3}{4}$ .....	$163.86 = \frac{170.4}{1.04}$
227 .....	119.. 119 .. 119 .. 119 .....	119 .....	250.7
278 .....	143.. 143 .. 144 .. 143 .....	$143\frac{1}{4}$ .....	301.8
460 { .....	236.. 236 .. 235 .. 236 .....	$235\frac{3}{4}$ .....	496.7
Smaller wire.			
460 { .....	53.. 54 .. 55 .. 54 .....	54 .....	
754 .....	83.. 85 .. 84 .. 85 .....	$84\frac{1}{4}$ .....	775.1
932 .....	107.. 107 .. 107 .. 108 .....	$107\frac{1}{2}$ .....	986.7
1159 .....	128.. 128 .. 129 .. 128 .....	$128\frac{1}{4}$ .....	1179.9
1536 .....	An excellent lens, lost about 8 months before.		
2010 .....	236.. 236 .. 238 .. 236 .....	$236\frac{1}{2}$ .....	2175.8
3168 .....	281.. 283 .. 281 .. 280 .....	$281\frac{1}{4}$ .....	2585.5
6450 .....	635.. 625 .. 630 .. 626 .....	629 .....	5786.8

I beg leave, Sir, now to give a short description of the method I have formerly used to determine these powers. In the year 1776 I erected a mark of white paper, exactly half an inch in diameter, which I viewed with my telescope at the greatest convenient distance with one of the least magnifiers. An assistant was placed at right angles in a field, at the same distance from my eye as the object from the great speculum of the telescope. On a pole erected there I viewed the magnified image of the half inch, and the assistant marked it by my direction; this being measured, gave the power of the instrument at once. The power thus obtained was corrected by theory, to reduce it to what it would be on infinitely distant objects. The powers of the rest of the lenses I deduced from this, by a camera eye-piece, which I made for that purpose. ABCD (fig. 17, pl. 3) represents a perpendicular section of it. The end A screws into the telescope. On the end B may be screwed any of the common single lens eye-pieces. Imn is a small oval plane speculum, adjusted to an angle of  $45^\circ$  by 3 screws, 2 of which appear at o, p. When the observer looks in at B, he may see the object projected on a sheet of paper on a table placed under the camera piece, and mea-



sure its picture *ab*, as in fig. 18. The power of one lens therefore being known, that of the rest was also found by comparing the measures of the projected images.

It may not be amiss to mention some of the advantages and inconveniencies attending each of these methods. When we take the focus of an eye-lens, which the first method requires, we are liable to a pretty considerable uncertainty, and in very small lenses it is not to be done at all. Also, in calculating the power by that focus, no account is made of the aberration which takes place in all specula and lenses, and increases the image, so that we rather find out how much the telescope should magnify, than how much it really does magnify; but in determining the power by an experiment we avoid these difficulties. On the other hand, when the power is very great, the latter method becomes inconvenient, both on account of want of light in the object, and a very considerable aberration which takes place, and makes the picture too indistinct to be very accurate in the measure, and of course larger than it ought to be; and this will account for the excess in the measures of my 2 largest powers. However, when I employed 6450 on the diameter of  $\alpha$  Lyræ, I incline to think the method I had used when I determined that power, ought to be preferred, because my lamp-micrometer gives the measure of an object as it appears in the telescope, and therefore this aberration is included, and should be taken into consideration.

To prevent any mistakes, I wish to mention again, that I have all along proceeded experimentally in the use of my powers, and that I do not mean to say I have used 6450, or 5786, on the planets, or even on double stars; every power I have mentioned is to be understood as having been used just as it is related; but further inferences ought not as yet to be drawn. For instance, my observations on  $\epsilon$  Bootis mention that I have viewed that star with 2010, or as in the above table with 2175, extremely distinct; but on several other celestial objects I have found this power of no service. Many plausible suggestions have already occurred to account for these appearances; but I wait till further experiments shall have furnished me with more materials to reason on. The use of high powers is a new and untrodden path, and in this attempt variety of new phenomena may be expected; I therefore wish not to be in a haste to make general conclusions. I shall not fail to pursue this subject, and hope soon to be able to attack the celestial bodies with a still stronger armament, which is now preparing.

*XIV. Continuation of the Experiments and Observations on the Specific Gravities and Attractive Powers of various Saline Substances. By Rich. Kirwan, Esq., F. R. S. p. 179.*

Before entering (says Mr. K.) on a detail of the new experiments I have made in the prosecution of this subject, I must beg leave to rectify some mistakes I have fallen into in my last paper.



1. In computing the quantity of acid taken up by 10.5 gr. of mild vegetable fixed alkali, I made no allowance for the small quantity of earth it contains, viz. 0.7035 of a grain; but in large quantities of alkali, this proportion is considerable, and it occasioned a small but sensible error in my subsequent calculations of the proportion of ingredients in neutral salts, the quantity of alkali being, by that fraction, less than I supposed it in 10.5 gr. This correction being made, it will be found, that 100 gr. of perfectly dry vegetable fixed alkali, abstracted from the quantity of earth, generally contain 22.457 gr. of fixed air, instead of 21, as before determined: though the former determination is right, where the earth is not separated, yet may well be supposed to exist, as in the alkali of pearl-ash, purified by 3 repeated calcinations and solutions. Hence also 100 gr. of such alkali, free from earth, water, and fixed air, take up 46.77 gr. of the mineral acids, that is, of the mere acid part; and 100 gr. of common mild vegetable alkali take up about 36.23 of real acid.

Now, 100 gr. of perfectly dry tartar vitriolate contain 30.21 of real acid, 64.61 of fixed alkali, and 5.18 of water. Crystallized tartar vitriolate loses only 1 per cent. of water in a heat in which its acid also is not separated in any degree, and therefore contains 6.18 of water.

Again, 100 gr. of nitre, perfectly dried, contain 30.86 of acid, 66 of alkali, and 3.14 of water; but in crystallized nitre the proportion of water is somewhat greater; for 100 gr. of these crystals, being exposed to a heat of  $180^{\circ}$  for 2 hours, lost 3 gr. of their weight, without exhaling any acid smell; but when exposed to a heat of  $200^{\circ}$ , the smell of the nitrous acid is distinctly perceived. Hence 100 gr. of crystallized nitre contain 29.89 of mere acid, 63.97 of alkali, and 6.14 of water.

And 100 gr. of digestive salt perfectly dry contain 29.68 of marine acid, 68.47 of alkali, and 6.85 of water. 100 gr. of crystallized digestive salt lost but 1 gr. of their weight before the smell of the marine acid is perceived; and hence they contain 7.85 gr. of water.

But the mistake which cost me most time and pains to correct, was that fallen into when I imagined, that the mixtures of oil of vitriol and water, and spirit of nitre and water, had attained their maximum of density when they had cooled to the temperature of the atmosphere, which at the time of the experiments stood between  $50$  and  $60^{\circ}$  of Fahrenheit. The former I had even suffered to stand 6 hours, which was much longer than was necessary for its cooling; but when the acid was so much diluted as to cause little or no heat, I allowed it to stand but for a very little time before I examined its density: yet several months after I found many of these mixtures much denser than when I first examined them, and that at least 12 hours rest was requisite before concentrated oil of vitriol, to which even twice its weight of water is added, attains its utmost density, and still more when a less proportion of water is used: thus, when I made the mixture



of 2519.75 gr. of oil of vitriol, whose specific gravity was 1.819, with 180 of water, I found its density 6 hours after 1.771; but after 24 hours it was 1.798; and hence, according to the reasoning in the former paper, the accrued density was at least .064, instead of .045, as I had formerly found it. But by using oil of vitriol still more concentrated, whose specific gravity was 1.8846, I was enabled, by a similar train of reasoning, to make a still nearer approximation, and found that the accrued density of oil of vitriol, whose specific gravity is 1.819, amounts to 0.104; and consequently its mathematical specific gravity is 1.715. Now 6.5 gr. of this oil of vitriol contained, as I before found, 3.55 of mere acid, and the remainder water, and the weight of an equal bulk of water is 3.79 gr.; then subtracting from this the weight of the water that enters into the composition of the oil of vitriol, it will be found, that the weight of a bulk of water, equal to the acid part, is 0.84, and consequently the specific gravity of the pure and mere acid part is 4.226. On this ground, and constantly allowing the mixtures to rest at least 12 hours, till the oil of vitriol was diluted with 4 times its weight of water, and then often only 6 hours, before their density was examined, I constructed the table hereto annexed; the temperature of the room being constantly kept between 50 and 60°; and the column of acid being always 612.05.

Oil of vitriol.	Water.	Accrued density.	Mathem. sp. grav.	Physical spec. grav.	Oil of vitriol.	Water.	Accrued density.	Mathem. spec. grav.	Physical spec. grav.
Grains.					Grains.				
1000	387.95	.07	1.877	1.886	4100	3487.95	.070	1.128	1.198
1100	487.95	.104	1.738	1.844	4200	3587.95	.070	1.125	1.195
1200	587.95	.105	1.637	1.742	4300	3687.95	.070	1.121	1.191
1300	687.95	.144	1.561	1.705	4400	3787.95	.070	1.118	1.188
1400	787.95	.144	1.500	1.644	4500	3887.95	.070	1.115	1.185
1500	887.95	.137	1.452	1.589	4600	3987.95	.070	1.113	1.183
1600	987.95	.137	1.412	1.539	4700	4087.95	.070	1.110	1.180
1700	1087.95	.130	1.379	1.509	4800	4187.95	.070	1.107	1.177
1800	1187.95	.124	1.350	1.474	4900	4287.95	.070	1.105	1.175
1900	1287.95	.116	1.326	1.442	5000	4387.95	.070	1.103	1.172
2000	1387.95	.116	1.304	1.420	5100	4487.95	.069	1.100	1.169
2100	1487.95	.112	1.286	1.398	5200	4587.95	.069	1.098	1.167
2200	1587.95	.112	1.269	1.381	5300	4687.95	.069	1.096	1.165
2300	1687.95	.108	1.254	1.362	5400	4787.95	.069	1.094	1.163
2400	1787.95	.104	1.241	1.345	5500	4887.95	.068	1.092	1.160
2500	1887.95	.104	1.229	1.333	5600	4987.95	.067	1.091	1.158
2600	1987.95	.101	1.219	1.320	5700	5087.95	.067	1.089	1.156
2700	2087.95	.096	1.209	1.307	5800	5187.95	.067	1.087	1.154
2800	2187.95	.091	1.200	1.291	5900	5287.95	.065	1.086	1.151
2900	2287.95	.090	1.192	1.282	6000	5387.95	.064	1.084	1.148
3000	2387.95	.090	1.184	1.274	6100	5487.95	.064	1.082	1.146
3100	2487.95	.090	1.177	1.267	6200	5587.95	.063	1.081	1.144
3200	2587.95	.090	1.170	1.260	6300	5687.95	.062	1.080	1.142
3300	2687.95	.089	1.164	1.253	6400	5787.95	.062	1.078	1.140
3400	2787.95	.084	1.159	1.243	6500	5887.95	.061	1.077	1.138
3500	2887.95	.083	1.150	1.233	6600	5987.95	.060	1.076	1.136
3600	2987.95	.073	1.149	1.222	6700	6087.95	.060	1.074	1.134
3700	3087.95	.073	1.144	1.217	6800	6187.95	.060	1.072	1.132
3800	3187.95	.071	1.140	1.211	6900	6287.95	.060	1.070	1.130
3900	3287.95	.071	1.136	1.208	7000	6387.95	.059	1.069	1.128
4000	3387.95	.071	1.132	1.204					



With regard to the nitrous acid, I found also I had been a little too precipitate as to the time of examining its density, after it had been mixed with water. Hence using some whose specific gravity was 1.474, I allowed the mixtures to rest 12 hours, till it was diluted with twice its weight of water, and the subsequent mixtures 6 hours at least; by the former process of reasoning, I found the specific gravity of the mere nitrous acid to be 5.530; the constant number in the column of acid being here 393.

Spirit of nitre.	Water.	Accrued density.	Mathem. spec. grav.	Physical spec. grav.	Spirit of nitre.	Water.	Accrued density.	Mathem. spec. grav.	Physical spec. grav.
Grains.					Grains.				
900	507	....	1.557	1.557	3200	2807	.054	1.111	1.165
1000	607	....	1.474	1.474	3300	2907	.053	1.108	1.161
1100	707	.035	1.413	1.448	3400	3007	.052	1.104	1.156
1200	807	.056	1.367	1.423	3500	3107	.050	1.101	1.151
1300	907	.065	1.329	1.394	3600	3207	.048	1.098	1.146
1400	1007	.065	1.298	1.363	3700	3307	.047	1.095	1.142
1500	1107	.077	1.273	1.350	3800	3407	.045	1.092	1.137
1600	1207	.082	1.251	1.333	3900	3507	.043	1.089	1.132
1700	1307	.082	1.233	1.315	4000	3607	.040	1.087	1.127
1800	1407	.083	1.217	1.300	4100	3707	.037	1.085	1.122
1900	1507	.083	1.204	1.287	4200	3807	.035	1.083	1.118
2000	1607	.096	1.191	1.269	4300	3907	.034	1.080	1.114
2100	1707	.088	1.181	1.254	4400	4007	.032	1.078	1.110
2200	1807	.071	1.176	1.247	4500	4107	.029	1.077	1.106
2300	1907	.068	1.162	1.230	4600	4207	.027	1.075	1.102
2400	2007	.068	1.154	1.222	4700	4307	.025	1.073	1.098
2500	2107	.067	1.147	1.214	4800	4407	.022	1.072	1.094
2600	2207	.065	1.141	1.206	4900	4507	.020	1.070	1.090
2700	2307	.063	1.135	1.198	5000	4607	.018	1.068	1.086
2800	2407	.061	1.129	1.190	5100	4707	.015	1.067	1.082
2900	2507	.058	1.124	1.182	5200	4807	.012	1.066	1.078
3000	2607	.055	1.120	1.175	5300	4907	.008	1.066	1.074
3100	2707	.054	1.116	1.170					

The foregoing experiments were made at the temperature of between 50 and 60° of Fahrenheit; but as it may be suspected that the density of the above acids is much altered at degrees of temperature considerably different, I endeavoured to find the quantity of this alteration, and to calculate what this density would be at 55°, that the quantities of acid and water may thence be investigated. To this end I took some dephlogisticated spirit of nitre, and examined its specific gravity at different degrees of heat, and found it as annexed, viz. at

Deg.	Sp. gravity.
30 .....	1.4650
46 .....	1.4587
86 .....	1.4302
120 .....	1.4123

Therefore the total expansion of this spirit of nitre from 30 to 120°, that is, by 90° of heat, was 0.0527; for  $1.4650 - 1.4123 = .0527$ ; by which we see that the dilatations are nearly proportional to the degrees of heat; for beginning with the first dilatation from 30 to 46°, that is, by 16° of heat,  $90 : 0.0527 :: 16$



: 0.0093; but in reality these  $16^{\circ}$  of heat afforded a dilatation equal only to 0.0063; for  $1.4650 - 1.4587 = 0.0063$ ; so that the difference between the calculated and observed dilatations is only  $\frac{30}{100000}$ , a difference of no consequence in the present case; and even that might arise from the immersion of the cold glass ball filled with mercury in the liquor, it being the solid I use to try the specific gravity of liquids. In the next case the difference is still less; for  $90 : 0.0527 :: 56 : 0.0327$ ; but  $56^{\circ}$  of heat produced in reality a dilatation of 0.0348, for  $1.4650 - 1.4302 = 0.0348$ , so that the calculation is deficient only by  $\frac{21}{100000}$ .

I afterwards tried another, and somewhat stronger, spirit of nitre, whose specific gravity was, at

Deg.	Sp. gravity.
34 .....	1.4750
49 .....	1.4653
150 .....	1.3792

Here also the expansions are nearly proportional to the degrees of heat; for  $116^{\circ}$  of heat (the difference between 34 and 150) produce an expansion of 0.0958; and  $15^{\circ}$  of heat (the difference between 34 and 49) produce an expansion of 0.0097; and by calculation 0.0123, which last differs from the truth only by  $\frac{26}{100000}$ . By this experiment we see, that the stronger the spirit of nitre is, the more it is expanded by the same degree of heat: for if the spirit of nitre of the last experiment were expanded in the same proportion as in the first, its dilatation by  $116^{\circ}$  of heat should be 0.0679, whereas it was found to be 0.0958.

As the dilatation of spirit of nitre is far greater than that of water by the same degree of heat, and as it consists only of acid and water, it clearly follows, that its superior dilatibility must be owing to the acid part; and hence the more acid is contained in a given quantity of spirit of nitre, the greater is its dilatibility. We might therefore suppose, that the dilatation of spirit of nitre was intermediate between that of the quantity of water it contains and that of its quantity of acid; but there exists another power also which prevents this simple result, namely, the mutual attraction of the acid and water to each other, which makes them occupy a less space than the sum of their joint volumes, which condensation I have therefore called their accrued density. Taking this into the account, we may consider the dilatation of spirit of nitre as equal to those of the quantities of water and acid it contains, minus the condensation they acquire from their mutual attraction, and this rule holds as to all other heterogeneous compounds.

To find the quantities of acid and water in spirit of nitre, whose specific gravity was found in degrees of temperature different from those for which the table was constructed, viz.  $54$ ,  $55$ , or  $56^{\circ}$  of Fahrenheit, the surest method is to find how much that spirit of nitre is expanded or condensed by a greater or less degree of heat, and then, by the rule of proportion, find what its density would be at  $55^{\circ}$ ; but if this cannot be done, we shall approach pretty near the truth, if we



allow  $\frac{1}{1000}$  for every  $15^\circ$  of heat above or below  $55^\circ$  of Fahrenheit, when the specific gravity of spirit. is between 1.400 and 1.500; and  $\frac{2}{1000}$  when the specific gravity is between 1.400 and 1.300.

As to oil and spirit of vitriol, I found the dilatations exceedingly irregular; probably by reason of a white foreign matter, which is more or less suspended or dissolved in it, according to its greater or less dilution. This matter I would not separate, as I intended trying the density of this substance in the state in which it is commonly used. In general I found, that  $15^\circ$  of heat cause a difference of about  $\frac{2}{1000}$  in its specific gravity when it exceeds 1.800; and of  $\frac{3}{1000}$  when its specific gravity is between 1.400 and 1.300: its dilatation is greater than that of water, and so much greater as it is stronger.

The dilatations of spirit of salt are very nearly proportional to the degrees of heat, as appears by the annexed table. Hence  $\frac{6}{1000}$  should be added or subtracted for every  $21^\circ$  above or below

Deg.	Sp. gravity.
33 .....	1.1916
54 .....	1.1860
66 .....	1.1820
128 .....	1.1631

$55^\circ$ , in order to reduce it to  $55^\circ$ , the degree for which its proportion of acid and water was calculated. The dilatability of this acid is much greater than that of water, and even than that of the nitrous acid of the same density.

I now proceed to examine the quantity of pure acids taken up at the point of saturation by the various substances they unite with.

*Of the mineral alkali.*—That which I made use of was procured from Mr. Turner, who by a peculiar and ingenious process extracts it in the greatest purity from common salt. Of this alkali I rendered a portion tolerably caustic in the usual manner, and evaporating 1 oz. of the caustic solution to perfect dryness, I found it to contain 20.25 gr. of solid matter. I was assured that the watery part alone exhaled during the evaporation, as the quantity of fixed air contained in it was very small, and to dissipate this, a much greater heat would be requisite than that which I used. This dry alkali I immediately dissolved in twice its weight of water, and saturating it with dilute vitriolic acid, found it to contain 2.25 gr. of fixed air, that being the weight which the saturated solution wanted of being equal to the joint weights of the water, alkali, and spirit of vitriol employed.

The quantity of mere vitriolic acid necessary to saturate 100 gr. of pure mineral alkali, I found to be 60 or 61 gr.; the saturated solution, thus formed, being evaporated to perfect dryness, weighed 36.5 gr. but of this weight, only 28.38 were alkali and acid; therefore the remainder, viz. 8.12 gr. were water. Hence 100 gr. of Glauber's salt, perfectly dried, contain 29.12 of mere vitriolic acid, 48.6 of mere alkali, and 22.28 of water; but Glauber's salt crystallized contains a much larger proportion of water; for 100 gr. of these crystals, being heated red-hot, lost 55 gr. of their weight. This loss I suppose to arise merely



from the evaporation of the watery part, and the remaining 45 contained alkali, water, and acid, in the same proportion as the 100 gr. of Glauber's salt, perfectly dried, abovementioned; then these 45 contained 13.19 gr. of vitriolic acid, 21.87 of fixed alkali, and 9.94 of water; consequently 100 gr. of crystallized Glauber's salt contain 13.19 of vitriolic acid, 21.87 of alkali, and 64.94 of water.

I also saturated this alkali with the dephlogisticated nitrous acid, and found that 100 gr. of the alkali took up 57 of the mere nitrous acid in the experiment I most depended on; but this quantity varied in some experiments a few grains, being sometimes 60, and sometimes 63 gr.; so that I conclude the proportion of this acid, taken up by the alkali, is nearly the same as that of the vitriolic acid. Supposing this quantity to be 57 gr. then 100 gr. cubic nitre, perfectly dry, contain 30 of acid, 52.18 of alkali, and 17.82 of water; but cubic nitre crystallized contains something more water; for 100 gr. of these crystals lose about 4 by gentle drying; therefore 100 gr. of the crystallized salt contain 28.8 of acid, 50.09 of alkali, and 21.11 of water.

Of mere marine acid, 100 gr. of this alkali required from 63 to 66 or 67 gr.; perhaps one reason of this variety is, that it is exceeding hard to hit the true point of saturation. Allowing it to be 66 gr. then 100 gr. of perfectly dry common salt contain nearly 35 of real acid, 53 of alkali, and 13 of water; but 100 gr. of the crystallized salt lose 5 by evaporation; then 100 gr. of these crystals contain 33.3 of acid, 50 of alkali, and 16.7 of water.

The proportion of fixed air, alkali, and water, in crystallized mineral alkali, I investigated thus: 200 gr. of these crystals were dissolved in 240 of water; the solution was saturated by such a quantity of spirit of nitre as contained 40 of mere nitrous acid; hence I inferred, that these 200 gr. of alkali contained 70 of real alkali. The saturate solution weighed 40 gr. less than the sum of its original weight, and that of the spirit of nitre added to it; therefore it lost 40 gr. of fixed air. The remainder therefore of the original weight of the crystals, must have been water, that is, 90 gr.; consequently 100 gr. of these crystals contained 35 of alkali, 20 of fixed air, and 45 of water. This proportion is, particularly with regard to the alkali, very different from that found by Mr. Bergman and Lavoisier, which I impute to their having used soda recently crystallized. Mine had been made some months, and probably lost much water and fixed air by evaporation which altered the proportion of the whole. According to the calculation of these philosophers 100 gr. of this alkali takes up 80 of fixed air. The specific gravity of the crystallized mineral alkali, weighed in ether, I found to be 1.421.

*Of the volatile alkali.*—It is not possible, by the old chemical methods, to find the proportion of the ingredients in volatile alkalis, whether in a liquid or in



a concrete state; seeing that, though it may be separated from fixed air, yet it cannot from water, on account of its extreme volatility. Then to find this proportion we must recur to the experiments of Dr. Priestley, who by his new analysis produced this alkali free from the ærial acid and water in the form of air; and in the 3d volume of his observations, p. 294, informs us, that  $1\frac{6}{10}$  measures of alkaline air take up, and are saturated by, 1 measure of fixed air.\* Let us suppose the measure to contain 100 cubic inches; then 185 cubic inches of alkaline air take up 100 of fixed air; but 185 cubic inches of alkaline air weigh, at a medium, 42.55 gr.; and 100 cubic inches of fixed air weigh 57 gr.; then 100 gr. of pure volatile alkali, free from water, take up 134 of fixed air.

On expelling its ærial acid from a parcel of this alkali in a concrete state, and formed by sublimation, I found 100 gr. of it to contain 53 of fixed air, and therefore, according to the preceding reasoning, 39.47 of real alkali and 7.53 of water per cent. Saturating a solution of this alkali with the vitriolic, nitrous, and marine acids, I found, that 100 gr. of the mere alkali take up 106 of mere vitriolic acid, 115 of the nitrous, and 30 of the marine. The specific gravity of the concrete volatile alkali weighed in ether was 1.4076.

The proportion of water in the different ammoniacal salts I have not been able to find, on account of their volatility; but believe it to be very small, as volatile alkali and fixed air crystallize without the help of water, when both are in an ærial state.

*Of calcareous earth.*—I first dissolved this earth in the nitrous acid, and found that, after allowing for the loss of fixed air and the quantity of water I formerly mentioned, 100 gr. of the pure earth take up 104 of mere nitrous acid. Instead of dissolving this earth immediately in the vitriolic acid, I precipitated its solution in the nitrous by the gradual addition of the vitriolic, and found that to effect this, 91 or 92 gr. only of mere vitriolic acid were required. 100 gr. of this pure earth demand for their solution 112 of mere marine acid. The solution, which is at first colourless, grows greenish on standing. Natural gypsum varies in its proportion of acid, earth, and water, 100 gr. of it containing from 32 to 34 of acid, and also of earth, and from 26 to 32 of water. The artificial contains 32 of earth, 29.44 of acid, and 38.56 of water; when well dried it loses about 24 of water, and therefore contains 42 of earth, 39 of acid, and 19 of water per cent. 100 gr. nitrous selenite, carefully dried, contain 33.28 of acid, 32 of earth, and 34.72 of water. 100 gr. marine selenite,

\* I have lately repeated this experiment, and found that one measure of alkaline air is saturated by less than half of one measure of fixed air, but more than  $\frac{1}{2}$ ; conformably to Dr. Priestley's first experiment, p. 293; by which it appears, that 100 gr. of alkaline air require about 120 of fixed air to saturate them: and hence 100 gr. of concrete volatile alkali contain about 53 of fixed air, 44 of mere volatile alkali, and 3 of water.—Orig.



well dried, so as to lose no part of the acid, contain 42.56 of acid, 38 of earth, and 19.44 of water.

*Of magnesia or muriatic earth.*—This earth, perfectly dry and free from fixed air, could not be dissolved in any of the acids without heat. In the temperature of the atmosphere even the strongest nitrous acid did not act on it in 24 hours; but in a heat of  $180^{\circ}$  these acids, diluted with 4 or 6 times their quantity of water, attacked it very sensibly; but as much of the acids dissipated by heat, I could not judge of the exact quantity of acid requisite to dissolve a given quantity of it, any otherwise than by precipitating the solutions by another substance, whose capacity for taking up acids was known. The substance used was a tolerably caustic vegetable alkali. By this method I found, that 100 gr. of pure magnesia take up 125 gr. of mere vitriolic acid, 132 of the nitrous, and 140 of the marine. None of these solutions reddened vegetable blues; all of them appeared to contain something gelatinous; that in the marine acid became greenish on standing for some time.

100 gr. of perfectly dry Epsom salt contain 45.67 of mere vitriolic acid, 36.54 of pure earth, and 17.83 of water; but 100 gr. of crystallized Epsom lose 48 by drying, and consequently contain 23.75 of acid, 19 of earth, and 57.25 of water. Common Epsom salt contains an excess of acid, for its solution reddens vegetable blues. 100 gr. of nitrous Epsom, well dried, contain 35.64 of acid, 27 of pure earth, and 37.36 of water. The solution of marine Epsom cannot be tolerably dried without losing much of its acid, together with the water. The specific gravity of pure muriatic earth is 2.3296.

*Of earth of alum or argillaceous earth.*—This earth I found to contain about 26 per cent. of fixed air, though I had previously kept it red-hot for half an hour: this surprized me much, as most writers say it contains scarcely any. It dissolved in acids with a moderate effervescence till the heat was raised to  $220^{\circ}$ , after which I found the solution lighter than the quantities employed in the proportion I mentioned.

100 gr. of this earth, exclusive of the fixed air, require 133 of the mere vitriolic acid to dissolve them. This solution I made in a very dilute spirit of vitriol, whose specific gravity was 1.093, in which the proportion of acid to that of water was nearly as 1 to 14. This solution contained a slight excess of acid, turning vegetable blues into a brownish red; but it crystallized when cold, and the crystals were of the form of alum; so that I believe this to be nearly the proper proportion of its acid and earth; but there was not water enough to form large crystals. As this solution contained an excess of acid, I added more earth to it, but could not prevent its tinging blue paper red, till it formed an insoluble salt, that is, one that required an exceeding large quantity of water to dissolve it, and while part was thus become insoluble, yet another part would still retain



an excess of acid; so that at the same time part would be supersaturated with earth, and another with acid, if tinging vegetable blues be a mark of an excess of acidity, which indeed in this case seems dubious.

100 gr. of alum, perfectly dried, contained 42.74 of acid, 32.14 of earth, and 25.02 of water; but crystallized alum loses 44 per cent. by desiccation; therefore 100 gr. of it contain 23.94 acid, 18 of earth, and 58.06 of water.

100 gr. of this pure earth take up, as far as I can judge, 153 of the mere nitrous acid. The solution still reddened vegetable blues; but after the addition of this quantity of pure earth, I think it was, that an insoluble salt came to be formed. The solution, when cold, grew turbid, and could not be wholly dissolved by 500 times its weight of water. The same quantity of pure earth requires 173.45 of the mere marine acid for its solution; but the solution still reddens vegetable blues. After this, an insoluble salt was formed; but the beginning of its formation is difficultly discovered both in this and the former cases. The specific gravity of argillaceous earth, containing 25 per cent. of fixed air, I found to be 1.9901.

*Of phlogiston.*—Before proceeding to investigate its proportion in various compounds, and particularly in phlogisticated acids, it will be necessary to say something of its nature. It is allowed on all hands, that fixed air, or the aerial acid, as it is more properly called, is capable of existing in 2 states; the one fixed, concrete, and unelastic, as when it is actually combined with calcareous earth, alkalis, or magnesia; the other, fluid, elastic, and aëriform, as when it is actually disengaged from all combination. In its concrete and unelastic state, it can never be produced single and disengaged from other substances; for the moment it is separated from them, it assumes its aerial and elastic form. The same thing may be said of phlogiston: it can never be produced in a concrete state, single and uncombined with other substances; for the instant it is disengaged from them, it appears in a fluid and elastic form, and is then commonly called inflammable air. These different states of the same substance arise, according to discoveries of Dr. Black, from the different portions of elementary fire contained in such substance, and absorbed by it, while its sensible heat remains the same, and hence called its specific fire. For want of attention to these different states, the very existence of phlogiston as a distinct principle has been frequently called in question, and chemists have been required to exhibit it separate in its fixed state, without recollecting, that neither can fixed air be shown separate in a concrete state, nor that phlogiston may also be in the same predicament; while others have totally mistaken the nature of inflammable air, and imagined it to be a combination of acid and phlogiston. The reason why fixed air cannot be separated from any substance in a concrete state is, because when it is separated, for instance by means of an acid, there is always a double decomposi-



tion, the acid yielding its specific quantity of fire to the concrete fixed air, which then assumes an aërial form, while the fixed air yields the substance it was combined with to the acid. This is so true, that though a solution of lime in the nitrous acid yields a considerable quantity of heat, yet a solution of chalk in that acid scarcely yields any; for all the fire that is set loose, and rendered sensible in the first case, is absorbed by the fixed air in the 2d case, being precisely that which converts it into an aërial form. The separation of phlogiston from a metallic earth, in the form of inflammable air, arises from the same cause, the dissolving acid yielding its fire to the phlogiston, which then assumes an aërial form, while the phlogiston yields the metallic earth to the acid. It is true, that much sensible heat is produced on this occasion, for which 3 substantial reasons may be assigned; first, the proportion of fixed air in a given weight of crude calcareous earth, is much greater than that of phlogiston in any metal, as will hereafter be shown, it being in the former  $\frac{1}{3}$  of the whole, and that of phlogiston in the latter for the most part not even  $\frac{1}{6}$ . Secondly, much of the phlogiston combines with the acid itself during the solution, and expels part of its specific quantity of fire, as Dr. Crawford has shown, and as I have since experienced; and this fire must occasion sensible heat. Thirdly, much of the phlogiston, during solution, unites to the surrounding atmosphere, expelling also part of its specific fire, and this also must occasion sensible heat; and hence it is, that metallic solutions in vacuo are generally attended with less heat, though with a more violent effervescence, than in open air. The solution of metallic calces is not attended with so much heat as that of their respective metals, not only because neither the dissolving acids nor the surrounding air is much phlogisticated; but also because they contain an elastic fluid in a concrete state, which absorbs much of the fire given out by the dissolving acids, as it acquires an aërial state.

The origin and formation of inflammable air being thus explained, I now proceed to show its identity and homogeneity with phlogiston. By phlogiston is generally understood that principle in combustible bodies on which their inflammability principally depends; that principle to which metals owe their malleability and splendor; that which combined with vitriolic acid forms sulphur; that which diminishes respirable air. Now inflammable air is that very principle which alone is truly inflammable, as Mr. Volta has elegantly shown. In effect, combustible substances are either animal or vegetable, as horn, hair, grease, wood, &c. from all of which Dr. Hales has extracted inflammable air; or charcoal, from which Mr. Fontana has extracted it, as did Dr. Priestley from resin, spirit of wine, and ether, in all which it is the only principle that is inflammable, and they are inflammable only in proportion as they yield it; or phosphorus, from whose acid Dr. Priestley has obtained this air by means of minium, for it was the acid, and not the minium, that contained it, as Dr. Priestley rightly conjectured,



the acid obtained by deliquescence being never thoroughly dephlogisticated till heated and vitrified, as Mr. Margraaf has shown; or they are mineral substances, as sulphur, from which inflammable air has been separated by means of fixed alkalis, and, according to Dr. Priestley, also by means of marine air, or bitumens or bituminous substances, all of which may be made to yield it; or metallic substances, as zinc and regulus of arsenic, both of which are inflammable; but neither of them is so when deprived of its inflammable air: this is therefore the true and only principle of inflammability in any substance. I acknowledge that the inflammable air, proceeding from almost all these substances, is exceeding impure; that it contains from some a mixture of ærial acid or of oil, and from all some part of the substance which yields it or expels it, and hence its smell is different, according to the class of the substances from which it is extracted; but it is equally true, that none of these substances contribute to its inflammability; on the contrary, it is so much the less inflammable (that is, requires so much more air to be mixed with it before it flames) as it contains more of these heterogeneous substances. Hence inflammable air of the morasses is never totally consumed; and, on the contrary, inflammable air, from metals, which is the purest of all, is also the most inflammable.

Secondly, inflammable air is also the principle which reduces metallic earths to a metallic state, and gives them their metallic splendour. This has been proved analytically and synthetically, and therefore may be said to be as completely demonstrated as any thing in natural philosophy: thus Dr. Priestley has extracted inflammable air from iron and zinc by heat alone; and the iron, thus stripped of its phlogiston, lost its splendour, and was of a black colour, which is that which iron, slightly dephlogisticated, always assumes, as appears by martial æthiops: so also zinc and regulus of arsenic, when once inflamed, lose their metallic appearance: so also a mixture of lead and tin inflames in a moderate heat, and then both are converted into a calx destitute of splendour and malleability. On the other hand, if a current of inflammable air, in the act of combustion, be directed on the calces of iron, lead, or mercury, they are immediately revived and restored to their metallic form, as appears by the experiment of Mr. Chaussier. The following experiment is still more conclusive: if a polished plate of iron be put into a saturate and dilute solution of copper in the vitriolic or marine acids (I mention these because they are commonly used for the production of inflammable air, though the result is the same when other acids are used), no effervescence will arise, no inflammable air will be caught; but the iron will be dissolved, and the copper precipitated in its metallic form. Here inflammable air must be produced as usual, for the acid quits the copper and dissolves the iron; but this inflammable air instantly loses its ærial form, and unites to the copper, just as fixed air leaves alkalis to unite to lime without any



effervescence; and, by this same inflammable air is the copper evidently reduced, acquiring splendour, malleability, and every other metallic property. But if the solution of copper be not saturated with copper, a small quantity of inflammable air may be caught, as the excess of acid will disengage more of it from the iron than the calx of copper can take up. Inflammable air is then the principle that metallizes metallic earth; and if metals contain only a specific earth and phlogiston, inflammable air certainly contains nothing else but phlogiston. If iron and the arsenical acid be digested together, no inflammable air is produced; but the arsenical acid is, in great measure, converted into white arsenic, as Mr. Bergman has observed, and also Mr. Scheele; what reason can be assigned why inflammable air is not produced by this as well as by all other acids; but that this metallic acid received it, and was by it reduced to a semi-metallic form, as by pure phlogiston? Yet this acid produces inflammable air from zinc, because zinc gives out more phlogiston than the regulus of arsenic can take up; but it attracts and is metallized by a part of it, and it is only the excess that appears in the form of inflammable air, as Mr. Scheele has remarked. This inflammable air indeed is not pure, for it holds some of the regulus in solution; but this portion of regulus does not enter into its composition, as is very evident.

Thirdly, inflammable air is the substance which, with vitriolic acid, forms sulphur, for it is the very substance which the vitriolic acid separates from metals; and this substance, so separated, when in sufficient quantity, and in proper circumstances, unites to it in such proportion as to form common sulphur. Thus sulphur is formed by distilling concentrated vitriolic acid with iron or bismuth, or by distilling tartar vitriolate with regulus of antimony. It is this also that diminishes respirable air, as Dr. Priestley has clearly shown in the 5th vol. of his *Observations*, p. 84; for though in its complete aërial state, after it has absorbed that large quantity of fire requisite to its aërial form, it difficultly and slowly unites to respirable air in the heat of the atmosphere, their points of contact through their difference of density being very small, and there being no substance at hand to receive the large portion of elementary fire they both contain, and of which they must lose a large proportion before they can combine together; yet while inflammable air is, as Dr. Priestley elegantly expresses it, in its nascent state, before it acquires its whole quantity of specific fire, respirable air easily unites to it, and is diminished in proportion to its purity; but if to a mixture of both, igneous particles of density sufficient to be visible be introduced, a degree of heat is excited, which, as it rarefies the dephlogisticated part of respirable air to a greater degree than it can inflammable air, brings both into nearer contact, increases their attraction to each other, and both uniting give out their fire, or in other words inflame, when in proper proportion to each other, without any decomposition of either, unless the loss of a great part of their specific fire be



called a decomposition, which loss is not usually called a decomposition; for water is never said to be decomposed when it becomes ice, nor metals when they become solid on cooling.

In answer to all this it will be said, that inflammable air undoubtedly contains phlogiston, which produces all the beforementioned effects; but that the phlogiston it contains is united to some other substance, which some will have to be an acid, some an earth, and others respirable air. To these hypotheses I shall oppose one general observation, which is, that since inflammable air, when pure, that is, when disengaged from all heterogeneous substances which no way contribute to its inflammability, has always the same properties; it must, if it consists of phlogiston combined with any other substance, be always united to the same specific substance; that is, if this be an acid, it must be always the same species of acid, or if an earth, it must be always the same species of earth; for we find, that substances, which are only generically the same, always produce, with any other given substance, compounds whose properties are very different from each other. Thus we see that the different species of alkalis, or earths, or metals, produce, with one and the same species of acid compounds essentially different. This is a rule which, as far as I know, admits of no exception; and if we apply it to the abovementioned suppositions it will entirely destroy them; for it is impossible to think, that the phlogiston can in every substance, that produces inflammable air, meet either the same acid, or earth, or any respirable air.

But to be more particular, the following reasons demonstrate that an acid of any sort cannot be the basis of inflammable air. 1st. Inflammable air has been, by Dr. Priestley, separated from metals by mere heat. Now metals contain no acid, except perhaps their dephlogisticated calx, which those eminent chemists, Bergman and Scheele, suspect to be of an acid nature; but these calces cannot enter into the composition of inflammable air, otherwise the inflammable air of each different metal would have different properties, as already shown: nor indeed are these the acids that have been supposed to enter into the composition of inflammable air; but rather those acids by whose means it is extricated. But as this air is extricated from metals, not only by acids, but also by alkalis,\* this supposition must vanish of course.

The same reasons militate with equal strength against the supposition that an earth of any kind enters into the composition of this air; nor is there an instance of any earth rendered permanently fluid by any means, except in sparry air. Besides, if it were a metallic earth, it must necessarily be supposed to be in a metallic state; and how then could it escape the action of all kind of acids? for

\* Mem. Par. 1776, p. 687.



no acid is capable of decomposing inflammable air. Lastly, respirable air cannot be said to be the basis of inflammable air, unless we suppose that respirable air enters into the composition of metals; for Dr. Priestley has, by solar heat, extracted inflammable air from them in a vessel full of mercury, into which respirable air had no access, and even in vacuo. Besides respirable air and phlogiston form other compounds very different from inflammable air, viz. fixed and phlogisticated airs, as will presently be seen.

It may also be fairly urged against all these suppositions, that they are not founded on any direct experiment, nor any known analogy, but merely gratuitous, or at least deduced from experiments inadequate to their support; whereas the opinion that inflammable air is nothing else than phlogiston thrown into a fluid form by elementary fire, is directly founded on that experiment by which inflammable air is separated from metals by mere solar heat in the most perfect vacuum, just as fixed air united to marble and in a concrete state (in which it is nearly of equal density with gold) is separated from the marble, and thrown into a permanently fluid form by heat alone.

In favour of the existence of an acid in inflammable air, it has been said, that if this air be passed through water tinged blue by litmus, it reddens instantly. I have seen this frequently happen when inflammable air has been extracted from iron by spirit of vitriol; but if this air be washed, by passing it through lime-water, and then passed through, or agitated in, an infusion of litmus, it will not discolour it in the least: this I have seen done by Mr. Fontana in June 1779. It has also been said, that inflammable air and alkaline air, mixed together, form a cloud; but this has been fully disproved by Dr. Priestley, in the 4th volume of his observations. That an earth of any kind is essentially requisite to the constitution of inflammable air, seems to me utterly improbable; nor do I know of any experiment whence it can be inferred. That metallic substances may be held in solution by inflammable air is certain; but it is equally so, that they no way contribute to its inflammability, and are quite distinct from it.

But the opinion, that inflammable air consists of respirable air super saturated with phlogiston, is grounded on very specious arguments, drawn from experiments to be found in various parts of Dr. Priestley's works, which deserve so much the more attention as the facts mentioned by that excellent philosopher are not to be questioned. I shall endeavour to state them with accuracy; but shall at the same time accompany them with such remarks as seem to invalidate the conclusion that has been drawn from them.

In the first volume of Dr. Priestley's observations it appears, that a quantity of strong inflammable air, having been agitated in a glass jar immersed in a trough of water, the surface of which was exposed to the common atmosphere,



after the operation had continued 10 minutes, near  $\frac{1}{4}$  of the quantity had disappeared; the remainder became fit for respiration, and yet was weakly inflammable. By further agitation it was diminished half, and then admitted a candle to burn in it, though feebly; but, on continuing the agitation a little longer, it came to extinguish a candle. On this I shall remark, first, that it clearly follows, from this experiment, that if the external respirable air had no access to the inside of the jar, half nearly of the inflammable air was converted into, or consisted of respirable air, since such quantity of air was found in it after the operation. Now it is absolutely impossible that either could happen; for inflammable air could not be converted into half or even  $\frac{1}{3}$  or  $\frac{1}{4}$  of its volume of respirable air, as even  $\frac{1}{4}$  of respirable air contains more matter than 4 times its bulk of inflammable air; it is then evident, that the external air must have had access to it. Secondly, I agitated about half a pint of inflammable air, obtained from iron and previously passed through lime-water and kept over mercury, in about 12 times its bulk of water, out of which its air had been boiled in a glass bottle closed with a glass-stopper. The agitation continued at several times at least 2 hours. A large quantity of the air was indeed absorbed, as appeared by opening the bottle in water; but the remainder appeared, by the nitrous test, as noxious, and was also found to be as inflammable as at first. Even Dr. Priestley attests, that inflammable air, which had been united to water for one month, was afterwards as inflammable as ever. 3 PR. 267.

The true explanation of the first experiment appears, therefore, to be the following: first, water easily imbibes inflammable air, but does not combine with it; for after it has imbibed  $\frac{1}{4}$  of it, its taste is no way altered, as Dr. Priestley has observed. 1 PR. 196. Water also easily imbibes common air: therefore when inflammable air is agitated in water, having a communication with the atmosphere, the inflammable air must necessarily be diminished by reason of its absorption, and the part so absorbed immediately escapes out of the water into the atmosphere, as is evident by the smell which is perceived when the quantity of inflammable air is considerable. This escape gives room for the further absorption of the inflammable air which then escapes in the same manner. In the mean time the common air under the jar rises into it, as appears by the direct experiments both of Dr. Priestley and Mr. Fontana; and hence the air in the jar must appear by the nitrous test slightly phlogisticated and respirable; but a further agitation will decompose the common air, as we shall soon see, and then a candle will be extinguished. The same process takes place when inflammable air stands long in water whose surface is exposed to the atmosphere.

Another experiment of the same tendency, but seemingly more decisive, is to be found in the 4th vol. of Dr. Priestley's Observations, p. 368. There it is related, that a portion of inflammable air, inclosed in a glass tube hermetically



sealed and heated till the glass was softened, stained the glass black, and the tube being opened, the air was found reduced to  $\frac{1}{3}$  of its bulk; and this residuum was found to be mere phlogisticated air, neither precipitating lime-water, nor being affected by nitrous air, or in the least inflammable. Yet decisive as this experiment appears, a little consideration will show the absolute impossibility that inflammable air should consist of  $\frac{1}{3}$  phlogisticated air and  $\frac{2}{3}$  phlogiston; for, in the first place, one cubic inch of phlogisticated air weighs 0.377 of a grain: now let us suppose, that to this phlogisticated air is added  $\frac{2}{3}$  of its bulk of phlogiston; and to make the supposition still stronger, let us also suppose that phlogiston has no weight; then, by the supposition, this compound of phlogisticated air and phlogiston will constitute inflammable air, and amount to a bulk of 3 cubic inches, and these 3 cubic inches will weigh no more than 0.377 of a grain; but if 3 cubic inches of inflammable air weigh 0.377 of a grain, 1 cubic inch should weigh 0.105 of a grain, which cannot be; for then inflammable air would be little more than  $\frac{1}{3}$  lighter than common air, contrary to all the experiments that have been hitherto made, and particularly those of Mr. Cavendish, Fontana, and Dr. Priestley himself, which show it to be about 11 times lighter than common air. Secondly, it is said, that the matter which stained the glass black was the true phlogistic part of inflammable air, and was afterwards separated by means of minium. This then contained no phlogisticated air; but is it not certain, that if there had been enough of it, the minium would have been reduced and converted into lead? And might not inflammable air be again separated from that lead, though no phlogisticated or common air were at hand to supply its other supposed constituent part? Thirdly, in one of Dr. Priestley's experiments the inflammable air, contained in the glass tube which was most heated, was reduced to so small a bubble, that no experiment could be made on it: therefore, in this, at least, the quantity of phlogisticated air did not amount to  $\frac{1}{3}$ , but was quite inconsiderable; the remainder then, being taken up by the calx of lead in the glass, was pure mere phlogiston; so that this experiment is a strong proof of my opinion. Fourthly, if phlogiston could be decomposed by heat, and then leave a residuum of phlogisticated air, amounting to  $\frac{1}{3}$  of its bulk, the diminution arising from its inflammation with common or dephlogisticated air could never be so great as it is found to be by repeated experiments; for when inflammable and common air are fired in the proportion of 11 of the latter to 4 of the former, a bulk equal to the whole of the inflammable air, and to  $\frac{1}{3}$  of the common air, disappears, according to Mr. Volta, and the diminution is about  $\frac{2}{3}$  of the whole, or more exactly out of 15 measures, only 8.8 remain; but if the inflammable air were decomposed, and  $\frac{1}{3}$  of it, being phlogisticated air, should remain, then not quite  $\frac{1}{3}$  the whole would vanish; and the residuum should be 10.54 measures. This evidently proves, that pure inflam-



mable air is never decomposed, unless the loss of its fire be called a decomposition; but in the act of inflammation is totally transferred on the pure part of respirable air to which it unites. Fifthly, to obtain still a clearer insight into this matter, I entreated Mr. Cavallo, who is very expert in the management of the blow-pipe, as well as in pneumatic experiments, to repeat this experiment in my laboratory. We accordingly filled a tube 10.5 inches long, and  $\frac{1}{4}$  of an inch in diameter, with inflammable air from iron received over mercury, and having made the tube red-hot throughout and black, and softened it so far as to endanger the escape of the air, we opened it on mercury. The air was diminished only  $\frac{1}{10}$ , and inflamed with an explosion as loud as an equal quantity of the same inflammable air that had not been heated.

The only question that remains then is, whence the phlogisticated air proceeded which Dr. Priestley mentions to have found? The circumstance of his experiment would furnish a plausible answer; but the doctor has lately informed me, that he believes the air was really inflammable, but, being a very small quantity, escaped before the flame could be applied. It seems, therefore sufficiently proved, that inflammable air purified from the acids or other substances that expel it from its basis, and also from all particles of the body to which it was originally united, such as inflammable air from metals received on mercury, and well washed in lime-water, is one and the same substance with phlogiston, differing only in quantity of fire, inflammable air containing nearly the same quantity of this element as the same bulk of atmospheric air, as Dr. Crawford has found by some late experiments, an account of which will soon be laid before the public. This does not contradict that most important discovery of this ingenious philosopher, that fire and phlogiston repel each other: the meaning of this being only, that the addition of phlogiston to any substance, as to respirable air, dephlogisticated acids, metallie calces, expels part of the fire already contained in such substance; and, on the contrary, by the removal of phlogiston from any substance, the quantity of fire absorbed by such substance is increased.

It may appear extraordinary, supposing inflammable air and phlogiston to be the same substance, that inflammable air should mix so easily with water, whereas phlogiston constantly repels and is repelled by it; but this entirely depends on the state of this same substance, which, when fixed and concrete, is called phlogiston, and, when rarefied and aëriform, inflammable air. In this latter state it mixes with water in proportion to its rarefaction, as it even does in the less dense forms of its concrete state: thus ether is totally absorbed by 10 times its weight of water. The animal oil of Dippel mixes entirely with water; so does pure Petrol, and essential oils frequently distilled, and the spiritus rector of plants. Much more remains to be said of the different states of phlogiston from its most rarefied known state, viz. that of inflammable air, to its most condensed.



state, that in which it is combined with metallic earths, &c. I have already distinguished eight intermediate states each differing from the other by the portion of elementary fire they contain, this quantity being, as far as I can judge directly, as the rarefaction of the phlogiston; but these researches are foreign to my present subject. I shall only remark, that phlogiston, in a state perhaps 100 times rarer than inflammable air, and consequently containing much more fire, may possibly constitute the electric fluid.

P. S. Since the above was written, I have been honoured with a letter from Dr. Priestley, in which he informs me, that he has reduced the calces of iron, copper, lead, and tin, merely by melting them in inflammable air by means of a burning glass. A certain quantity of inflammable air was absorbed by each during their reduction; but the unabsorbed part was equally inflammable, so that there was no decomposition; but the remainder was of the same nature as the part absorbed. He also, by the same means, converted nitrous vapour into nitrous air, and the phosphoric acid into phosphorus. And since the communication of the last-mentioned experiments, which seem to him also a direct proof of the identity of inflammable air and phlogiston, he has been so obliging as to inform me, that he has revived the calces of metals in alkaline air as well as in inflammable air, and also formed a phosphorus; and that he has little doubt but that he shall be able to produce any thing else in which phlogiston is supposed to be concerned. This, he says, agrees with several of his former experiments, especially that in which he produces inflammable air from alkaline air, by means of the electric spark and volatile alkali from iron, supersaturated with phlogiston by means of nitrous air, which he has repeatedly done since the publication of his last volume. This observation, he adds, may help to explain some things in the theory of chemistry, especially the affinity which all acids have both with phlogiston and with alkalis; but, he says, that alkaline air contains something else besides phlogiston; because when this air is used, there is always a residuum of something that is neither alkaline nor inflammable air; but he wants more sun-shine to complete and extend his experiments on this subject.\*

*Of the quantity of phlogiston in nitrous air.*—100 gr. of filings of iron being dissolved in a sufficient quantity of very dilute vitriolic acid, produced, with the assistance of heat gradually applied, 155 cubic inches of inflammable air, the barometer at 29.5, and the thermometer between 50 and 60°. Now inflammable air and phlogiston being the same thing, this quantity of inflammable air amounts to 5.42 gr. of phlogiston.

Again, 100 gr. of iron, dissolved in dephlogisticated nitrous acid, in a heat

\* Since this paper was committed to the press, I find that Mr. Pelletier has reduced the arsenical acid to a regulus, by merely passing inflammable air through the solution of that acid in twice its weight of water. Roz. Journ. February 1782.—Orig:



gradually applied and raised to the utmost, afford 83.87 cubic inches of nitrous air. And as this nitrous air contains nearly the whole quantity of phlogiston which iron will part with (it being more completely dephlogisticated by this acid than by any other means) it follows, that 83.87 cubic inches of nitrous air contain at least 5.42 gr. of phlogiston; but it may reasonably be thought that the whole quantity of phlogiston which iron will part with, is not expelled by the vitriolic acid, and that nitrous acid may expel and take up more of it. To try whether this was really so, I calcined a certain quantity of green vitriol, till its ferruginous basis was quite insipid; I then extracted from 64 gr. of this ochre 2 cubic inches of nitrous air, consequently 100 gr. of this ochre would give 3.12 cubic inches of nitrous air; and if 83.87 cubic inches of nitrous air contain 5.42 of phlogiston, then 3.12 cubic inches of this air contain 0.2 of a grain of phlogiston; consequently, nitrous acid extracts from 100 gr. of iron,  $\frac{2}{10}$  of a grain more phlogiston than the vitriolic acid does; therefore 83.87 cubic inches of nitrous air, containing nearly all the phlogiston which iron gives out, contain 5.62 gr. of phlogiston. Then 100 cubic inches of nitrous air contain 6.7 gr. of phlogiston, and since 100 cubic inches of nitrous air weigh 39.9 gr. they must also contain 33.2 gr. of nitrous acid. Also, 100 gr. of nitrous air contain 16.792 of phlogiston, and 83.208 of acid.

When first I made these experiments I imagined, that the nitrous air thus expelled contained all the phlogiston of the metals dissolved in the nitrous acid, as this acid is well-known to dephlogistate metals as perfectly as possible; but I soon observed, as did Dr. Priestley and Mr. Fontana, that the greater part of this is air resorbed and detained in the solution, the acid and calx having, according to the beautiful remark of Mr. Scheele, a greater attraction to phlogiston than either separately; yet that the calculation is nearly just, will appear clearly in my next paper, by its coincidence with the quantity of phlogiston discovered in lead by Dr. Priestley and that which is contained very evidently in regulus of arsenic, silver, and quicksilver.

*Of the quantity of phlogiston in fixed air.*—Before I attempt to determine this quantity, it will be necessary to prove that it contains any; and for this purpose minutely to examine its nature and origin. Dr. Priestley first discovered, that in all processes, in which phlogiston is disengaged from any substance, as in combustion, respiration, calcination of metals, putrefaction, decomposition of nitrous air by respirable air, &c. fixed air is precipitated from the common or dephlogisticated air in which these processes are performed, and that these last airs are diminished both in weight and bulk, and are afterwards less fit, or absolutely unfit, for these processes, according to the quantity of phlogiston that was set loose. These facts are admitted by all, let their systems be what they may. However, Dr. Priestley thinks he has seen one exception to this general rule;



for, he says, that in the combustion of inflammable and common air, no fixed air is precipitated, 5 Pr. 124. He also seems inclined to admit another exception in the case of the combustion of sulphur.

The questions that here arise are, first, whether the fixed air that appears in these circumstances proceeded from the respirable air or not? Secondly, if it proceeded from the respirable air, whether it pre-existed in that air; or whether it was generated during the process that exhibits it? and if so, what are its constituent parts? The first question is easily answered; for in such phlogistic processes as are attended with the destruction of the substances that are known to contain fixed air, as those of the animal and vegetable kingdom, the fixed air may be supposed to proceed in many cases, both from the decomposed substance and from the respirable air; and of this sort are the processes of combustion of most animal and vegetable substances, and fermentation; but the fixed air, that appears in such phlogistic processes as are performed on substances that contain no fixed air, must be deemed to proceed from the respirable air singly. And of this case we have 4 clear instances; the calcination of metals; the decomposition of nitrous air by respirable air; the diminution of common air by the electric spark; and, lastly, its diminution by amalgamation.

And first as to the calcination of metals, Dr. Priestley has observed, that by this operation respirable air (and only respirable air) is diminished between  $\frac{1}{4}$  and  $\frac{1}{5}$ , both in its weight and bulk; but Mr. Lavoisier has demonstrated, that nothing is lost or escapes through the vessels, as Mr. Scheele would have it; for the weight and materials continue undiminished when the operation is performed in close vessels. That part, therefore, which the air loses, is taken up by the metallic calx, which accordingly is found to gain the very weight which the air loses. Now the air contained in the calx is fixed air; for Mr. Lavoisier also observed, that by the calcination of lead, by solar heat, over lime-water, the water was rendered slightly turbid. It is true, that Dr. Priestley, in a similar experiment, did not observe this turbidity; but he accounts for this circumstance very justly, by supposing that the calx of lead absorbed the fixed air preferably to the lime. And this supposition is not gratuitous; for metallic calces, and particularly those of lead, are known to attract fixed air as strongly as quick lime, or rather more strongly: and what sets this matter beyond all doubt, the calces of lead all yield fixed air by heat, and the grey calx of lead, in particular, which was that produced by Dr. Priestley, in the experiment to which I allude, affords by heat fixed air only. Other calces of lead after fixed air afford also dephlogisticated air; but this I shall show also to have been originally fixed air. If filings of iron be mixed with water in close vessels, they will be converted into rust, and the incumbent air diminished  $\frac{1}{4}$ , as Mr. Lavoisier attests; but Dr. Priestley has shown, that rust of iron yields scarce any other than fixed air, which may



be expelled out of it by mere heat. Nay, iron alone, exposed to common air, over a vessel of water for 3 months, reduced this air  $\frac{1}{5}$ ; and being exposed to dephlogisticated air, over a vessel of mercury, it reduced it  $\frac{1}{10}$  in 9 months. In all these cases the fixed air could surely come from nothing else but the incumbent respirable air and the phlogiston of the metal.

Secondly, it is well known, that if nitrous air be decomposed by respirable air over lime-water, the lime will be precipitated. In this case also, the fixed air must proceed from the respirable air and the phlogiston of the nitrous air; for it cannot proceed from the nitrous acid, as this acid is not decomposed, but is taken up by the water over which the mixture of both airs is made, as Mr. Bewly has undeniably proved: and hence it is, that unless a large quantity of lime-water be used, so as to contain enough for both the nitrous and ærial acids to act on, there will be no precipitation of lime, as Mr. Fontana has observed; for the nitrous acid will seize on the lime preferably to the ærial. Dr. Priestley indeed observed, that if a bladder, filled with nitrous air, be dipped in lime-water, it occasions a precipitation of lime on the surface of the water. 1 Pr. 213. But he elsewhere acknowledges, that this proceeds from the inability of the bladder to confine nitrous air. 1 Pr. 76 and 128, which Mr. Baume also long ago observed, without knowing any thing more of this air: Baume sur l'Ether, 285. The phlogiston passes through the bladder, and unites to the common air contiguous to it. Besides, nitrous air acts on the bladder itself, and extracts fixed air from it. 1 Pr. 214. Hence also, if rain-water carefully boiled, and freed from its own air, be made to absorb a quantity of nitrous air, it will again, on boiling, yield it back as pure as at first; but if common water be made to imbibe nitrous air in the same manner, it will, on boiling, yield also a portion of fixed air. 3 Pr. 109. Does not this happen clearly because common water contains atmospheric air, or air somewhat purer, which is converted into fixed air by mixture with the nitrous air? This experiment also shows, that water itself never unites to phlogiston, since it does not take any from nitrous air, where the union of phlogiston to the acid is of the laxest kind.

Thirdly, if the electric spark be taken through common air, this air will be diminished  $\frac{1}{4}$ , and a solution of lime, if contiguous, will be precipitated, and a solution of turnsole tinged red. 1 Pr. 184, 186. Whence could the fixed air here produced proceed, but from the common air, and the phlogiston of the metallic conductors? This excellent philosopher has even shown it could proceed from nothing else; for after that air had contributed all it could to that production, that is, was diminished to the utmost, he changed the liquors, but could produce no change in their colour, nor the least sign of fixed air. This experiment has also been repeated in France, and the inside of the glass tube, in which the common air was contained, was moistened with a solution of caustic



fixed alkali, and the alkali, after the operation, was found crystallized; but when the tube was exhausted of air, and the experiment repeated, no change whatever was found in the alkali. *Essai sur l'Electricité*, par M. Le Comte De La Ceppe, vol. 1, p. 155.

Fourthly, if lead and mercury be agitated in a phial, partly filled with common air, this air will be diminished  $\frac{1}{4}$ , and the residuum will be found completely phlogisticated. The diminution will be still greater if the phial contain dephlogisticated air: 4 Pr. 149. The lead is converted into a calx, calcination being the known effect of the amalgamation of the base metals; and this calx absorbed the fixed air produced, for Dr. Priestley expelled this air from it: 4 Pr. 144; and hence an amalgama of lead and mercury decrepitates when heated. Whence could this fixed air proceed, but from the respirable air? For surely neither lead nor mercury contain any.

If the above experiments be attended to, the answer to the 2d question will be equally obvious. It is certain, that common air does not consist of  $\frac{1}{4}$  of its bulk of fixed air; for if it did, the remaining  $\frac{3}{4}$  must be dephlogisticated air: and if so, then the absolute weight of a mixture of  $\frac{3}{4}$  dephlogisticated air and  $\frac{1}{4}$  fixed air should coincide at least nearly with the absolute weight of an equal bulk of common air; but in fact it is very far from it: for 4 cubic inches of common air weighed 1.54 gr.; but a mixture of 3 cubic inches of dephlogisticated air and 1 of fixed air weighs 1.83 gr.; neither indeed has so large a portion of fixed air been ever supposed to exist in common air. Besides, if fixed air pre-existed in common air, it might be separated from it by lime-water, at least in some degree. I have mixed 1 part of fixed air with 20 of dephlogisticated air, and also with 20 of phlogisticated air in close vessels, and these mixtures did not fail to render lime-water turbid. But let common air be agitated in lime-water ever so long in close vessels, not the least cloudiness will appear; nor does quick-lime, in these circumstances, in the least affect common air, as Dr. Priestley has observed. 2 Pr. 184. The spontaneous precipitation of lime-water arises therefore from an accidental diffusion of fixed air through common air, and the slowness of this precipitation shows its quantity to be very small. The inference from the above experiments will be much stronger against the pre-existence of fixed air in respirable air, if, instead of common air, dephlogisticated air be used; for there the diminution is so great, and the quantity of fixed air produced so considerable, that it can by no means be supposed to have pre-existed, its properties being so very opposite to those of dephlogisticated air.

Having synthetically proved the constituent parts of fixed air to be pure elementary air and phlogiston, I shall now endeavour to do the same by its analysis: and, in the first place, that it contains phlogiston, and even in such quantity as to deserve to be classed among the phlogisticated acids, appears by



its action on black manganese. This semi-metallic calx, as has been proved by that admirable chemist Mr. Scheele, is completely soluble only in phlogisticated acids, and is precipitable from them by fixed alkalis in the form of a white calx. He also found, that this manganese is also soluble in water strongly impregnated with fixed air, and is also precipitable from it in the form of a white calx. 35 Mem. Stock. p. 96.

If fixed air be repeatedly dissolved in, and expelled from water, it leaves each time a residuum which is insoluble in water, diminishable by nitrous air, and capable of supporting animal life. Hence it is evidently decomposed, the phlogiston separating from it, and gradually uniting to the common atmosphere by reason of the repulsive power between it and water. Dr. Priestley indeed found, that a candle would not burn in it; but this arises only from a mixture of a small quantity of fixed air not yet decomposed, of which, according to the experiments of Mr. Cavendish,  $\frac{1}{9}$  is sufficient to extinguish a candle.

Again, Mr. Achard has converted fixed air into air of nearly the same purity as common air by passing it 5 or 6 times through melted nitre. Mem. Berlin. 1778. Mr. Cavallo passed it but once through melted nitre, and yet found it considerably meliorated, for it was diminished by nitrous air. In this case the nitrous acid attracted the phlogiston; for it is known to become phlogisticated by the fusion of nitre, so as to be expellable even by the vegetable acids. 2 N. Act. Ups. 171. And aqua regia may be made by mixing nitre with marine acid.

I shall now proceed to investigate the proportion of phlogiston and elementary or respirable air in fixed air. Dr. Priestley, in the 4th volume of his Observations, p. 380, has satisfactorily proved, that nitrous air parts with as much phlogiston to common air, as an equal bulk of inflammable air does when fired in the same proportion of common air. Now, when inflammable air unites with common air, its whole weight unites to it, as it contains nothing else but pure phlogiston; since therefore nitrous air phlogisticates common air to the same degree that inflammable air does, it parts with a quantity of phlogiston equal to the weight of a volume of inflammable air similar to that of nitrous air. Now 100 cubic inches of inflammable air weigh 3.5 gr.; therefore, 100 cubic inches of nitrous air part with 3.5 gr. of phlogiston when they communicate their phlogiston to as much common air as will take it up. I say, that nitrous air parts with as much phlogiston, because it is certain, that it does not part with the whole of its phlogiston to common or dephlogisticated air, for it contains much more, as already shown, and, as appears by the red colour, it constantly assumes when mixed with common or dephlogisticated air, which colour belongs to the nitrous acid combined with its remaining phlogiston, and not to the fixed air then produced, nor to the phlogisticated air remaining, as is very evident. Hence the acid, thus formed, is volatile. 4 Pr. 267.



One measure of the purest dephlogisticated air and 2 of nitrous air occupy only  $\frac{3}{100}$  parts of one measure, as Dr. Priestley has observed, vol. 4, p. 245. Suppose 1 measure to contain 100 cubic inches, then the whole very nearly of the nitrous air will disappear, its acid uniting to the water over which the experiment is made, and 97 cubic inches of the dephlogisticated air, which is converted into fixed air by its union with the phlogiston of the nitrous air; therefore 97 cubic inches of dephlogisticated air take up all the phlogiston which 200 cubic inches of nitrous air will part with; and this we have found to be 7 grains; therefore, a weight of fixed air, equal to that of 97 cubic inches of dephlogisticated air and 7 of phlogiston, will contain 7 gr. of phlogiston. Now, 97 cubic inches of dephlogisticated air weigh 40.74 gr.; to which adding 7 gr. we have the whole weight of the fixed air equal 47.74 gr. = 83.755 cubic inches; and consequently 100 cubic inches of fixed air contain 8.357 gr. of phlogiston, and the remainder elementary air.

100 gr. of fixed air contain 14.661 of phlogiston and 85.339 of elementary air; which, when stripped of phlogiston, and impregnated with its proper proportion of elementary fire, becomes again dephlogisticated air. Hence also 100 cubic inches of dephlogisticated air are converted into fixed air by 7.2165 gr. of phlogiston, and will be then reduced to the bulk of 86.34 cubic inches. And reciprocally, 100 cubic inches of fixed air, being decomposed, will afford 115.821 cubic inches of dephlogisticated air, and part with 7.2165 gr. of phlogiston, supposing the decomposition to be complete; that is, the dephlogisticated air absolutely pure.

*Of the quantity of phlogiston in vitriolic air.*—The method I pursued was this; 1st. I found the quantity of nitrous air a given weight of copper afforded when dissolved in the dephlogisticated nitrous acid, and by that means how much phlogiston it parts with. 2dly. I found the quantity of copper which a given quantity of the dephlogisticated vitriolic acid could dissolve; and observed, that it could not dissolve the greatest quantity of copper without dephlogisticating a further quantity which it does not dissolve. 3dly. I found how much it dephlogisticates what it thoroughly dissolves, and how much it dephlogisticates what it barely calcines. 4thly. How much inflammable air a given quantity of copper affords when dissolved in the vitriolic acid to the greatest advantage. 5thly. I deduct from the whole quantity of phlogiston expelled by the vitriolic acid the quantity of it contained in the inflammable air; the remainder shows the quantity of it contained in the vitriolic air.

The particulars were as follow:—1st. 100 gr. of copper dissolved in the dephlogisticated nitrous acid afforded 67.5 cubic inches of nitrous air, which, according to the before-mentioned calculation, contain 4.52 gr. of phlogiston. 2dly. 100 gr. of real vitriolic acid take up or dissolve 54.73 of copper, and 100



gr. of copper require about 182.714 gr. of real vitriolic acid to dissolve them. Again, 100 gr. of copper, when dissolved in the vitriolic acid, retain only as much phlogiston as is contained in 3 cubic inches of nitrous air, that is, 0.2 of a grain; therefore, since 100 gr. of copper give out 4.52 of phlogiston, the vitriolic acid strips it of  $4.52 - 0.2$ , that is, 4.32 gr. of phlogiston.

3dly. To dissolve 70 gr. of copper in the vitriolic acid, to the greatest advantage, 20 more must be slightly dephlogisticated; therefore, to dissolve 100 gr. of copper in this acid, 28.6 more must be slightly dephlogisticated. 8 grs. of this slightly dephlogisticated calx afforded 4 cubic inches of nitrous air; therefore, 28.6 would afford 14.3, which contain 0.958 gr. of phlogiston; but 28.6 gr. of copper, before any dephlogistication, contain 1.292 gr. of phlogiston; therefore, they lose by this slight dephlogistication 0.344 of a grain of phlogiston. Hence, when 100 gr. of copper are dissolved in the vitriolic acid, the quantity of phlogiston expelled is  $4.32 + 0.34 = 4.66$  gr.

4thly. The quantity of inflammable air afforded by the most advantageous solution of 100 gr. of copper in the vitriolic acid, is 11 cubic inches, which amount to 0.385 of a grain of phlogiston. 5thly. The solution of 100 gr. of copper in the vitriolic acid afforded over mercury 75.71 cubic inches of air; but of this only 11 cubic inches were inflammable air, the remainder therefore was vitriolic acid air, amounting to 64.71 cubic inches. 6thly. Then the whole quantity of phlogiston expelled during the solution of 100 gr. of copper in the vitriolic acid, is 4.66 gr.; of this inflammable air contains but 0.385 of a grain: the remainder therefore, which consists of 4.275 gr. must be contained in the 64.71 cubic inches of vitriolic air; therefore, 100 cubic inches of vitriolic air contain 6.6 gr. of phlogiston, and 71.2 gr. of acid, and 100 cubic inches of this air weighing 77.8 gr., 100 gr. of this air contain 8.48 gr. of phlogiston and 91.52 of acid.

*Of the quantity of phlogiston in sulphur.*—This I endeavoured to find by estimating the quantity of fixed air produced during its combustion. To the top of a glass bell, which was open, I firmly tied and cemented a large bladder, destined to receive the air expanded by combustion, a quantity of which generally escapes when this precaution is not used. Under this bell, which contained about 3000 cubic inches of air, I placed a candle of sulphur, weighing 347 gr.; its wick, which was not consumed, weighed half a grain: it was supported by a very thin concave plate of tin, to prevent the sulphur from flowing over during the combustion, and both were supported by an iron wire, fixed on a shelf in a tub of water. As soon as the sulphur was fired with a very feeble flame, it was covered with the bell, the air being squeezed out of the bladder. The inside of the bell was soon filled with white fumes, so that the flame could not be seen. In an hour after, the fumes thoroughly subsided, and all was cold. The water rose



within the bell to a height equal to 87.2 cubic inches; whence I deduce that 87.2 cubic inches of fixed air were produced, which contain 7.287 gr. of phlogiston, which separated from the vitriolic acid, and united to the dephlogisticated part of the common air under the bell. The candle of sulphur being weighed, was found to have lost 20.75 gr.; therefore, 20.75 gr. of sulphur contain 7.287 gr. of phlogiston, besides the quantity of phlogiston which remained in the vitriolic air. This air must have amounted to  $20.75 - 7.287 = 13.463$  gr. which contain 1.141 gr. of phlogiston; therefore the whole quantity of phlogiston in 20.75 gr. of sulphur, is 8.428 gr.; therefore, 100 gr. of sulphur contain 40.61 gr. of phlogiston and 59.39 of vitriolic acid.

Several attempts have hitherto been made to determine the proportion of the constituent parts of sulphur; but all were evidently defective. The first was that of Stahl, who calculated the quantity of phlogiston from that of the acid remaining after slow combustion; but as much, both of acid and phlogiston, was dissipated, and as the remaining acid was also phlogisticated, and attracted much of the moisture of the air, no conclusion whatever could be drawn from this experiment. The 2d method was, to form a liver of sulphur, and convert this by a gentle long continued heat into a tartar vitriolate, and then calculate the weight a given quantity of alkali would gain by this operation. This was also devised by Stahl, and followed by Brandt and Newman, and by it they determined the proportion of phlogiston to that of acid to be nearly as 1 to 16. But during the formation of the liver of sulphur, whether in the moist or dry way, much of the phlogiston and acid is dissipated, as is evident by the vapour and smell that proceed from it, their alkali also contained fixed air, which it lost during the operation, and of which they kept no account, as they were ignorant of its existence; and the tartar vitriol formed by them or sal polycreste retained much undecomposed sulphur, as always happens when it is not strongly heated; so that this method also was very imperfect: however some subsequent chemists, who made the experiment with more care, concluded from it, that sulphur contained  $\frac{1}{4}$  of phlogiston. Exleben, § 760.

By weighing flowers of sulphur in a perforated brass box in water, I found its specific gravity to be 1.924. It remained in the water a quarter of an hour before any air issued from it, and then some bubbles arose; but when I opened the box, I found the middle part of the flowers quite dry, so that I make no doubt but some air still remained, and that its specific gravity is still greater. Mr. Petit weighed it in oil, and found its specific gravity 2.344, which I believe to be nearly the truth.

*Of the quantity of phlogiston in marine acid air.*—8 gr. of copper dissolved in colourless spirit of salt, afforded but 4.9 cubic inches of air, when the air was received over water, and this air was inflammable. 8.5 gr. of copper being dis-



solved in the same quantity of the same spirit of salt, and the air received over mercury, afforded 91.28 cubic inches of air; but of these only 4.9 cubic inches were inflammable air; the remainder therefore, viz. 86.38, were marine air, which weigh 56.49 gr.

Now, as spirit of salt certainly does not dephlogistate copper more than the vitriolic acid does, it follows, that these 4.9 cubic inches of inflammable air, and 86.38 cubic inches of marine air, do not contain more phlogiston than would be separated from the same quantity of copper by the vitriolic acid: and since 100 grains of copper would yield to the vitriolic acid 4.32 gr. of phlogiston, 8.5 gr. of copper would yield 0.367 of a grain of phlogiston; this then is the whole quantity extracted by the marine acid, and contained in 91.28 cubic inches of air, and deducting from this the quantity of phlogiston contained in 4.9 cubic inches of inflammable air ( $= 0.171$  of a grain,) the remainder, viz.  $0.367 - 0.171 = 0.196$ , is all the phlogiston that can be found in 86.38 cubic inches of marine air. Then 100 cubic inches of marine air can contain but 0.227 nearly of a grain of phlogiston, 65.173 of acid. Hence we see why it acts so feebly on oils, spirit of wine, &c. having a very small affinity to phlogiston; and why it is not dislodged from any basis by uniting with phlogiston, as the vitriolic and nitrous acids are, its affinity to it being inconsiderable.

*XVI. Of the Method of rendering very sensible the weakest Natural or Artificial Electricity. By Mr. Alexander Volta, Professor of Experimental Philosophy in Como, &c. &c. From the Italian. p. 237.*

Whenever, in observing the atmospherical electricity, no degree of it can be discovered by the ordinary methods of performing those experiments, it is difficult to determine whether any electricity at all does or does not exist in the atmosphere at those times; since it may exist, and the quantity of it only be so small as not to affect the electrometers employed. In that case therefore, if we rely on the common electrometers, even the most sensible, we must conclude, that neither the conductor nor the atmosphere, so high as the conductor reaches, contain any electricity; but by means of the apparatus here described, it will be found, that the said conductors are never entirely void of electricity, and consequently the air, which surrounds them, is also at all times electrified. This method not only shows the existence of electricity, but also ascertains whether it is positive or negative, and that when the atmospherical conductor itself is not capable of attracting the finest thread; and if the conductor were to show any very small attraction, then, by means of our apparatus, there may be obtained even strong sparks. The electrophorus in this case might perhaps better deserve the name of electrometer, or micro-electrometer, but Mr. V. rather calls it a



condenser of electricity, for the sake of using a word which expresses at once the reason and cause of the phenomena to be treated of in this paper.

The whole method may be reduced to the following few observations.—1. An electrophorus must be procured, the resinous coat of which must be very thin, and either not at all electrified, or, if electrified, its electricity be entirely extinguished. 2. Its usual metal plate must be laid on this resinous and unelectrified plate, in full and flat contact; but care must be taken that it does in no point touch the lamina of metal on which the resinous stratum is usually fastened. 3. Those plates being so conjointly placed, a conducting communication, viz. a wire, must be brought from the atmospherical conductor to touch the metal plate of the electrophorus, and to touch that only. 4. The apparatus must be left in that situation for a certain time, viz. till the metal plate may have acquired a sufficient quantity of electricity through the conducting communication, which brings it from the atmospherical conductor very slowly. 5. Lastly, the conducting communication must be removed from the contact of the metal plate: the metal plate is then separated from the resinous one, by lifting it up by its insulating handle, after which it is in a state of attracting, of electrifying an electrometer, or, if the electricity be sufficiently strong, of giving sparks, &c. at the same time the atmospherical conductor itself shows either no electricity at all, or exceeding small signs of it.

It was mentioned above, that the conducting wire must be left in contact with the metal plate for a certain time, the length of which however is not easily determined, since it depends on variable circumstances. When the conductor itself shows no signs of electricity, then it will be necessary to leave the apparatus during 8, 10, or more minutes. But if the conductor itself be capable of just attracting a very small thread, then it will be sufficient to leave the apparatus in contact as above-mentioned, for a few seconds only, in order afterwards to obtain from it very conspicuous electrical appearances.

Of the electrophorus to be used, it must be remarked, first, that its being very thin is of great importance; it having been observed, that the thinner the resinous stratum is, the greater quantity of electricity can be accumulated into the metal plate laid on it; which is the case whether the electricity is brought to it from the atmosphere, or from any other electric power. The thickness of  $\frac{1}{50}$  of an inch, or that of a common coat of varnish, is very proper; whereas if the resin was an inch thick or more, the experiments would answer very badly. Also, the surface of the resinous stratum, as well as the under surface of the metal plate, must be as plain and as smooth as possible, in order that the two surfaces may coincide more perfectly when laid on each other.

Lastly, it deserves to be repeatedly and particularly observed, that the resinous



plate, when it is to be used for our experiment, should be quite free from any the least electricity, otherwise the experiments cannot be depended on. If therefore the resinous plate has been excited before, so as to remain in some measure electrified, all possible care should be taken to deprive it of that electricity, which however is not easily done. The most effectual method of doing it, is to expose the resinous plate to the hot rays of the sun or to a fire, so that its surface may be slightly melted, by which means it will entirely lose its electricity.\* The flame of a candle, or of a piece of paper, will easily deprive the resin of its electricity, if its surface be passed over the flame. To observe whether the resinous plate is quite free from any electricity, the metal plate must be laid upon it, there it must be touched with a finger, and afterwards, being lifted up after the usual manner, it must be presented to a fine hair; for if the hair is not attracted, it may be concluded that the resinous plate has no electricity, and consequently the apparatus is fit to be used as a condenser of electricity.

Whenever the atmospherical conductor by itself gives sufficiently strong signs of electricity, then there is no occasion to use our condensing apparatus. Besides, when the electricity is strong, it often happens that part of the electricity of the metal plate is impressed on the resin, in which case the apparatus acts as an electrophorus, and consequently is unfit for our purpose. To avoid such an inconvenience, Mr. V.'s plan is to substitute to the resinous plate a plane, which should not be a perfect electric, or quite impervious to electricity, but which should be an imperfect conductor, such as might hinder, in a certain degree only, the free passage of the electric fluid through its substance. There are many conductors of this kind; as, for instance, a clean and dry marble slab, a plate of wood, likewise clean and very dry, or covered with a coat of varnish, or wax; and the like. The surface of those bodies does not contract any electricity; or if any electricity adhere to them, it vanishes soon, on account of their semi-conducting nature; for which reason they cannot answer the office of an electrophorus, and therefore are more fit to be used as condensers of electricity. On the other hand, care should be taken, in choosing the above-mentioned plane, that it be not too much of a conducting nature, or capable of becoming

\* It has been believed for a long time, that to heat, and especially to melt, sulphur and resins, was sufficient to excite in them some electricity; but except the tourmalin and some other stones, which are really excited by heat alone, the resins and sulphur never become electrified by that means, except when they have by some means or other suffered any friction. The mistake, as Bèccaria observed, was occasioned by this, viz. that even the least friction of the hand, or other body, is sufficient to excite such substances in those favourable circumstances, without which friction, those substances, melted and left to cool by themselves, are so far from acquiring any electricity, that they lose every vestige of it in case they were excited before the fusion, as may be easily proved by experiment: nor ought this to appear wonderful, since fusion or a strong degree of heat renders every body a conductor of electricity.—Orig.



so in a very short time, it being quite necessary, that the electricity should find a considerable degree of resistance in going through its substance. In choosing, or in preparing, such a plane by drying, or otherwise, it is better to render it too near to than too far from the nature of a non-conductor. A marble slab, or a board properly dried, answers admirably well, and is preferable to any other plane: otherwise the resinous plate of an electrophorus is preferable to a common table or marble slab not prepared; for these bodies, having in some measure imbibed moisture, conduct much better than is necessary.

For this purpose it is better to use a flat piece of marble, and to grind it against the metal plate, till they coincide so well as to show a sensible cohesion between them. Afterwards the piece of marble should be exposed for several days to the heat of a warmed place, such as an oven, a chimney, &c. to expel the moisture, and to render it quite fit for our experiments. The marble, thus prepared, will continue dry for a considerable time, unless it be long exposed to very damp air. As for the small quantity of moisture which the marble may accidentally and superficially attract, it may be removed by exposing it to the sun, or to a fire, or even by wiping it with a dry and clean cloth, previous to the performing of experiments. It is always advantageous to warm the marble previous to the experiment. But, instead of preparing the piece of marble by a long continued heat, it will be sufficient to give it a coat of copal varnish, or amber, or lac varnish: after which it must be kept in an oven for a short time. By this means even the worst sort of marble answers very well, even without previously warming or keeping it hot during the experiment. By means of the varnish even a metal plate may be used instead of the marble. This should be first made flat by grinding it against the upper plate, and then it must be varnished, but rather thicker than when the varnish is laid on marble. In this case both the plates might be varnished, though it is sufficient to varnish one of them.

The advantages which a varnished plate has above the common electrophorus, are, 1. That the varnish is always thinner than the common resinous stratum of an electrophorus. 2. That the varnish acquires a more smooth and plain surface; hence the metal plate may be more easily, and to more advantage adapted to it. Instead of the above-mentioned plane of marble or metal varnished, there may be substituted, with equal advantage, any sort of plane covered with dry and clean oil cloth or oil silk or sattin, or other silk stuff that is not very thick; which will answer very well, without requiring any more than perhaps a slight warming. The silk stuffs answer better for this purpose than those made of cotton or wool, and these better than linen. However, by a previous drying, and keeping them hot during the experiment, paper, leather, wood, ivory, bone, and every sort of imperfect conductor, may be made to answer to a certain de-



gree. But if those imperfectly conducting substances were dried too much, then they would become quite electrics, and consequently useless for our purpose, excepting when they were used like resins, &c.

The apparatus may be rendered more simple by applying the silk, or other semi-conducting stratum, to the upper, viz. to the metal plate, which is furnished with a glass handle, instead of the marble or other plate, which in that case becomes useless: for in its stead a plane of any kind may be used, such as a common wooden or marble table, even not very dry, a piece of metal, a book, or other conductor, whether perfect or imperfect, it being only necessary that its surface be flat. In fact, nothing more is requisite for the experiment, than that the electricity, which tends to pass from one surface to the other, should find some resistance or opposition in either of the surfaces. It is immaterial whether the non-conducting or semi-conducting stratum be laid on one or the other of the planes, it being only necessary that they should coincide very well together, which cannot be easily obtained when a common table is used for one of the planes, which is the only reason why it is better to use two planes which have been worked flat by grinding one upon the other, and one of them varnished, &c. A single metal plate, covered with silk, with 3 silk strings fastened to it by way of a handle, may be conveniently used for ordinary experiments.

Hitherto has been considered the use of the condenser in exploring the weak atmospherical electricity, which is brought down by the atmospherical conductor.\* But this, though the principal, is not the only use to which it may be applied. It serves likewise to discover the artificial electricity when this is so weak as not to be discoverable by any other means, which happens in various cases, as for instance: a Leyden phial charged, and then discharged by touching its coated sides 3 or 4 times with the discharging rod, or the hand, seems to be quite deprived of electricity; yet if you touch with the knob of it the metal plate of our condenser, when properly situated (viz. on an imperfectly conducting plane, &c.) and immediately after take up the said plate, this will be found to give very con-

\* Here it will be proper to mention a remarkable observation, which I have made on the atmospherical electricity with the help of the condenser. The late Mr. Canton and others affirmed that they had obtained stronger signs of electricity from their atmospherical apparatus at the time of an aurora borealis, than at other times; but various other philosophers doubted of the influence of electricity in that meteor, and some absolutely denied it. I myself was much in doubt about it; but at present Mr. Canton's assertion seems to be established beyond a doubt, as I have observed by actual experiment. During the strong aurora borealis, which appeared in the night of the 28th of July, 1780, the light of which rising gradually from the horizon, reached the zenith at near 11 o'clock, and enlightened the heavens with a reddish light, the weather being clear and windy; our condensing apparatus being applied to an atmospherical conductor, gave fine bright sparks; whereas, at other times, that is, in clear weather, and at every hour of the day or night, the same apparatus afforded either no sparks at all, or exceedingly small ones; the reason of which is, because the said conductor was not much elevated.—Orig.



spicuous signs of electricity; which shows that the Leyden phial is not quite deprived of electricity. But if the phial was left so far charged as just to attract a light thread, then if the metal plate were to be touched by the knob of it, even for a moment, it would afterwards, when lifted up, give a strong spark; and if then it were to be touched again by the knob of the phial, it would afford a 2d spark, hardly smaller than the former; and thus spark after spark may be obtained for a long time, which is a very surprizing experiment. 2dly. Suppose you have an electrical machine in such bad order that its conductor will not afford any spark, but will just attract a thread; then if you let this conductor touch the metal plate of the condenser, and after suffering it to continue in that situation for a few minutes, while the machine is kept in motion, lift up the metal plate, you will obtain from it a strong spark. 3dly. In case the electrical machine acts very well, but its conductor is so badly insulated, that it will not give any sparks, as when the conductor touches the walls of the room, or when a chain falls from it on the table; then if you let the said conductor in that state touch the metal plate of the condenser, while the electrical machine is in action, the plate will afterwards give sufficiently strong signs of electricity; which shows the great power this apparatus has of drawing and condensing the electricity. In short, by either of those methods you will obtain some electricity from such bodies as could hardly be expected to give any, even when they are not very dry. Indeed, coals and metals excepted, every other body will give some electricity.

It is now necessary to give an explanation of those phenomena, the theory of which will greatly facilitate the practical performance of this kind of experiments. The whole matter then may be reduced to this, viz. that the metal plate has a much greater capacity for holding electricity in one case, viz. when it lies on a proper plane, than when it stands quite insulated; as when it is suspended in the air by its silk strings or insulating handle, or when it stands on an insulating stand, as a thick stratum of resin or the like. It is easy to comprehend, that wherever the capacity of holding electricity is greater, there the intensity of electricity is proportionably less, viz. a greater quantity of electricity is in that case required, in order to raise its intensity to a given degree; so that the capacity is inversely as the intensity, or the endeavour by which the electricity of an electrified body tends to escape from all the parts of it, to which tendency or endeavour the electrical phenomena of attraction, repulsion, and especially the degree of elevation of an electrometer, correspond.

That the intensity of electricity must be inversely proportional to the capacity of the body electrified, will be clearly exemplified by the following experiment. Take 2 metal rods of equal diameter, but one of them a foot, and the other 5 feet long; and let the first be electrified so high as that the index of an electrometer annexed to it may be elevated to  $60^{\circ}$ ; then let this electrified rod touch



the other rod; then it is evident, that the intensity of the electricity, by being parted between the 2 rods, will be diminished in proportion as the capacity is increased; so that the index of the electrometer, which before was elevated to  $60^{\circ}$ , will now fall to  $10^{\circ}$ , viz. to  $\frac{1}{6}$  of the former intensity, because now the capacity is 6 times greater than when the same quantity of electricity was confined to the first rod alone. For the same reason, if the said quantity of electricity was to be communicated to a rod 60 times longer, its intensity would be diminished to 1 degree: and, on the contrary, if the electricity of this long conductor was to be contracted into the 60th part of that capacity, its intensity would be increased to  $60^{\circ}$ .

Now not only conductors of different bulk have different capacities for holding electricity, but also the capacity of the same conductor may be increased or diminished by various circumstances, some of which have not yet been properly considered. It has been observed, that the capacity of the same conductor is increased or diminished in proportion as its surface is enlarged or contracted, as is shown by Dr. Franklin's experiment of the can and chain, and various other experiments; from which it has been concluded, that the capacity of conductors is in proportion to their surface, and not to their quantity of matter. This conclusion is true, but does not comprehend the whole theory, since even the extension contributes to increase the capacity; so that, of 2 conductors, which have equal but dissimilar surfaces, that which is the more extended in length has the greater capacity. In short, it appears from all the experiments hitherto made, that the capacity of conductors is in proportion, not to the surfaces in general, but to the surfaces which are free, or uninfluenced by an homologous atmosphere. But that which comes nearer to our case is, that the capacity of a conductor, which has neither its form nor surface altered, is increased when, instead of remaining quite insulated, the conductor is presented to another conductor not insulated; and this increase is more conspicuous, according as the surfaces of those conductors are larger and come nearer to each other.

When an insulated conductor is opposed or presented to any other conductor whatever, Mr. V. calls it a conjugate conductor. The circumstance mentioned above, which augments prodigiously the natural capacity of conductors, is that which has been hitherto principally overlooked: but let us (says Mr. V.) begin with those experiments which show this increased capacity in the simplest manner. I take, for example, the metal plate of an electrophorus, and holding it by its insulating handle in the air, electrify it so high that the index of an electrometer annexed to it might be elevated to  $60^{\circ}$ ; then lowering this metal plate by degrees towards a table or other conducting plain surface, I observe that the index of the electrometer falls gradually from  $60^{\circ}$  to  $50^{\circ}$ ,  $40^{\circ}$ ,  $30^{\circ}$ , &c. Notwithstanding this appearance, the quantity of electricity in the plate remains the



same, except the plate be brought so near the table as to occasion a transmission of the electricity from the former to the latter; at least the quantity of electricity will remain as much the same as the dampness of the air, &c. will permit. The decrease therefore of intensity is owing to the increased capacity of the plate, which now is not insulated solitary but conjugate. In proof of this proposition, if the plate be removed gradually farther and farther from the table, it will be found, that the electrometer rises again to its former station, namely to  $60^{\circ}$ , excepting the loss of that quantity of electricity, which during the experiment must have been more or less imparted to the air, &c.

The reason of this phenomenon is easily derived from the action of electric atmospheres. The atmosphere of the metal plate, which for the present I shall suppose to be electrified positively, acts on the table or other conductor to which it is presented; so that the electric fluid of the table, agreeably to the known laws, retiring to the remoter parts of it, becomes more rare in those parts which are exposed to the metal plate, and this rarefaction becomes greater the nearer the electrified metal plate is brought to the table. If the metal plate is electrified negatively, then the contrary effects must take place. In short, the parts immersed into the sphere of action of the electrified metal plate, contract a contrary electricity, which accidental electricity, making in some manner a compensation for the real electricity of the metal plate, diminishes its intensity, as is shown by the depression of the electrometer.

The 2 following experiments will throw more light on the reciprocal action of the electric atmospheres. First, suppose 2 flat conductors, electrified both positively or both negatively, to be presented towards, and to be gradually brought near, each other: it will appear, by 2 annexed electrometers, that the nearer those 2 conductors come to each other, the more their intensities will increase; which shows, that either of the 2 conjugate conductors has a much less capacity now, than when it was singly insulated, and out of the influence of the other. This experiment explains the reason why an electrified conductor will show a greater intensity when it comes to be contracted into a smaller bulk; and also why a long extended conductor will show a less intensity than a more compact one, supposing their quantity of surface and of electricity to be the same; because the homologous atmospheres of their parts interfere less with each other in the former than in the latter case. Secondly, Let the preceding experiment be repeated with this variation only, viz. that one of the flat conductors be electrified positively, and the other negatively: the effects then will be just the reverse of the preceding, viz. the intensity of their electricities will be diminished, because their capacities are increased the nearer the conductors come to each other.

This matter may be rendered still more clear by insulating the conducting



plane, while the other electrified plate is on it, and afterwards separating them; for then both the metal plate and the conducting plane, which may be called the inferior plane, will be found electrified, but possessed of contrary electricities, as may be ascertained by electrometers. If the inferior plane be insulated first, and then the electrified plate brought over it, then the latter will cause an endeavour in the former to acquire a contrary electricity, which however the insulation prevents from taking place; hence the intensity of the electricity of the plate is not diminished, at least the electrometer will show a very little and almost imperceptible depression, which small depression is owing to the imperfection of the insulation of the inferior plane, and to the small rarefaction and condensation of the electric fluid, which may take place in different parts of the said inferior plane. But if in this situation the inferior plane be touched, so as to cut off the insulation for a moment, then it will immediately acquire the contrary electricity, and the intensity in the metal plate will be diminished.

If the inferior plane, instead of being insulated, were itself a non-conducting substance, then the same phenomena would happen, viz. the intensity of the electrified metal plate laid on it would not be diminished. This however is not always the case; for if the inferior non-conducting plane be very thin, and be laid on a conductor, then the intensity of the electrified metal plate will be diminished, and its capacity will be increased by being laid on the thin insulating stratum; because in that case the conducting substance, which stands under the non-conducting stratum, acquiring an electricity contrary to that of the metal plate, will diminish its intensity, &c. and then the insulating stratum will only diminish the mutual action of the two atmospheres more or less, according as it keeps them more or less asunder.

The intensity or electric action of the metal plate, which diminishes gradually as it is brought nearer and nearer to a conducting plane not insulated, becomes almost nothing when the plate is nearly in contact with the plane, the compensation or accidental balance being then almost perfect. Hence, if the inferior plane only opposes a small resistance to the passage of the electricity (whether such resistance is occasioned by a thin electric stratum, or by the plane's imperfect conducting nature, as is the case with dry wood, marble, &c.) that resistance joined to the interval, however small, that is between the two planes, cannot be overcome by the weak intensity of the electricity of the metal plate, which on that account will not dart any spark to the inferior plane (except its electricity were very powerful, or its edges not well rounded) and will rather retain its electricity; so that, being removed from the inferior plane, its electrometer will nearly recover its former height. Besides, the electrified plate may even come to touch the imperfectly conducting plane, and may remain in that situation for



some time; in which case the intensity being reduced almost to nothing, the electricity will pass to the inferior plane exceeding slowly.

But the case will not be the same if, in performing this experiment, the electrified metal plate be made to touch the inferior plane edgewise; for then its intensity being greater than when laid flat, as appears by the electrometer, the electricity easily overcomes the small resistance, and passes to the inferior plane, even across a thin stratum;\* because the electricity of the one plane is balanced by that of the other, only in proportion to the quantity of surface which they oppose to each other within a given distance; and when the metal plate touches the other plane in flat and ample contact its electricity is not dissipated. This apparent paradox is clearly explained by the theory of electric atmospheres.

Hitherto we have considered in what manner the action of electric atmospheres must modify the electricity of the metal plate in its various situations. We must now consider the effects which take place when the electricity is communicated to the metal plate while standing on the proper plane. The whole business having been proved in the preceding pages, it is easy to deduce the applications from it; yet it will be useful to exemplify it by an experiment. Suppose that a Leyden phial, or a conductor, were so weakly electrified that the intensity of its electricity was only of half a degree or even less: if the metal plate of our apparatus, when standing on the proper plane, was to be touched with that phial or conductor, it is evident, that either of them would impart to it a quantity of its electricity, proportional to the plate's capacity, viz. so much of it as should make the intensity of the electricity of the plate equal to that of the electricity in the conductor or phial, suppose of half a degree; but the plate's capacity, now that it lies on the proper plane, is above 100 times greater than if it stood insulated in the air, or, which is the same thing, it requires 100 times more electricity in order to show the same intensity; therefore, in this case it must require upwards of 100 times more electricity from the phial or

\* This explanation, properly applied, renders evident the actions of points in general. Properly speaking, a pointed conductor, not insulated, when presented to an electrified body, has not in itself any particular virtue of attracting electricity. It acts only like a conductor not insulated, which does not oppose any resistance to the passage of the electric fluid. If the same conductor, instead of being pointed, was to present a globular or flat surface, to the electrified body, neither would it in that case oppose a greater resistance to the passage of the electricity. But the reason why the electricity will not pass nearly so easily from the electrified body to the conductor when it is flat or globular, as when it is pointed, is because in the former case the intensity of the electricity in the electrified body is weakened by the opposed flat surface, which, acquiring the contrary electricity, compensates the diminished intensity incomparably more than a point can. It appears, therefore, that it is not the particular property of a point or of a flat surface, but the different state of the electrified body, that makes it part with its electricity easier, and from a greater distance, when a pointed conducting substance, than when a flat or globular one is presented to it.—Orig.



conductor. It naturally follows, that when the metal plate is afterwards removed from the proper plane, its capacity being lessened so as to remain equal to the 100th part of what it was before, the intensity of its electricity must become of  $50^{\circ}$ ; since, agreeably to the supposition, the intensity of the electricity in the phial or conductor was of half a degree.

A conductor that is electrified while it stands in full and ample contact with another proper conductor, as above specified, and is afterwards separated from it, shows the same phenomena that are exhibited by a conductor, which, after being electrified, is contracted into a smaller bulk, or contrarywise, like Dr. Franklin's experiment of the can and chain. If a small quantity of electricity applied to the metal plate of the condenser enables it to give a strong spark, it may be asked, what would a great quantity of electricity do? The answer is, that it would do nothing more, because, when the electricity communicated to the metal plate is so strong as to overcome the small resistance of the inferior plane, it will be dissipated.

After all that has been said in the preceding pages, it may be easily understood, that if the metal plate of our condenser can receive a good share of electricity from a Leyden phial, or from an ample conductor, however weakly electrified; it cannot receive any considerable quantity of it from a conductor of a small capacity; for this conductor cannot give what it has not, except it were continually receiving a stream, however small, of electricity, as is the case with an atmospherical conductor, or with a prime-conductor of an electrical machine, which acts very poorly, but continues in action. In those cases it has been observed above, that a considerable time is required before the metal plate has acquired a sufficient quantity of electricity.

As an ample conductor, weakly electrified, imparts a considerable quantity of electricity to the metal plate of our condenser, so that when the said metal plate is afterwards separated from its proper plane, the electricity in it appears much condensed and vigorous; so when the same metal plate contains a small quantity of electricity, and such as cannot give a spark or affect an electrometer, that electricity may be rendered very conspicuous by communicating it to another small metal plate or condenser.

Mr. Cavallo was the first who thought of this improvement, which he derived by reasoning on my experiments. He actually made a small metal plate not exceeding the size of a shilling: this 2d condenser is certainly of great use in many cases, in which the electricity is so small as not to be at all, or not clearly, observable by my method or a first condenser only, as has been evidently proved by some experiments we made together. Sometimes the usual metal plate of my condenser acquired so small a quantity of electricity, that being afterwards taken up from the inferior plane, and presented to an extremely sen-



sible electrometer of Mr. Cavallo's construction, it did not affect it. In this case, when the metal plate, thus weakly electrified, was made to touch the other small plate properly situated, and that was afterwards brought near an electrometer, the electricity was then generally stronger than what would have been sufficient to ascertain its quality. Now, if by the help of both condensers the intensity of the electricity has been augmented 1000 times, which is by no means an exaggeration, how weak must then be the electricity of the body examined? how small must that electricity be which is produced by rubbing a piece of metal with one's hand, since when this electricity is condensed by both condensers, and then is communicated to an electrometer, it can hardly affect that instrument? Yet it is sufficient to afford conviction, that the metal can be electrified by the friction of a person's hand. Some years ago, viz. before the discovery of our condenser, and of Mr. Cavallo's sensible electrometer, we were very far from being able to discover such weak excitations; whereas at present we can observe a quantity of electricity incomparably smaller than the smallest observable at those times.

*Appendix.*—I mentioned, that after various attempts I at last succeeded in obtaining undoubted signs of electricity from the simple evaporation of water, and from various chemical effervescences; but as this is a fact not less interesting than new, it seems proper to subjoin in this place a faithful account of the experiments made for that purpose. The first set of experiments were made at Paris, in company with Mr. Lavoisier and Mr. De la Place, two intelligent philosophers and members of the Royal Academy of Sciences. After I had shown them my experiments with my condenser, they, as well as myself, began to entertain hopes of succeeding in the experiments on the evaporation, &c. Accordingly Mr. Lavoisier ordered a larger condenser with a marble plane to be made. The first experiment I attempted with this instrument, in company with Mr. De la Place, proved unsuccessful; but the weather at that time was bad, the room was narrow and full of vapours, and the apparatus was not quite in proper order. Mr. De la Place and Mr. Lavoisier repeated those experiments in the country, and then they were attended with success, which incited us to repeat and diversify the experiments, by which means the discovery was completed; having obtained unequivocal signs of electricity from the evaporation of water, from the simple combustion of coals, and from the effervescence of iron filings in diluted vitriolic acid. This observation was made the 13th of April of the present year 1782, and the experiments were performed in the following manner. In an open garden a long metal plate was insulated, which, by means of a large iron wire, was made to communicate with the metal plate of the condenser laid on the piece of marble, which was kept continually warm by some lighted coals set underneath. This done, some chafing dishes, containing burning charcoal, were placed on



the large insulated plate. The combustion of the coals was helped by a gentle wind. Some minutes after, the iron wire, by which the large insulated plate was connected with the metal plate of the condenser, was taken off; then the metal plate being removed from the marble by its insulated handle, and presented to Mr. Cavallo's electrometer, it made the balls of it diverge with negative electricity. The experiment was repeated by placing on the large insulated plate 4 vessels, containing iron filings and water, instead of the chafing dishes: then some vitriolic acid was poured into those 4 vessels, sufficient to cause a vigorous effervescence, and when the strongest ebullition was going to subside, the metal plate of the condenser was removed from over the marble; and being examined, not only electrified the electrometer with negative electricity, but gave a sensible spark. At this time having tried to obtain electricity from the evaporation of water, the effects were equivocal or hardly sensible; the same thing happened a few days after, when however we obtained clear signs of electricity from those effervescences, which produce fixed and nitrous air. One day the electricity arising from the evaporation of water seemed to be positive; but subsequent experiments, and other circumstances, indicate that such a phenomenon must be attributed to a mistake.

The experiment on the evaporation of water, which did not answer so well at Paris, succeeded much better in London, where I bethought me of throwing water on the lighted coals, which were kept in an insulated chafing dish. In this manner the electricity of the evaporation never fails to electrify the chafing dish negatively, and strongly enough for the electricity to be discovered by the simple electrometer; it will even afford a spark, if the condenser be used. Another time this experiment was repeated with success at Mr. Cavallo's, in the following manner. A small crucible, containing 3 or 4 small coals lighted, was insulated; then a spoonful of water was thrown on the coals, and immediately after an electrometer, which communicated with the coals by means of a wire, diverged with negative electricity.

The experiments hitherto made, though not numerous, yet concur to show, that the vapours of water, and in general the parts of all bodies, that are separated by volatilization, carry away an additional quantity of electric fluid as well as of elementary heat, and consequently that those bodies, from the contact of which the volatile particles have been separated, remain both cooled and electrified negatively; from which it may be deduced, that whenever bodies are resolved into a volatile elastic fluid, their capacity for holding electric fluid is augmented, as well as their capacity for holding common fire, or the calorific fluid. This is a striking analogy by which the science of electricity throws some light on the theory of heat, and alternately derives light from it; I mean on the doctrine of latent or specific heat, the first notions of which were suggested by the admirable ex-



periments of Dr. Black and Wilke, and which has been afterwards much elucidated by Dr. Crawford, who followed the experiments of Dr. Irwin. By following this analogy, it seems that, as the vapours on their condensing lose part of their latent heat, on account of their capacity being diminished, so they part with some electric fluid. Hence originates the positive electricity, which is always more or less predominant in the atmosphere, when the sky is clear, viz. at that height where the vapours begin to be condensed. Accordingly, the atmospheric electricity is stronger in fogs, in which case the vapours are more condensed, so as to be almost reduced into drops, and is still stronger when thick fogs become clouds.

Hitherto we have accounted for the positive atmospheric electricity; but it is easy to account for clouds negatively electrified; for when a cloud, positively electrified, has been once formed, its sphere of action is extended a great way round, so that if another cloud comes within that sphere, its electric fluid, agreeably to the well known laws of electric atmospheres, must retire to the parts of it which are the most remote from the first cloud; and from thence the electric fluid may be communicated to other clouds, or vapours, or terrestrial prominences. Thus a cloud may be electrified negatively, which cloud, after the same manner, may occasion a positive electricity in another cloud, &c. This explains, not only the negative electricity, which is often obtained from the atmosphere in cloudy weather; and the frequent changes from positive to negative electricity, and contrarywise in stormy weather; but also the waving motion often observed in the clouds, and the hanging down of them, so as nearly to touch the earth. After the fore-mentioned discoveries we need no longer wonder at the appearance of lightnings in the eruptions of volcanos, as was particularly observed in the late dreadful eruption of Mount Vesuvius. The few experiments I have made show, that the quantity of smoke, but much more the rapidity with which it is produced, tends to increase the electricity which arises from combustion, &c. How great must then be the quantity of electricity that is produced in such eruptions!



*XVII. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1780. By Thomas Barker, Esq. p. 281.*

		Barometer.			Thermometer.						Rain.
		Highest.	Lowest.	Mean.	In the House.			Abroad.			
					Hig.	Low	Mean	Hig.	Low	Mean	
Jan.	Morn.	30.15	28.55	29.45	46½	32	38	47	21	32	2.264
	Aftern.				46	33	38	50½	25	36	
Feb.	Morn.	29.82	28.32	29.29	46½	38	42½	45	30	37½	1.664
	Aftern.				47	39	43	52½	36½	44	
Mar.	Morn.	30.01	29.46	29.74	51	40	46	44½	30	38	0.160
	Aftern.				53½	42	47½	56½	40	48	
Apr.	Morn.	29.88	28.92	29.43	57	41	50	53	33½	43	1.938
	Aftern.				58	43	51½	64	39	54	
May	Morn.	29.95	29.20	29.60	66	47	54	64	38	47	0.974
	Aftern.				70	48	55	76½	47½	59½	
June	Morn.	29.88	29.00	29.40	69	58	62	69	52	60	2.958
	Aftern.				73	59	63½	79½	57	67	
July	Morn.	29.96	29.22	29.59	68	61	64	67	49	58½	1.683
	Aftern.				71½	62	66	79½	62	70½	
Aug.	Morn.	29.89	29.02	29.45	70½	60	65	64	49½	57	1.097
	Aftern.				72½	61	66½	80	61	71	
Sept.	Morn.	29.86	29.04	29.45	65	50½	59	63	38	52	4.004
	Aftern.				67	51	60	80	50½	63	
Oct.	Morn.	30.00	28.96	29.67	59	44	52½	59	26½	44	0.081
	Aftern.				61	44	53	66	41	53½	
Nov.	Morn.	29.74	28.49	29.22	51	40½	45	52	28	39	2.296
	Aftern.				51½	41	46	54	35	45	
Dec.	Morn.	29.68	29.00	29.34	50	37½	43½	51	28½	39	1.703
	Aftern.				51	37	44	53	33½	42½	
Mean of all .....				29.47	52			50			20.822

*XVIII. Meteorological Journal kept at the House of the Royal Society, for the Year 1781. By Order of the President and Council. p. 285.*

A summary of the whole is as follows.

1781.	Thermometer without.			Thermometer within.			Barometer.			Rain. Inches.
	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	Greatest Height.	Least Height.	Mean Height.	
Jan..	51.5	25.0	38.5	49.0	32.0	35.9	30.55	29.04	29.90	1.348
Feb..	52.5	31.0	42.8	51.0	36.0	45.7	30.34	28.95	29.65	1.676
Mar.	60.5	32.0	44.8	53.5	35.0	45.9	30.47	29.88	30.21	0.292
April	66.5	37.0	49.2	67.0	38.0	52.8	30.34	29.29	29.88	0.650
May.	80.5	42.0	56.8	76.0	44.0	56.8	30.36	29.58	30.05	0.619
June.	84.0	53.5	66.2	79.5	55.0	66.8	30.42	29.42	29.84	0.688
July.	84.0	57.0	68.4	80.0	53.5	68.7	30.44	29.69	30.05	1.045
Aug.	82.0	52.0	67.7	78.0	61.0	69.2	30.39	29.54	29.95	2.198
Mean of 8 months.			53.0			55.2			29.95	8.516



*XIX. An Attempt to make a Thermometer for measuring the Higher Degrees of Heat from a Red Heat up to the Strongest that Vessels made of Clay can support. By Josiah Wedgwood.\** p. 305.

A measure for the higher degrees of heat, such as the common thermometers afford for the lower ones, would be an important acquisition, both to the philosopher and the practical artist. The latter must feel the want of such a measure on many occasions; particularly when he attempts to follow, or apply to use, the curious experiments of Mr. Pott, related in his *Lithogeognosia*, and other modern writers on similar subjects. When we are told, for instance, that such and such materials were changed by fire into a fine white, yellow, green, or other coloured glass: and find that these effects do not happen, unless a particular degree of fire has fortunately been hit upon, which degree we cannot be sure of succeeding in again:—when we are disappointed, by having the result at some times an unvitrified mass, and at others an over-vitrified scoria, from a little deficiency or excess of heat:—when we see colours altered, not only in shade but

\* This ingenious and respectable gentleman died the 3d of Jan. 1795, at Etruria, in Staffordshire, at 64 years of age. Mr. W. was the younger son of a potter. He derived little or no property from his father; but became, by his elegant manufactories, the maker of his own fortune, and the benefactor of his country to an incalculable extent. His many discoveries of new kinds of earthen wares and porcelains; his studied forms and chaste style of decoration; and the correctness and judgment with which all his works were executed, under his own eye, and mostly by artists of his own forming, completely turned the current of this branch of commerce in favour of England, which before imported the finer earthen wares from the Continent. But by Mr. W.'s ingenious endeavours, and through his example, this country has ever since exported such wares to a great annual amount; the whole of which is drawn from the earth, and from the industry of the inhabitants; while the national taste has been improved, and its reputation greatly raised in foreign countries. Mr. W. was also commendably known in the walks of philosophy: besides his ingenious mechanical contrivances, and philosophical arrangements and operations, through which his private manufactory had the effect of a public work of experiments; his communications to the R. S. (of which learned body he became a member about the year 1784,) show a mind enlightened by science, and contributed to procure him the esteem of all scientific men. Besides the above paper, other ingenious and useful communications of his appear in the *Phil. Trans.* volumes 73, 74, 76, and 80. Mr. W. was the proposer of the Grand Trunk Canal, and the chief agent in obtaining the act of parliament for making it, against the prejudices of the landed interest, then very powerful. That canal, 90 miles in length, unites the rivers Trent and Mersey; branches have been made from it to the Severn, to Oxford; and to many other parts; and it has also a communication with the Grand Junction Canal from Braunston to Brentford, &c.—Mr. W. having very honourably acquired a large fortune, his purse was always open to the calls of charity, and to the support of every institution for the public good. To his relations, friends, and neighbours, he was endeared by his many private virtues; and he has been deeply regretted by his country, as the able and zealous supporter of her commerce, and the steady patron of every valuable interest of society. Mr. W.'s merits have also otherwise contributed to the benefit of futurity; having reared and left behind him a family, whose virtues and useful ingenuity do so much honour to his paternal care in their education. Other particulars of Mr. W.'s life and works may be seen in the *Gentleman's Magazine* for the year 1795, vol. 65, p. 84.



in kind, and in many cases destroyed, by a small augmentation of the heat which had produced them; insomuch, that in the gradual increase of the fire, a precise moment of time must be happily seized, in order to catch them in perfection:—and when inconveniences, similar to these, arise in operations by fire on metals and other substances:—how much is it to be wished, that the authors had been able to convey to us a measure of the heat made use of in their valuable processes!

In a long course of experiments, for the improvement of the manufacture I am engaged in, some of my greatest difficulties and perplexities have arisen from not being able to ascertain the heat to which the experiment-pieces had been exposed. A red, bright red, and white heat, are indeterminate expressions; and even though the 3 stages were sufficiently distinct from each other, they are of too great latitude; as the brightness or luminousness of fire increases, with its force, through numerous gradations, which can neither be expressed in words, nor discriminated by the eye. Having no other resource, I have been obliged to content myself with such measures as my own kilns and the different parts of them afforded. Thus the kiln in which our glazed ware is fired furnishes 3 measures, the bottom being of one heat, the middle of a greater, and the top still greater: the kiln in which the biscuit ware is fired furnishes 3 or 4 others, of higher degrees of heat; and by these I have marked my registered experiments. But though these measures had been fully adequate to my own views, which they were not, it is plain, that they could not be communicated to others; that their use is confined to a particular structure of furnaces, and mode of firing; and that, on any alteration in these, they would become useless and unintelligible, even where now they are best known. And indeed, as this part of the operation is performed by workmen of the lowest class, we cannot depend on any great accuracy even in one and the same furnace. It has accordingly often happened, that the pieces fired in the top of the kiln in one experiment, have been made no hotter than those fired in the middle in another, and vice versa.

The force of fire, in its higher as well as lower stages, can no otherwise be justly ascertained than by its effects on some known body. Its effect in changing colours has already been hinted at; and I have observed compositions of calces of iron with clay to assume, from different degrees of fire, such a number of distinct colours and shades, as promised to afford useful criteria of the respective degrees. With this idea, I prepared a quantity of such a composition, and formed it into circular pieces, about an inch in diameter, and a quarter of an inch thick. A number of these were placed in a kiln, in which the fire was gradually augmented, with as much uniformity and regularity as possible, for near 60 hours; the pieces taken out at equal intervals of time during the succes-



sive increase of heat, and piled in their order on each other in a glass tube, exhibited a regular and pretty extensive series of colours; from a flesh-colour to a deep brownish red, thence to a chocolate, and so on to nearly black, with all the intermediate tints between these colours. A rack being fixed to the tube, like the scale of a thermometer, and the numbers of the pieces marked on it respectively opposite to them, it is obvious, that these numbers may be considered as so many thermometric divisions or degrees; and that, if another piece of the same composition be fired in any other kiln or furnace, not exceeding the utmost heat of the first, it will acquire a colour corresponding to some of the pieces in the tube, and thus point out the degree of heat which that piece, and consequently such other matters as were in the fire along with it, have undergone.

It must however be confessed, that for general use a thermometer on this principle is liable to objection, as ideas of colours are not perfectly communicable by words; nor are all eyes, or all lights, equally adapted for distinguishing them, especially the shades which approach near to each other; and the effects of phlogistic vapours, in altering the colour, may not in all cases be easily guarded against.

In considering this subject attentively, another property of argillaceous bodies occurred to me; a property which obtains, in a greater or less degree, in every kind of them that has come under my examination, so that it may be deemed a distinguishing character of this order of earths: I mean, the diminution of their bulk by fire; I have the satisfaction to find, in a course of experiments lately made with this view, that it is a more accurate and extensive measure of heat than the different shades of colour. I have found, that this diminution begins to take place in a low red heat; and that it proceeds regularly, as the heat increases, till the clay becomes vitrified, and consequently to the utmost degree that crucibles, or other vessels made of this material, can support. The total contraction of some good clays, which I have examined, in the strongest of my own fires, is considerably more than  $\frac{1}{4}$  part in every dimension.

If, therefore, we can procure at all times a clay sufficiently apyrous or unvitrescible, and always of the same quality in regard to contraction by heat; and if we can find means of measuring this contraction with ease and minute accuracy, I flatter myself, that we shall be furnished with a measure of fire sufficient for every purpose of experiment or business. We have, in different parts of England, immense beds of clay; each of which, at equal depths, is pretty uniform in quality throughout its whole extent. Of all the sorts I have hitherto tried, some of the purest Cornish porcelain clays seem the best adapted, both for supporting the intensity, and measuring the degrees, of fire. For preparing and applying this material to thermometric purposes, the following method is



proposed: the clay is first to be washed over, and while in a dilute state passed through a fine lawn. Let it then be made dry, and put up in boxes.\*

The dry clay is to be softened, for use, with about  $\frac{2}{5}$  of its weight of water; and formed into small pieces, in little moulds of metal,  $\frac{1}{10}$  of an inch in breadth, with the sides pretty exactly parallel, this being the dimension intended to be measured, about  $\frac{1}{10}$  of an inch deep, and 1 inch long. To make the clay deliver easily, it will be necessary to oil the mould, and make it warm. These pieces, when perfectly dry, are put into another iron mould or gage, consisting only of a bottom, with two sides, half an inch deep; to the dimensions of which sides the breadth of the pieces is to be pared down.

For measuring the diminution which they are to suffer from the action of fire, another gage is made, of two pieces of brass, 24 inches long, with the sides exactly straight, divided into inches and tenths, fixed half an inch asunder at one end, and  $\frac{3}{10}$  at the other, on a brass plate; so that one of the thermometric pieces, when pared down in the iron gage, will just fit to the wider end. Let us suppose this piece to have diminished in the fire  $\frac{1}{5}$  of its bulk, it will then pass on to half the length of the gage; if diminished  $\frac{2}{5}$  it will go on to the narrow-end; and in any intermediate degree of contraction, if the piece be slid along till it rests against the converging sides, the degree at which it stops will be the measure of its contraction, and consequently of the degree of heat it has undergone.

These are the outlines of what appears to me necessary for the making and using of this thermometer; and it is hoped, that the whole process will be found sufficiently simple, and easy of execution. It may however be proper to take notice of a few minuter circumstances, and to mention some observations which occurred in the progress of the inquiry. 1. There ought to be a certainty of the clay being easily, and at all times, procurable in sufficient quantity, and on moderate terms. That this is the case with the clay here made choice of, will be evident to every one acquainted with the natural history of Cornwall, where there are beds of this clay inexhaustible, and in too many hands to be monopolized. If this should not prove satisfactory, the author offers to this illustrious Society, and will think himself honoured by their acceptance of, a sufficient space in a bed of this clay to supply the world with thermometer-pieces for numerous ages; and he does not apprehend, that any greater inconveniences can arise to foreign artists or philosophers, from their being supplied with clay for these thermometers from this spot only, than what we now feel from being sup-

\* While the clay is thus kept dry in boxes, as well as while it continues in its natural bed, it is secure from alterations in quality, which clays in general are subject to undergo, when exposed, for a long course of years, to the joint actions of air and moisture.—In the lawns I made use of, the interstices were each less than the 100,000 parts of an inch.—Orig.



plied with mercury for the common thermometers from the Spanish or Hungarian mines.

2. We ought to be assured also, that all the clay made use of for these thermometers, is perfectly similar. For this purpose, it will be best to dig it out of the earth in considerable quantity at once, an extent of some square feet or yards in area, and to the depth of 6 or 7 yards or more from the surface, and to mix the whole thoroughly together, previous to the further preparation already mentioned. When the first quantity is exhausted, another perpendicular column may be dug from the same bed, close to the first, to the same depth, and prepared in the same manner; by which means we may be assured of its similarity with the former parcel, and that it will diminish equally in the fire.

3. This clay, dried by the summer heat, or in a moderately warm room, or with more heat before a fire, has not been observed to differ in degree of dryness. After being so dried, it loses about a 10th part of its weight in the heat of boiling water, about as much more in that of melted lead, and from thence to a red heat 10 parts, in all  $\frac{1}{10}$ . Each of these heats soon expels from the clay its determinate quantity of matter, chiefly air; after which, the same heat, though continued for many hours, has no further effect. I had some hopes, that the graduation of the common thermometer might be continued, on this principle, up to the red heat at which the shrinking of the clay commences, so as to connect the 2 thermometers together by one series of numbers; but the loss of weight appears not to be sufficiently uniform, or proportional to the degree of heat, to answer that purpose; for it was found to go on quicker, and bladders tied to the mouths of the vessels in which the pieces were heated, became more rapidly distended, at the commencement of redness than at any other time. From low red heat to a strong one, such as copper melts in, the loss of weight was only about 2 parts in 100; though the difference between these 2 heats appears to be much greater than what the same loss corresponds to in the lower stages. After this period, the decrease of weight entirely ceased. The vapours expelled from the clay, caught separately in the different degrees of heat, seemed, from the few trials made with them, to consist of common air mixed with fixed air. They all precipitated lime-water; that which was first extricated, exceeding weakly; the others more and more considerably; but the last not near so strongly as the air expelled from lime-stone in burning. None of them were inflammable.

The thermometric pieces may be formed much more expeditiously than in the single mould, by means of an instrument used for similar purposes by the potters. It consists of a cylindrical iron vessel, with holes in the bottom, of the form and dimensions required. The soft clay, put in the vessel, is forced by a press down through these apertures, in long rods, which may be cut while moist, or broken



when dry, into pieces of convenient lengths. It was hoped, that this method would of itself have been sufficient, without the addition of the paring gage, making proper allowance, in the size of the holes, for the shrinking of the clay in drying. But it was found, that a variety of little accidents might happen to alter the shape and dimensions of the pieces, in a sensible degree, while in their soft state; so that it will be always safest to have recourse to the paring gage, for ascertaining and adjusting their breadth when perfectly dry, this being the period at which the pieces are exactly alike with regard to their future diminishing; so that if they are now reduced to the same breadth, we may be sure that they will suffer equal contractions from equal degrees of heat afterwards, whether they have been made in a mould, or by a press, or in any other way; neither is any variation in the length or thickness of these pieces of the least consequence, provided one of the dimensions, that by which they are afterwards to be measured, is made accurate to the gage.

5. It will be proper to bake the pieces, when dry, with a low red heat, in order to give them some firmness or hardness, that they may, if necessary, be able to bear package and carriage; but more especially to prepare them for being put into an immediate heat, along with the matters they are to serve as measures to, without bursting or flying, as unburnt clay would do. We need not be solicitous about the precise degree of heat employed in this baking, provided only that it does not exceed the lowest degree which we shall want to measure in practice; for a piece that has suffered any inferior degrees of heat, answers as well for measuring higher ones as a piece which has never been exposed to fire at all. In this part of the preparation of the pieces, it may be proper to inform the operator of a circumstance, which, though otherwise immaterial, might at first disconcert him: if the heat be not in all of them exactly equal, he will probably find, that while some have begun to shrink, others are rather enlarged in their bulk; for they all swell a little just on the approach of redness. As this is the period of the most rapid produce of air, the extension may perhaps be owing to the air having at this moment become elastic to such a degree, as to force the particles of the clay a little asunder before it obtains its own enlargement.

6. Each division of the scale, though so large as a 10th of an inch, answers to  $\frac{1}{80}$  part of the breadth of the little piece of clay. We might go to much greater nicety, either by making the divisions smaller, or the scale longer; but it is not apprehended that any thing of this kind will be found necessary: and indeed, in proceeding much further in either way, we may possibly meet with inconveniences sufficient to counter-balance the apparent additional accuracy of measurement.

7. The divisions of this scale, like those of the common thermometers, are



unavoidably arbitrary; but the method here proposed appears sufficiently commodious and easy of execution, the divisions being adjusted by measures everywhere known, and at all times obtainable: for however the inches used in different countries may differ in length, this cannot affect the accuracy of the scale, provided the proportions between the wider and narrower end of the gage are exactly as  $\frac{5}{10}$  of those inches to  $\frac{3}{10}$ , and the length 240 of the same 10ths; and that the pieces in their perfectly dry state, before firing, fit precisely to the wider end. When one gage is accurately adjusted to these proportional measures, 2 pieces of brass should be made, one fitting exactly into one end, and the other into the other; these will serve as standards for the ready adjustment of other gages to the dimensions of the original. By this simple method we may be assured that thermometers on this principle, though made by different persons, and in different countries, will all be equally affected by equal degrees of heat, and all speak the same language: the utility of this last circumstance is now too well known to need being insisted on.

8. If a scale 2 feet in length should be reckoned inconvenient, it may be divided into 2, of 1 foot each, by having 3 pieces of brass fixed on the same plate; the 1st and 2d,  $\frac{5}{10}$  of an inch apart at one end, and  $\frac{4}{10}$  at the other; the 2d and 3d,  $\frac{4}{10}$  at one end, and  $\frac{3}{10}$  at the other; so that the first reaches to the 120th division, and the 2d from thence to the 240th.

9. As this thermometer, like all others, can express only the heat felt by itself, the operator must be careful to expose the pieces to an equal action of the fire with the body whose heat he wants to measure by them. In kilns, ovens, reverberatories, under a muffle, and wherever the heat is pretty steady and uniform, the means of doing this are too obvious to need being mentioned. But in a naked fire, where the heat is necessarily more fluctuating, and unequal in different parts of the fuel, some precaution will be required. The thermometer-piece may generally be put into the crucible, along with the subject-matter of the experiment. But where the matter is of such a kind as to melt and stick to it, the piece may be previously inclosed in a little case made of crucible clay. The smallness of the pieces will admit of this being done without inconvenience, at least in any but the smallest crucibles, as the pieces themselves may be diminished to any size that may be found proper, provided only that one of the dimensions,  $\frac{3}{10}$  of an inch, be preserved as mentioned in obs. 4. For the very smallest sort of crucibles, the case may be put in close to the crucible, so as to form as it were an addition to its bulk on the outside. If it be asked, why the case is not always thus put in by the side of the crucible? it is answered, that in judging of the heat of large crucibles from a thermometer-piece placed on the outside of them, we may sometimes be deceived, as the piece in its little



case has been found to heat sooner than the matter in the larger vessel; but in small ones, as the crucible and case are nearly alike in bulk, there is little danger of error from this cause.

10. These thermometer-pieces possess some singular properties, which we could not have expected to find united in any substance whatever, and which peculiarly fit them for the purposes they are here applied to. 1st. When baked by only moderate degrees of fire, though they are, like other clays, of a porous texture, and imbibe water; yet, when saturated with the water, their bulk continues exactly the same as in a dry state. 2d. By very strong fire, they are changed to a porcelain or semi-vitreous texture; yet their contraction, on further augmentations of the heat, proceeds regularly as before, up to the highest degree of fire that I have been able to produce. 3d. They bear sudden alternatives of heat and cold; may be dropped at once into intense fire, and, when they have received its heat, may be plunged as suddenly into cold water, without the least injury from either. 4th. Even while saturated with water in their porous state, they may be thrown immediately into a white heat, without bursting or suffering any injury. 5th. Sudden cooling, which alters both the bulk and texture of most bodies, does not at all affect these, at least not in any quality subservient to their thermometric uses. 6th. Nor are they affected by long continuance in, but solely by, the degree of heat they are exposed to. In 3 minutes or less, they are perfectly penetrated by the heat which acts on them, so as to receive the full contraction which that degree of heat is capable of producing; equally with those which had undergone its action during a gradual increase of its force for many hours. Strong degrees of heat are communicated to them with more celerity than weak ones: perhaps the heat may be more readily transmitted, in proportion as the texture becomes more compact.

These facts have been ascertained by many experiments, the particulars of which are omitted, because they would swell this paper much beyond the bulk intended.

11. The use and accuracy of this thermometer for measuring, after an operation, the degree of heat which the matter has undergone, will be apparent. The foregoing properties afford means of measuring it also, easily and expeditiously, during the operation, so that we may know when the fire is increased to any degree previously determined on. The piece may be taken out of the fire in any period of the process, and dropped immediately into water, so as to be fit for measuring by the gage in a few seconds of time. At the same instant, another piece may be introduced into the place of the former, to be taken out and measured in its turn; and thus alternately, till the desired degree of heat is obtained. But as the cold piece will be 2 or 3 minutes in receiving the full heat, and corresponding contraction; to avoid this loss of time it may be proper, on



some occasions, to have 2 or more pieces, according to convenience, put in together at first, that they may be successively cooled in water, and the degrees of heat examined at shorter intervals. It will be unnecessary to say any thing further on precautions or procedures which the very idea of a thermometer must suggest, and in which it is not apprehended that any difficulty can occur, which every experimenter will not readily find means to obviate.

12. It now only remains, that the language of this new thermometer be understood, and that it may be known what the heats meant by its degrees really are. For this purpose a great number of experiments has been made, from which the following results are selected. The scale commences at a red-heat, fully visible in day-light; and the greatest heat that I have hitherto obtained in my experiments is  $160^{\circ}$ . This degree I have produced in an air-furnace about 8 inches square. Mr. Alchorne has been so obliging as to try the necessary experiments with the pure metals at the Tower, to ascertain at what degrees of this thermometer they go into fusion; and it appears, that the Swedish copper melts at 27, silver at 28, and gold at 32. Brass is in fusion at 21. Yet, in the brass and copper foundries, the workmen carry their fires to  $140^{\circ}$  and upwards: for what purpose they so far exceed the melting heat, or whether so great an additional heat be really necessary, I have not learnt. The welding heat of iron is from 90 to  $95^{\circ}$ ; and the greatest heat that could be produced in a common smith's forge 125. Cast iron was found to melt at  $130^{\circ}$ , both in a crucible in my own furnace, and at the foundry; but could not be brought into fusion in the smith's forge, though that heat is only  $5^{\circ}$  lower. The heat by which iron is run down among the fuel for casting is  $150^{\circ}$ .

As the welding state of iron is a softening or beginning fusion of the surface, it has been generally thought that cast iron would melt with much less heat than what is necessary for producing this effect on the forged; whereas, on the contrary, cast iron appears to require, for its fusion, a heat exceeding the welding heat 35 or  $40^{\circ}$ , which is much more than the heat of melted copper exceeds the lowest visible redness. Thus we find, that though the heat for melting copper is by some called a white heat, it is only  $27^{\circ}$  of this thermometer. The welding heat of iron, or  $90^{\circ}$ , is likewise a white heat; even  $130^{\circ}$ , at which cast iron is in fusion, is no more than a white heat; and so on to  $160^{\circ}$  and upwards is all a white heat still. This shows abundantly how vague such a denomination must be, and how inadequate to the purpose of giving us any clear ideas of the extent of what we have been accustomed to consider as one of the 3 divisions of heat in ignited bodies.

A Hessian crucible, in the iron foundry, viz. about  $150^{\circ}$ , melted into a slag-like substance. Soft iron nails, in a Hessian crucible in my own furnace, melted into one mass with the bottom of the crucible, at  $154^{\circ}$ : the part of the crucible



above the iron was little injured. The fonding heat of the glass furnaces I examined, or that by which the perfect vitrification of the materials is produced, was at one of them  $114^{\circ}$  for flint-glass, and  $124^{\circ}$  for plate-glass; at another it was only  $70^{\circ}$  for the former, which shows the inequality of heat, perhaps unknown to the workmen themselves, made use of for the same purpose. After complete vitrification, the heat is abated for some hours to  $28$  or  $29^{\circ}$ , which is called the settling heat; and this heat is sufficient for keeping the glass in fusion. The fire is afterwards increased, for working the glass, to what is called the working heat; and this I found, in plate-glass, to be  $57^{\circ}$ . Delft ware is fired by a heat of  $40$  or  $41^{\circ}$ ; cream-coloured, or queen's ware, by  $86^{\circ}$ ; and stone ware, called by the French pots de grès, by  $102^{\circ}$ : by this strong heat, it is changed to a true porcelain texture. The thermometer-pieces begin to acquire a porcelain texture at about  $110^{\circ}$ .

The above degrees of heat were ascertained by thermometer-pieces fired along with the ware in the respective kilns. But this thermometer affords means of doing much more, and, going further in these measures than I could at first even have expected; it will enable us to ascertain the heats by which many of the porcelains and earthen wares of distant nations and different ages have been fired: for as burnt clay, and compositions in which clay is a prevailing ingredient, suffer no diminution of their bulk by being repassed through degrees of heat which they have already undergone, but are diminished by any additional heat (according to obs. 5), if a fragment of them be made to fit into any part of the gage, and then fired along with a thermometer-piece till it begins to diminish, the degree at which this happens points out the heat by which it had been fired before. Of several pieces of ancient Roman and Etruscan wares, which I have examined, none appear to have undergone a greater heat than  $32^{\circ}$ , and none less than  $20^{\circ}$ ; for they all began to diminish at those or the intermediate degrees.

By means of this thermometer some interesting properties of natural bodies may likewise be discovered, or more accurately determined, and the genus of the bodies ascertained. Jasper, for instance, is found to diminish in the fire, like an artificial mixture of clay and siliceous matter; granite, on the contrary, has its bulk enlarged by fire, while flint and quartzose stones are neither enlarged nor diminished. These experiments were made in fires between  $70$  and  $80^{\circ}$  of this thermometer. A sufficient number of facts like these, compared with each other, and with the properties of such natural or artificial bodies as we wish to find out the composition of, may lead to various discoveries, of which I have already found some promising appearances; but many more experiments are wanting to enable me to speak with that certainty and precision on these subjects which they appear to deserve.



A piece of an Etruscan vase melted completely at  $33^{\circ}$ ; pieces of some other vases and Roman ware about  $36^{\circ}$ ; Worcester china vitrified at  $94^{\circ}$ ; Mr. Sprimont's Chelsea china at  $105^{\circ}$ ; the Derby at  $112^{\circ}$ ; and Bow at  $121^{\circ}$ ; but Bristol china showed no appearance of vitrification at  $135^{\circ}$ . The common sort of Chinese porcelain does not perfectly vitrify by any fire I could produce; but began to soften about  $120^{\circ}$ , and at  $156^{\circ}$  became so soft as to sink down, and apply itself close on a very irregular surface underneath. The true stone nankeen, by this strong heat, does not soften in the least; nor does it even acquire a porcelain texture, the unglazed parts continuing in such a state as to imbibe water and stick to the tongue. The Dresden porcelain is more refractory than the common Chinese, but not equally so with the stone nankeen. The cream-coloured or queen's ware bears the same heat as the Dresden, and the body is as little affected by this intense degree of fire.

Mr. Pott says, that to melt a mixture of chalk and clay in certain proportions, which proportions appear from his tables to be equal parts, is "among the master-pieces of art." This mixture melts into a perfect glass at  $123^{\circ}$  of this thermometer. The whole of Mr. Pott's or any other experiments may, by repeating and accompanying them with these thermometric pieces, have their respective degrees of heat ascertained, and thereby be rendered more intelligible and useful, to the reader, the experimenter, and the working artist. I flatter myself that a field is thus opened for a new kind of thermometrical inquiries; and that we shall obtain clearer ideas with regard to the differences of the degrees of strong fire, and their corresponding effects on natural and artificial bodies; those degrees being now rendered accurately measurable, and comparable with each other, equally with the lower degrees of heat which are the province of the common mercurial thermometer.

APPENDIX. *Analysis of the clay of which the thermometric pieces are formed.*—This clay makes no effervescence with acids. Diluted nitrous and marine acids being boiled on it, and afterwards saturated with fixed alkali, no precipitation or turbidness appeared. It therefore contains no calcareous earth, as that earth would have been dissolved by the acids, and precipitated from them by the alkali. Calcined with powdered charcoal, it contracted no sulphureous smell, and the acids had no more action on it than before. It therefore contains no gypsous matter, or combination of calcareous earth with vitriolic acid; as that acid would have formed sulphur with the inflammable principle of the charcoal, and left the calcareous earth pure, or in a state of solubility by acids.

Some of the clay was calcined with an equal weight of salt of tartar, which, for the greater certainty in regard to its purity, had been run per deliquium, and afterwards evaporated to dryness. The calcined mixture was boiled in water, the filtered liquor slowly evaporated, and suffered to cool at intervals. No



crystallization was formed: the dry salt appeared merely alkaline as at first, and deliquiated in the air; a further proof that this clay contains no gypseous matter; for the vitriolic acid would have been absorbed by the alkali, and formed vitriolated tartar, a salt which neither liquefies in the air, nor dissolves easily in water, and which therefore would have crystallized long before the alkali became dry, or remained after its deliquiation. A 20th part of gypsum, ground with clay, was very distinguishable by both the foregoing processes; producing a sulphureous smell; and calcareous earth by calcination with charcoal powder; and crystals of vitriolated tartar by calcination with the same alkaline salt.

To separate the pure argillaceous part, or that matter which in all clays forms alum with the vitriolic acid, 240 grains of this clay were thoroughly moistened with oil of vitriol, boiled to dryness, and at last made nearly red-hot. The mixture was then boiled in water; the earth which remained undissolved was treated again in the same manner with vitriolic acid, and this operation repeated 5 or 6 times. The clay was diminished in the first operation about 70 grains; but less and less in the succeeding ones, and in the last scarcely 2 grains. The filtered liquors yielded crystals of true alum; but its quantity was not examined, as the produce of alum from aluminous earth is already sufficiently known, and the quantity of aluminous earth itself, or its proportion to the indissoluble earth, was here the object. From the 240 grains of clay there remained in one experiment 98, and in another 95 grains of indissoluble earth; so that 5 parts of this clay consist of 3 parts of pure argillaceous or alum earth, and 2 parts of an earth of a different kind.

With respect to the nature of this last earth, it is easier to determine negatively what it is not, than positively what it is; but ascertaining the former will be a great step towards the discovery of the latter. That it is not calcareous, gypseous, or argillaceous, is manifest from the experiments.—It is not jasper; as this consists, in great part, of argillaceous earth, which would have been extracted by the vitriolic acid.—It is not fluor; as this, by the same acid, would have been decomposed, its own acid expelled, and a gypseous earth left.—It is not of the micaceous kind; as the peculiar aspect of these earths would readily betray them to the eye.—It is not granite; for strong fire, which granite melts in, has no effect on this. Nor is there any known kind of earth to which it is in any degree similar, except those of the siliceous order; and with these it perfectly agrees in all the properties I am acquainted with, that they possess in a state of powder.

It does not vitrify or soften with pure clay, in the strongest fire I have been able to produce. Nor is it disposed to melt with the matter of Hessian crucibles; for a little of it rubbed on the inside of a crucible, and urged with strong



fire, continued white, powdery, and unaltered. Thirty grains of this earth were mixed with an equal weight of dry fossil alkali, and the same quantity of a fine white quartz sand was mixed with the same proportion of the same alkali: the two mixtures were put into 2 small crucibles, which were surrounded with sand in a larger one, that both might be exposed to an equal heat. They both began to melt at the same time; and at about  $80^{\circ}$  of the thermometer they had formed perfect transparent glasses. Though these properties may not, perhaps, be thought sufficient of themselves, for determining with certainty that this substance is of the siliceous kind, yet, when joined to the negative proofs, of its not belonging to any other known order of earthy bodies, they afford the fullest evidence which the nature of the subject can admit of, that the indissoluble part of this clay is truly siliceous; and consequently that the clay consists of 2 parts of pure siliceous earth, to 3 parts of pure argillaceous or aluminous earth.

*XX. An Analysis of Two Mineral Substances, viz. the Rowley-rag-stone, and the Toad-stone. By William Withering,\* M. D. p. 327.*

In a prefatory letter to Dr. Priestley, Dr. W. states that he had sent the results of his examination of the toad-stone and the Rowley-rag-stone; being part of a plan which he had long before formed for a chemical analysis of all the substances that are known to exist in the earth in large quantity. Some years before he transmitted to the R. S. an analysis of the different marles found in Staffordshire; and in the course of experiments which this subject had led him to, he found it convenient to form some new tables, and to enlarge some that

\* Dr. Withering practised physic, for a great number of years, with much celebrity, at Birmingham; near which place he died in 1799, in the 58th year of his age. He was initiated in the medical profession under his father, who was an apothecary at Wellington, in Shropshire, and was afterwards sent to Edinburgh, where he took his degree of M. D. in 1766. In 1776 he published his *Arrangement of Plants growing naturally in Great-Britain*, in 2 vols., and which has since gone through 4 editions, with numerous improvements and additions, so as to make 4 vols. in 8vo. It is a most complete national Flora. In 1779 appeared his *Account of the Scarlet Fever and Sore Throat*, and in 1785 his *Account of the Foxglove*. In both these publications he appeared to great advantage as a practical physician. And although he was not the first to point out the diuretic powers of the foxglove in hydropic affections (that having been done before by Dr. Darwin) yet he produced a great number of cases in which it had been given with success, together with many useful admonitions concerning its preparations and doses. In 1783 he published a translation of Bergmann's *Sciagraphia Regni Mineralis*, under the title of *Outlines of Mineralogy*; before which time he had shown that he had bestowed considerable attention on chemical pursuits, by some papers inserted in the *Philos. Trans.* It should be added that, while he was at Lisbon for the benefit of his health in 1795, he analyzed the hot mineral waters in the neighbourhood of that city. For other particulars concerning the life of this ingenious physician and naturalist, the reader is referred to the *Gent. Mag.* for 1799, and to Dr. Duncan's *Annals of Medicine* for the same year.



were less completely formed before. One of them he subjoined to this paper. The facts taken from M. Macquer are marked with an M; those with the \* are the consequence of his own experiments.

In order to save much repetition in future, it may not be amiss to mention, once for all, a few particulars in the conduct of these processes. 1st. By water, is always meant water distilled in glass vessels, or by means of a large tin refrigeratory, in Mr. Irwin's method. 2d. Only glass or china vessels are used in the liquid processes. 3d. By a mortar he means those excellent ones made by Mr. Wedgwood; or as will be specified at the time, a steel mortar tempered so hard that it will bear the grinding of enamel in it without discolouration.

4th. Filtres are never employed, it being found impossible to get the quantities accurate where they are used. The powdery parts are allowed to subside till the supernatant liquor becomes clear. This sometimes requires days or weeks; but he was ignorant of a better method. By giving the vessels a circular motion round their axes, he could greatly facilitate the subsiding of the solid contents. If the separating vessels are made like a common tart-dish, with a spreading border, the liquors may be poured off very near, without disturbing the sediments. 5th. Phlogisticated alkali, means the vegetable fixed alkali prepared by the deflagration of nitre and crystals of tartar dissolved in water, and boiled with Prussian blue in such quantity, that it will not any longer precipitate an earth from an acid.

#### ROWLEY-RAG.

The stone which is the subject of the following experiments forms a range of hills in the southern part of Staffordshire. The lime-stone rocks at Dudley bed up against it, and the coal comes up to the surface against the lime-stone. The highest part of the hills is near the village of Rowley. The summit has a craggy, broken appearance, and the fields on each side to a considerable distance are scattered over with large fragments of the rock, many of which are sunk in the ground. In a quarry near Dudley, where a pretty large opening has been made in order to get materials for mending the roads, the rock appears to be composed of masses of irregular rhomboidal figures: some of these masses inclose rounded pebbles of the same materials. At the distance of 4, 5, or 6 miles from the hills, as at Bilston, Willenhall, and Wednesbury, the rag-stone is frequently found some feet below the surface in rhomboidal pieces, forming an horizontal bed of no great depth, and seldom of more than a few yards extent. Over the whole of this tract of country it is used to mend the roads, and lately has been carried to Birmingham to pave the streets. Some people sell it in powder, as a substitute for emery in cutting and polishing.

*More obvious properties.*—Its appearance dark grey, with numerous minute



shining crystals. When exposed to the weather, gets an ochry colour on the outside; strikes fire with steel; cuts glass; melts, though not easily, under the blow-pipe. Heated in an open fire, becomes magnetic, and loses about 3 in 100 of its weight.

*Exper.*—A. after 3 drs. had been broken to small pieces with a hard steel hammer, on a plate of the same metal, it was ground to an impalpable powder in one of Mr. Wedgewood's China mortars. The mortar, which had been previously weighed, lost only  $\frac{1}{3}$  of a grain weight during this operation.

B. This powder was repeatedly washed with pure water, so as to carry off all the finer parts, and the coarser ground again, till the whole was washed away. The washings were then filtered, and the powder carefully collected and dried. The water employed in the washings did not appear to have dissolved any part of the stone; for no precipitate was formed either on the addition of mild fixed alkali, or of silver dissolved in the nitrous acid.

C. 100 parts of this powder were put into a small matrass, and covered with marine acid: a degree of heat was excited, and a very slight effervescence took place. Water was then added, and the mixture kept boiling for half an hour. The liquor was decanted off, and more acid added, which was boiled as before. This was decanted, and the residuum washed with water till the water came off tasteless. These waters were added to the liquors before decanted. The powder had now an ash-coloured appearance, and when dried weighed  $80\frac{1}{4}$ . To the liquors (c) phlogisticated fixed alkali was added, till no more Prussian blue was precipitated. To effect this, it took 1 oz. 5 drs. 12 grs. of the phlogisticated alkali. The precipitate, when washed and dried, weighed 47.

E. The powder of  $80\frac{1}{4}$  (c) mixed with twice its weight of fossile fixed alkali, was put into a black lead crucible, and exposed to a red heat for 2 hours. The heat was never sufficient to render the mass fluid, nor to make it adhere firmly to the crucible. The saline part was then washed away by repeated effusions of hot water. To the remaining powder marine acid was added repeatedly, and boiled as before. The powder was now perfectlyedulcorated by hot water, and when dry weighed  $47\frac{1}{2}$ . The above liquors were all added to the liquor (c), and phlogisticated fixed alkali was dropped in, till no more Prussian blue was precipitated. To effect this,  $\frac{1}{2}$  oz. of the alkali was required. This precipitate weighed 19; so that the whole of the Prussian blue weighed 66. After calcination in a crucible it was reduced to  $31\frac{1}{2}$ , and was then wholly attracted by a magnet.

F. Mild fixed alkali was now gradually added to the liquors after the separation of the Prussian blue, and a white powder was precipitated. This powder, when well washed and dried, weighed  $46\frac{3}{4}$ . After being exposed to a low red heat for 10 minutes, it weighed only  $32\frac{1}{2}$ .



G. Theedulcorated powder (E) was now perfectly white; was not acted on either by the vitriolic, nitrous, or marine acids, but readily melted into a glass with fossile fixed alkali; during the melting an effervescence took place.

H. The white powder (F) readily dissolved in diluted vitriolic acid, and under a slow evaporation formed crystals which had the appearance and the taste of alum. These crystals were then reduced to powder, and boiled in alcohol. The alcohol was decanted off, but did not appear to have dissolved any part of the powder; nor did it afford any precipitate on the addition of mild fixed alkali.

*Conclusions.*—From these experiments it appears, that the Rowley-rag-stone consists of siliceous earth, clay, or earth of alum, and calx of iron. From the latter must be deducted  $11\frac{1}{2}$  for the quantity of calciform iron, found by experiment to be contained in the quantity of phlogisticated alkali made use of, and then the proportions in 100 parts of the stone will be these: Pure siliceous earth  $47\frac{1}{2}$ ; pure clay, free from fixable air,  $32\frac{1}{2}$ ; iron in a calciform state 20; the sum 100.

From this view of the component parts of this stone, it is not improbable, that it might advantageously be used as a flux for calcareous iron ores. The makers of iron are acquainted with such ores; but never could work them to advantage, for want of a cheap and efficacious flux.

#### TOAD-STONE,

From Derbyshire; sent to Dr. W. by Mr. Whitehurst, who has so fully and so accurately described the mode of its stratification, that it is needless to enlarge on that subject.

*More obvious properties.*—Of a dark brownish grey, a granulated texture; with several cavities filled with crystallized spar. It does not strike fire with steel. It melts to a black glass.

*Exp.*—A. 100 parts rubbed to an extremely fine powder in a China mortar, and boiled in marine acid; the solution was decanted: the undissolved part, after proper washing and drying, weighed 71.

B. The undissolved part was rubbed with twice its weight of mild fossil alkali, and then exposed to a red heat in a black lead crucible for 1 hour.

C. This mixed mass was reduced to powder, and repeatedly boiled, first in marine, afterwards in strong vitriolic acid: the residuum now weighed 56, and was perfectly white.

D. The liquors of exp. A. and C. being put all together, phlogisticated fixed alkali was added till no further precipitation ensued. This precipitate was a Prussian blue, which, when washed and dried, weighed  $56\frac{5}{6}$ . After exposure to a red heat in a crucible for 40 minutes, it weighed only 29, and was wholly attracted by the magnet. Now the 2 oz. 5 dr. and 32 gr. of phlogisticated fixed alkali used in this experiment, contain 13 gr. of calciform iron, as ascertained by



a separate trial; therefore, deducting 13 from 22, we have 16 for the quantity of calciform iron obtained from the stone.

E. The earthy parts were next precipitated from the liquors by the addition of mild fossil alkali. The precipitate, when perfectlyedulcorated and dried, weighed  $29\frac{8}{10}$ .

F. Distilled vinegar was added to this powder, and suffered to stand in a cool place for 4 hours; the vinegar was poured off, and the residuum repeatedly washed with pure water. To these liquors mild fixed alkali was added, and a white precipitate subsided, which, when washed and dried, weighed  $7\frac{5}{10}$ .

G. To the residuum (F) dilute vitriolic acid was added: a solution took place, which solution, by evaporation and crystallization, yielded alum.

H. The part of the residuum (F) undissolved by the vitriolic acid was boiled in nitrous acid, in marine acid, and in aqua regia, without being diminished; the weight of it when dried was  $7\frac{5}{10}$ . It could not be fused by the greater heat of a blow-pipe, but melted into a glass when mixed with calcareous earth.

I. The undissolved part (exp. c.) was not fusible by itself; nor was it acted on by vitriolic, nitrous, or marine acid. It melted into a glass with fossil alkali.

K. The precipitate of  $7\frac{5}{10}$  (exp. F.) after a sufficient exposure to heat was put into 1 oz. of water: the next morning the water had a pellicle on its surface, and tasted like lime-water.

*Conclusions.*—Hence it appears, that 100 parts of this specimen of toad-stone contained:

c. Siliceous earth.....	56	} = $63\frac{18}{10}$
H. More ditto .....	$7\frac{18}{10}$	
D. Calciform iron.....	16	
F. K. Calcareous earth.....	$7\frac{5}{10}$	
G. H. Earth of alum. ....	$14\frac{8}{10}$	
		<hr/>
		$101\frac{8}{10}$

From the addition of  $1\frac{8}{10}$  of weight it is probable, that the substances capable of uniting with fixable air were not in the specimen used fully saturated with it, as they would be after their precipitation by the mild alkali. On repeating these experiments with different portions of the toad-stone, the quantities of the calcareous earth were found to differ a little; but nothing further appeared to invalidate the general conclusions.



A Table showing the Solubility or Insolubility of certain Saline Substances in Alcohol.

		Substances.	Results.			Substances.	Results.	
Vitriolic acid	Neut.	Vitriolated tartar.	Insoluble m.	Muriatic acid	Neut.	Digestive salt.	Soluble m.	
		Glauber's salt.	Insoluble m.			Common salt.	Insoluble m.	
		Vitriolic ammoniac.	Insoluble m.			Sal ammoniac.	Soluble m.	
		Vitriol of silver.	Insoluble m.			Luna cornea.	Insoluble m.	
	Metal.	—mercury.	Insoluble m.		Metal.	Corros. Sublimate.	Soluble m.	
		—copper.	Insoluble m.			Muria cupri.	Soluble m.	
		—iron.	Insoluble m.			—ferri.	Soluble m.	
		—zinc.	Insoluble.*			Muria calcarea.	Soluble m.	
	Earthy	Heavy spar.	Insoluble.*		Earthy.	—magnesiae.	Soluble.*	
		Selenite.	Insoluble m.			—aluminosa.	Soluble.*	
		Alum.	Insoluble.*	Neut.	Soluble tartar.	Soluble *		
		Epsom salt.	Soluble.*		Rochelle salt.	Insoluble.*		
Nitrous acid	Neut.	Nitre.	Soluble m.	Veget. acid	Neut.	Veget. ammoniac.		
		Cubic nitre.	Soluble m.			Metal.	Verdigris.	Soluble.*
		Nitrous ammoniac.	Soluble m.				Sugar of lead.	Soluble.*
		Nitre of silver.	Soluble m.					
	Metal.	—mercury.	Insoluble m.		Calca. acid	Neut.	Veg. alkali mild.	Insoluble.*
		—copper.	Soluble m.				Foss. alkali mild.	Insoluble.*
		—lead.	Soluble.*				Vol. alkali mild.	Soluble.*
		Calcareous.	Soluble m.				Calcareous spar.	Soluble.*

XXI. *New Fundamental Experiments on the Collision of Bodies.* By Mr. John Smeaton, F. R. S. p. 337.

It is universally acknowledged, that the first simple principles of science cannot be too critically examined, in order to their being firmly established; more especially those which relate to the practical and operative parts of mechanics, on which much of the active business of mankind depends. A sentiment of this kind occasioned my tract on mechanic power, published in the Philos. Trans., for 1776 (abridg. vol. 14, p. 72). What I have now to offer was intended as a supplement to that, and the experiments were then in part tried; but the completion of them was deferred at that time, partly from want of leisure; partly to avoid too great a length of the paper itself; and partly to avoid the bringing forward too many points at once. My present purpose is to show, that the true doctrine of the collision of bodies hangs as it were on the same hook, as the doctrine of the gradual generation of motion from rest, considered in that paper; that is, that whether bodies are put into gradual motion, and uniformly accelerated from rest to any given velocity; or are put in motion, in an instantaneous manner, when bodies of any kind strike one another; the motion, or sum of the motions produced, has the same relation to mechanic power there defined, which is necessary to produce the motion desired. To prove this, and at the same time to show some capital mistakes in principle, which have been assumed as indisputable truths by men of great learning, is the reason of my now pursuing the same subject.

I do not mean to point out the particular mistakes which have been made by



particular men, as that would lead me into too great a length : I shall therefore content myself with observing, that the laws of collision, which have been investigated by mathematical philosophers, are principally of 3 kinds ; viz. those relating to bodies perfectly elastic ; to bodies perfectly unelastic, and perfectly soft ; and to bodies perfectly unelastic, and perfectly hard. To avoid prolixity, I shall consider in each, only the simple case of 2 bodies which are equal in weight, or quantity of matter, striking each other. Respecting those which are perfectly elastic, it is universally agreed that, when 2 such bodies strike each other, no motion is lost ; but that in all cases, what is lost by one is acquired by the other : and hence, that if an elastic body in motion strike another at rest, on the stroke the former will be reduced to a state of rest, and the latter will fly off with an equal velocity. In like manner, if a non-elastic soft body strike another at rest, they neither of them remain at rest, but proceed together from the point of collision with exactly one half of the velocity that the first had before the stroke ; this is also universally allowed to be true, and is fully proved by very good experiments on the subject.

Respecting the 3d species of body, that is, those that are non-elastic, and yet perfectly hard ; the laws of motion relating to them, as laid down by one species of philosophers, have been rejected by another ; the latter alledging, that there are no such bodies to be found in nature to try the experiment on ; but those who have laid down and assigned the doctrine that would attend the collision of bodies of this kind, if they could be found, have universally agreed, that if a non-elastic hard body was to strike another of the same kind at rest, that, in the same manner as is agreed concerning non-elastic soft bodies, they neither of them would remain at rest, but would in like manner proceed from the point of collision, with exactly one half of the velocity that the first had before the stroke : in short, they lay it down as a rule attending all non-elastic bodies, whether hard or soft, that the velocity after the stroke will be the same in both, viz. one half of the velocity of the original striking body. Here is therefore the assumption of a principle, which in reality is proved by no experiment, nor by any fair deduction of reason that I know of, viz. that the velocity of non-elastic hard bodies after the stroke must be the same as that resulting from the stroke of non-elastic soft bodies ; and the question now is, whether it is true or not ?

Here it may be very properly asked, what ill effects can result to practical men, if philosophers should reason wrong concerning the effects of what does not exist in nature, since the practical men can have no such materials to work on, or misjudge of ? But it is answered, that they who infer an equality of effects between the 2 sorts, may from thence be misled themselves, and in consequence mislead practical men in their reasonings and conclusions concerning the sort



with which they have abundant concern, to wit, the non-elastic soft bodies, of which water is one, which they have much to do with in their daily practice.

Previous to the trying my experiment on mills I never had doubted the truth of the doctrine, that the same velocity resulted from the stroke of both sorts of non-elastic bodies; but the trial of those experiments made me clearly see at least the inconclusiveness, if not the falsity, of that doctrine: because I found a result which I did not expect to have arisen from either sort; and from which, when it appeared from experiment, I could see a substantial reason why it should take place in one sort, and that it was impossible that it could take place in the other; for if it did, the bodies could not have been perfectly hard, which would be contrary to the hypothesis. Of this deduction I have given notice in my said tract on mills, published in the Philos. Trans. for 1759,\* [Abridg. vol. ii. p. 338]:

It may also be said, that since we have no bodies perfectly elastic, or perfectly unelastic and soft, why should we expect any bodies perfectly unelastic and hard? Why may not the effects be such as should result from a supposition of their being imperfectly elastic joined with their being imperfectly hard? But here I must observe, that the supposition appears to be a contradiction in terms. We have bodies which are so nearly perfectly elastic, that the laws may be very well deduced and confirmed by them; and the same obtains with respect to non-elastic soft bodies; but concerning bodies of a mixed nature, which are by far the greatest number, so far as they are wanting in elasticity, they are soft, and bruise, yield, or leave a mark in collision; and so far as they are not perfectly soft they are elastic, and observe a mixture of the law relative to each; but imperfectly elastic bodies, imperfectly hard, come in reality under the same description as the former mixed bodies: for so far as they are imperfectly hard they are soft, and either bruise and yield, or leave a mark in the stroke; and so far as they want perfect elasticity, they are non-elastic; that is to say, they are bodies imperfectly elastic, and imperfectly soft; and in fact I have never yet seen any bodies but what come under this description. It seems therefore, that respecting the hardness of bodies, they differ in degrees of it, in proportion as they have a greater degree of tenacity or cohesion; that is, are farther removed from perfect softness, at the same time that their elastic springs, so far as they reach, are very stiff; and hence we may by the way conclude, that the same mechanic power that is required to change the figure in a small degree of those bodies that have the popular appellation of hard bodies, would change it in a great degree in those bodies that approach towards softness, by having a small degree of tena-

\* "The effect therefore, of overshot wheels, under the same circumstance of quantity and fall, is at a medium double to that of the undershot: and as a consequence thereof, that non-elastic bodies, when acting by their impulse or collision, communicate only a part of their original power; the other part being spent in changing their figure in consequence of the stroke."—Orig.



city or cohesion. In the former kind we may rank the harder kinds of cast iron, and in the latter, soft tempered clay.

While the philosophical world was divided by the dispute about the old and new opinion, as it was called, concerning the powers of bodies in motion, in proportion to their different velocities: those who held the old opinion contending, that it was as the velocity simply, asked those of the new, How, on their principles, they would get rid of the conclusions arising from the doctrine of un-elastic perfectly hard bodies? They replied, they found no such bodies in nature, and therefore did not concern themselves about them. On the other hand, those of the new opinion asked those of the old, How they would account for the case of non-elastic soft bodies, where, according to them, the whole motion lost by the striking body was retained in the two after the stroke, the two bodies moving together with the half velocity, though the two non-elastic bodies had been bruised and changed their figure by the stroke; for, if no motion was lost, the change of figure must be an effect without a cause? To obviate this, those of the old opinion seriously set about proving, that the bodies might change their figure, without any loss of motion in either of the striking bodies.

Neither of these answers have appeared to me satisfactory, especially since my mill experiments; for with respect to the first, it is no proper argument to urge the impossibility of finding the proper material for an experiment, in answer to a conclusion drawn from an abstract idea. On the other hand, if it can be shown, that the figure of a body can be changed, without a power, then, by the same law, we might be able to make a forge hammer work on a mass of soft iron, without any other power than that necessary to overcome the friction, resistance, and original vis inertiae, of the parts of the machine to be put in motion; for, as no progressive motion is given to the mass of iron by the hammer, it being supported by the anvil, no power can be expended that way; and if none is lost to the hammer from changing the figure of the iron, which is the only effect produced, then the whole power must reside in the hammer, and it would jump back again to the place from which it fell, just in the same manner as if it fell on a body perfectly elastic, on which, if it did fall, the case would really happen: the power therefore to work the hammer would be the same, whether it fell on an elastic or non-elastic body; an idea so very contrary to all experience, and even apprehension, of both the philosopher and vulgar artist, that I shall here leave it to its own condemnation.

As nothing however is so convincing to the mind as experiments obvious to the senses, I was very desirous of contriving an experiment in point; and as I saw no hopes of finding matter to make a direct experiment, I turned my mind towards an indirect one; so circumscribed however, as to prove incontestably, that the result of the stroke of two non-elastic perfectly hard bodies could not



be the same as would result from the collision of two soft ones; that is, if it can be bonâ fide proved, that one-half of the original power is lost in the stroke of soft bodies by the change of figure, as was very strongly suggested by the mill experiments; then, since no such loss can happen in the collision of bodies perfectly hard, the result and consequence of such a stroke must be different.

The consequence of a stroke of bodies perfectly hard, but void of elasticity, must doubtless be different from that of bodies perfectly elastic: for having no spring, the body at rest could not be driven off with the velocity of the striking body, for that is the consequence of the action of the spring or elastic parts between them, as will be shown in the result of the experiments; the striking body will therefore not be stopped, and as the motion it loses must be communicated to the other, from the equality of action and reaction, they will proceed together, with an equal velocity, as in the case of non-elastic soft bodies: the question therefore that remains is, what that velocity must be?—It must be greater than that of the non-elastic soft bodies, because there is no mechanical power lost in the stroke. It must be less than that of the striking body, because, if equal, instead of a loss of motion by the collision, it will be doubled. If therefore non-elastic soft bodies lose half their motion, or mechanical power, by change of figure in collision, and yet proceed together with half the velocity, and the non-elastic hard bodies can lose none in any manner whatever; then, as they must move together, their velocity must be such as to preserve the equality of the mechanic power unimpaired, after the stroke, the same as it was before it.

For example, let the velocity of the striking body before the stroke be 20, and its mass or quantity of matter 8; then, according to the rule deduced from the experiments in the tract on Mechanic Power (see exper. 3 and 4) that power will be expressed by  $20 \times 20 = 400$ , which  $\times 8 = 3200$ ; and if half of it is lost in the stroke,\* in the case of non-elastic soft bodies, it will be reduced to 1600; which  $\div 16$  the double quantity of matter, will give 100 for the square

\* But, it may be said, if half of it is not lost by the stroke, what then becomes of Mr. S.'s rule? And what good reason has he to suppose that the half, or indeed any part, of the power or momentum of any body is lost by the stroke? In fact, Mr. S. bewilders and puzzles himself about a thing which he calls mechanical power, which is proportional to the height or space that a body falls through to acquire its velocity, which is known to be proportional to the square of that velocity. Whereas the real force or momentum of a body in motion, or with which it strikes any obstacle, is a quite different thing, being proportional to the velocity in a given body, or to the product of the velocity and the body, no part of which is lost by the stroke, but remains the same after the stroke as before it. And hence, instead of Mr. S.'s complex and unnatural way of computing the common velocity of the two bodies after the stroke, the rule is very plain and simple; viz. divide the momentum,  $20 \times 8 = 160$ , by the sum of the two bodies,  $8 + 8 = 16$ , and the quotient 10, is their common velocity after the stroke.



of their velocity; the square root of which being 10, will be the velocity of the two non-elastic soft bodies after the stroke, being just one-half of the original velocity, as it is constantly found to be. But in the non-elastic hard bodies, no power being lost in the stroke, the mechanic power will remain after it, as before it,  $= 3200$ ; this, in like manner, being divided by 16, the double quantity of matter, will give 200 for the square of the velocity, the square root of which is 14.14 &c. for their velocity after the stroke, which is to 10, the velocity of the non-elastic soft bodies after the stroke, as the square root of 2 to 1; or as the diagonal of a square to its side.\*

It remains therefore now to be proved, that precisely half of the mechanic power is lost in the collision of non-elastic soft bodies; for which purpose my mind suggested the following reflections. In the collision of elastic bodies, the effect, seemingly instantaneous, is yet performed in time; during which time the natural springs residing in elastic bodies, and which constitute them such, are bent or forced, till the motion of the striking body is divided between itself and the body at rest; and in this state the two bodies would then proceed together, as in the case of non-elastic soft bodies; but as the springs will immediately restore themselves in an equal time, and with the same degree of impulsive force, with which they were bent in this reaction, the motion that remained in the striking body will be totally destroyed, and the total exertion of the two springs, communicated to the original resting body, will cause it to fly off with the same velocity with which it was struck.

On this idea, if we could construct a couple of bodies in such a way, that they should either act as bodies perfectly elastic; or, that their springs should at pleasure be hooked up, retained, or prevented from restoring themselves, when at their extreme degree of bending; and if the bodies under these circumstances observed the laws of collision of non-elastic soft bodies, then it would be proved, that one-half of the mechanical power, residing in the striking body, would be lost in the action of collision; because the impulsive force or power of the spring in its restitution being cut off, or suspended from acting, which is equal to the impulsive force or power to bend it, and which alone has been employed to communicate motion from one body to the other, it would make it evident, that one-half of the impulsive force is lost in the action, as the other half remains locked up in the springs. It also follows, as a collateral circumstance, that be the impulsive power of the springs what it may from first to last, yet as one-half of the time of the action is by this means cut off, in this sense also it will follow, that

\* This erroneous conclusion is deduced by Mr. S. from his false rule, for finding the velocities of bodies after impact. The true rule is the same for all non-elastic bodies, whether hard or soft. If the bodies adhere and move together with one common velocity immediately after the stroke, that velocity must be the half of the velocity before the stroke, as found in the note above; whatever the nature of the bodies may be.



one-half of the mechanic power is destroyed; or rather, in this case, remains locked up in the springs, capable of being re-exerted whenever they are set at liberty, and of producing a fresh mechanical effect, equivalent to the motion or mechanical power of the two non-elastic soft bodies after their collision.

Hence we must infer, that the quantity of mechanical power expended in displacing the parts of non-elastic soft bodies in collision, is exactly the same as that expended in bending the springs of perfectly elastic bodies; but the difference in the ultimate effect is, that in the non-elastic soft bodies, the power taken to displace the parts will be totally lost and destroyed, as it would require an equal mechanic power to be raised afresh, and exerted in a contrary direction to restore the parts back again to their former places; whereas, in the case of the elastic bodies, the operation of half the mechanic power is, as observed already, only locked up and suspended, and capable of being re-exerted without a further original accession.

Those ideas arose from the result of the experiments tried on the machine described in my said tract on Mechanic Power, and were also communicated to my very worthy and ingenious friend Wm. Russel, Esq., F.R.S., at the same time that I showed him those experiments in 1759; but the mode of putting this matter to a full and fair mechanical trial has since occurred; and though some rough trials, sufficient to show the effect, were made on it, prior to offering the paper on mechanical power to the Society in 1776, yet the machine itself I had not leisure to complete to my satisfaction till lately; which I mention, to apologize for the length of time that these speculations have taken in bringing forward.

*Description of the machine for collision.*—Fig. 6, pl. 5, shows the front of the machine as it appears at rest when fitted for use. A is the pedestal, and AB the pillar, which supports the whole; c, d, are two compound bodies of about a pound weight each, but as nearly equal in weight as may be. These bodies are alike in construction, which will be more particularly explained by fig. 7. These bodies are suspended by 2 white fir rods, of about half an inch diameter, ef and gh, being about 4 feet long from the point of suspension to the centre of the bodies; and their suspension is on the cross piece II, which is mortised through, to let the rods pass with perfect freedom; and they hang on 2 small plates filed to an edge on the under side, and pass through the upper part of the rods. Their centres are at k and l, and the edges being let into a little notch, on each side of the mortise, the rods are at liberty to vibrate freely on their respective points, or rather edges, of suspension, and are determined to one plain of vibration. MN is a flat arch of white wood, which may be covered with paper, that the marks on it may be the more conspicuous. The cross piece II is made to project so far before the pillar, that the bodies in their vibrations may



pass clear of it, without danger of striking it; and also the arch *MN* is brought so far forward as to leave no more than a clearance, sufficient for the rods to vibrate freely without touching it.

Fig. 7, shows one of the compound bodies, drawn of a larger size. *AB* is a block of wood, and about as much in breadth as it is represented in height, through a hole in which the wooden rod *cc* passes, and is fixed in it. *DB* represents a plate of lead, about  $\frac{3}{8}$  of an inch thick, one on each side, screwed on by way of giving it a competent weight. *dbefg* represents the edge of a springing plate of brass, rendered elastic by hard hammering; it is about  $\frac{5}{8}$  of an inch in breadth, and about  $\frac{1}{20}$  of an inch thick. It is fixed down on the wooden block *db* by means of a bridge plate, whose end is shown at *hi*, and is screwed down on each side the spring plate by a screw which, being relaxed, the spring can be taken out at pleasure, and adjusted to its proper situation. *kl* is a light thin slip of a plate, whose under edge is cut into teeth like a fine saw or ratchet, and is attached to the spring by a pin at *k*, which passes through it, and also through a small stud rivetted into the back part of the spring, and on which pin, as a centre, it is freely moveable.

*mn* shows a small plate or stud seen edgewise, raised on the bridge plate, through a hole in which stud the ratchet passes; and the lower part of the hole is cut to a tooth shaped properly to catch the teeth of the ratchet, and retain it together with the spring at any degree to which it may be suddenly bent; and for this purpose it is kept bearing gently downward, by means of a wire-spring *opq*, which is in reality double, the bearing part at *o* being semi-circular; from which branching off on each side the rod *cc*, passes to *p*, and fixes at each end into the wood at *q*. However, to clear the ratchet, which is necessarily in the middle as well as the rod, the latter is perforated; and also the block is cut away, so far as to set the main spring at *e* free of all obstacles that would prevent its play from the point *B*. The part *fg* is shown thicker than the rest, by being covered with thin kid leather tight sewed on, to prevent a certain jarring that otherwise takes place on the meeting of the springs in collision.

In fig. 6, the marks on the arch *MN* are put on as follows. *op* is an arch of a circle from the centre *l*, and *qr* an arch of a circle from the centre *k* intersecting each other at *s*. Now the middle line of the marks *t*, *v*, are at the same distance from the middle line at *s* that the centres *lk* are; so that when each body hangs in its own free position, without bearing against the other, the rod *ef* will cover the mark at *t*, and the rod *gh* will cover the mark at *v*. From the point *s*, and on the arches *sp* and *sq* respectively, set off points at an equal and competent distance from *s* each way, which will give the middle of the mark *w* and *x*: and on the arch *sp* find a middle point between the mark *v* and *w*, which let be *y*; and on the other side, in like manner, on the arch *sq* find a middle point



for the mark *z* ; then set off the distance *sv* or *st* from *y* each way, and from *z* each way ; and from these points, drawing lines to the respective centres *l* and *k*, they will give the place and position of the marks *a*, *b*, and *c*, *d* ; and thus is the machine prepared for use.

*For trials on elastic bodies.*—For this use, take out the pins and ratchets from each respectively, and the springs being then at liberty, with a short bit of stick, suppose the same size as the rods, turn aside the rod *gh* with the right hand, carrying the body *D* upwards till the stick is on the mark *w*, as suppose at  $\odot$  ; there hold it, and with the left set the body *c* perfectly at rest ; in which case the rod *ef* will be over the mark *t* ; then suddenly withdraw the stick, in the direction that the rod *gh* is to follow it, and the spring of the body *D*, impinging on that of the body *c*, they will be both bent, and also restored ; and the body *c* will fly off, and mount till its rod *ef* covers the mark *x* ; the rod of the striking body *D* remaining at rest on its proper mark of rest *v*, till the body *c* returns, when the body *D* will fly off in the same manner ; the two bodies thus rebounding a number of times, losing a part of their vibration each time ; but so nearly is the theory of elastic bodies thus fulfilled, that the single advantage of originally pushing the rod *gh* beyond the mark *w*, by the thickness of the stick, or its own thickness, is sufficient to carry the rod of the quiescent body *c* completely to its mark *x*.

There are several other experiments that may be made with this apparatus, in confirmation of the doctrine of the collision of elastic bodies ; which being universally agreed on, and well known, it is needless further to dwell on here ; but respecting the application to non-elastic soft bodies, it is far more difficult to come at a fitness of materials for this kind of experiments, than it is for those supposed perfectly elastic. The conclusions however may be attained with equal certainty.

*For trials on non-elastic soft bodies.*—For this purpose, the ratchet must be applied and put in order as before described, and the springs being both set to their point of rest, let the body *D* be put to its mark *w* in the same manner as before described, and the body *c* to rest. The body *D* being let go, and striking the body *c* at rest, in consequence of the stroke, the springs being hooked up by the ratchets, they both move from their resting marks *t*, *v*, respectively toward *m* : now if they both moved together, and the rod *ef* covered the mark *c*, and the rod *gh* covered the mark *d* at their utmost limit, then they would truly obey the laws of non-elastic soft bodies ; because their medium ascent would be to the mark *z*, which is just half\* the angle of ascent to the mark *x* ; but as in this piece of machinery, though the main or principle springs are hooked up, yet

\* It should be said *nearly* half the angle.



every part of them, and all the materials of which they are composed, and to which they are attached, have a degree, or more properly speaking, a certain compass of elasticity, which, as such, is perfect, and no motion thereby lost. We must not therefore expect the two compound bodies after the stroke to stick together without separating, as would be the case with bodies truly non-elastic and soft; but that from the elasticity they are possessed of, they will by rebounding be separated; but that elasticity being perfect, can occasion no loss of motion to the sum of the two bodies; so that if the body *c* ascends as much above its mark *c* as the body *D* falls short of its mark *d*, then it will follow, that their medium ascent will still be to the mark *z*, as it ought to have been, had they been truly non-elastic soft bodies; and this, in reality, is truly the case in the experiment, as nearly as it can be discerned.

After a few vibrations, by the rubbing of the springs against each other, they are soon brought to rest; and here they would always rest had they been truly and properly perfect non-elastic soft bodies; but here, as in the case of these bodies, by a change of the figure and situation of the component parts, there is expended one half of the mechanical power of the first mover, yet in this case the other half is not lost, but suspended, ready to be re-exerted whenever it is set at liberty; and that it is really and bonâ fide one half and neither more nor less, appears from this uncontroverted simple principle, that the power of restitution of a perfect spring is exactly equal to the power that bends it. And this may, in a certain degree, be shown to be fact by experiment, if there were any need of such a proof; for if, when the bodies are at rest after the last experiment, the two rods are lashed together near the bottom with a bit of thread, and then the ratchets unpinned and removed; on cutting the thread with a pair of scissars they will each of them rebound, *c* towards *M*, and *D* towards *N*; and if they rebounded respectively to *z* and *y*, the mechanical power exerted would be the same as it was after the stroke, when the mean of their two ascents was up to the mark *z*; but here it is not to be expected, because not only the motion lost by the friction of the ratchets is to be deducted, because it had the effect of real non-elasticity; but also the elasticity that separated them in the stroke, which was lost in the vibrations that succeeded; neither of which hindered the mean ascent to be to *z*; but yet, under all these disadvantages in the machine, if not unreasonably ill made, the rod *ef* will ascend to *d*, and *gh* to *a*: and hence I infer, as a positive truth, that in the collision of non-elastic soft bodies, one half of the mechanic power residing in the striking body is lost in the stroke.\*

\* Here Mr. S. puzzles himself again about his mechanical power. There is no real force or momentum lost by striking bodies, if they adhere and move together on the stroke; as he might easily and directly have convinced himself of, by discharging a leaden bullet into a pendulous block of wood; being both, in some degree, soft and non-elastic bodies.



Respecting bodies unelastic and perfectly hard, we must infer, that since we are unavoidably led to a conclusion concerning them, which contradicts what is esteemed a truth capable of the strictest demonstration; viz. that the velocity of the centre of gravity of no system of bodies can be changed by any collision among them, something must be assumed that involves a contradiction. This perfectly holds, according to all the established rules, both of perfectly elastic and perfectly non-elastic soft bodies; rules which must fail in the perfectly non-elastic hard bodies, if their velocity after the stroke is to the velocity of the striking body, as 1 is to the square root of 2; for then the centre of gravity of the two bodies will by the stroke acquire a velocity greater than the centre of gravity the two bodies had before the stroke in that proportion; which is proved thus.

At the outset of the striking body, the centre of gravity of the two bodies in our case will be exactly in the middle between the two; and when they meet it will have moved from their half distance to their point of contact, so that the velocity of the centre of gravity before the bodies meet will be exactly one half of the velocity of the striking body; and therefore, if the velocity of the striking body be 2, the velocity of the centre of gravity of both will be 1. After the stroke, as both bodies are supposed to move in contact, the velocity of the centre of gravity will be the same as that of the bodies; and as their velocity is proved to be the square root of 2, the velocity of their centre of gravity will be increased from 1, to the square root of 2; that is, from 1 to 1.414 &c.\*

The fair inference from these contradictory conclusions therefore is, that an unelastic hard body (perfectly so) is a repugnant idea, and contains in itself a contradiction; for to make it agree with the fair conclusions that may be drawn on each side, from clear premises, we shall be obliged to define its properties thus: that in the stroke of unelastic hard bodies they cannot possibly lose any mechanic power in the stroke; because no other impression is made than the communication of motion; and yet they must lose a quantity of mechanic power in the stroke; because, if they do not, their common centre of gravity, as above shown, will acquire an increase of velocity by their stroke on each other. In like manner, the idea of a perpetual motion perhaps, at first sight, may not appear to involve a contradiction in terms; but we shall be obliged to confess that it does, when, on examining its requisites for execution, we find we shall want bodies having the following properties; that when they are made to ascend against gravitation their absolute weight shall be less; and that when they descend by gravitation, through an equal space, their absolute weight shall be greater; which, according to all we know of nature, is a repugnant or contradictory idea.

\* Here is another false conclusion, drawn in consequence of a former mistake.



*XXII. Proceedings relative to the Accident by Lightning at Heckingham. By Dr. Blagden and Mr. Nairne. p. 355.*

The first communication is a letter to the president of the R. S. from the principal officers of the Board of Ordnance, dated Dec. 22, 1781, as follows.

SIR—Having received information that, last summer, a stroke of lightning set fire to the poor-house at Heckingham, near Norwich, notwithstanding it was armed with eight pointed conductors, we request you will communicate to us such particulars relating to that fact, as may have come to your knowledge.

(Signed) Amherst; Charles Frederick; H. Strachey; J. Kenrick.

*Sir Jos. Banks, Bart. President of the Royal Society.*

It does not appear that any particulars relating to that fact had come to the president's knowledge. However, the council of the R. S. appointed a committee of their members to inquire into the particulars, as appears by the following extracts.

*Extracts from the Minutes of the Council of the Royal Society.*

Jan. 10, 1782.—The president laid before the council a letter to him from the Board of Ordnance, acquainting him, that the poor-house at Heckingham, near Norwich, had been struck by lightning, notwithstanding it was armed with 8 pointed conductors; and requesting him to communicate to them such particulars relating to that fact as may have come to his knowledge.—Resolved, That Dr. Blagden and Mr. Nairne be requested to repair to Heckingham, and examine into the circumstances of the accident, and report thereon to the council: that they engage a draughtsman, to take such drawings as may be requisite; and that the necessary expenses be defrayed by the Society.

Feb. 7, 1782.—Dr. Blagden read to the council his and Mr. Nairne's report of the survey made by them of the poor-house at Heckingham in Norfolk, in consequence of their appointment by a former council. The said report was ordered to be read to the Society on Thursday the 14th inst. And the president was requested to transmit it immediately afterwards to the Board of Ordnance; and to desire that they would return the drawings as soon as they should have taken copies of them, or made such other use of them as they might think necessary.

Report of the Committee.—Read February 14, 1782.—To the President and Council of the Royal Society.—Gentlemen, pursuant to your resolution, appointing us a committee to examine the House of Industry at Heckingham in Norfolk, which had been struck by lightning though it was armed with conductors, we arrived there on the 21st of January. Seven months had then elapsed since the accident, yet we had the satisfaction to learn, that no material changes had been made in the conductors or the building in that period; some laths that had been burnt, some bricks and pantiles which had been damaged or thrown



down, were replaced; but we found means to procure distinct information of those repairs from the workmen who had been employed to execute them. In order to communicate a clear idea of the accident, it will be necessary to premise a general account of the building; then to represent the manner in which the conductors were applied; and, lastly, to describe the stroke of lightning, with its effects.

The general form of the building is that of the Roman letter *H* consisting of a centre range and two flanks. It stands on a gentle rising, which can by no means be termed a hill, with its front facing s. 9° w. To the western side of the west flank, and eastern side of the east flank, some lower buildings are annexed, serving as offices of different kinds; and there are two courts, one before and the other behind the house, with some small gardens and yards on each of the flanks, in all of which stand various detached offices.

A very minute description is then given of all the parts of the building of the poor-house, with various low detached and attached offices, as lean-tos, stables, yards, privies, pig-houses, &c. &c. the whole illustrated by drawings of plans and elevations in 6 engraved plates; which may well be spared on this occasion.

To all the 8 chimnies of the building they found iron rods affixed, reaching between 4 and 5 feet above the top of the chimney, pointed at the upper end, and tapering about 10 inches to that point. Each rod or bar was nearly square, measuring, on a mean, about half an inch one way, and  $\frac{4}{10}$  of an inch the other, with the angles just rounded off. These conductors were continued down the building by a succession of similar bars of iron, in general from 6 to 8 feet long, joined to each other by 2 hooks and nuts; that is, the corresponding ends of each bar being formed into a hook bent at right angles, the hook of the uppermost went into a hole of the lowermost, where it was fastened with a nut, and the hook of the lowermost went into a similar hole of the bar above, where it was fixed in the same manner; the length of each of these joints, from nut to nut, was about 2 inches. Though there were 8 of these conductors reaching above the chimnies, yet they had only 4 terminations below. For the conductors to the 2 chimnies, called *D* and *E*, being continued toward each other along the roof, united in the valley over the lead gutter there, and from that point only one conductor was continued down the valley toward the ground. In like manner the 2 conductors from the chimnies *A* and *C* united in the valley of the roof between them, and were carried down toward the ground as a single rod. All the 3 conductors from the chimnies *F*, *G*, and *H*, successively joined together, and only a single rod was continued from them down the lower part of the building. Lastly, the conductor from the chimney *B* went down single all the way, without having formed a junction with any other.

The conductors in their passage down the building being thus reduced to 4,



the gentlemen next show their 4 terminations ; which it hence appears were far from being so proper or fit as they ought to have been ; being carried but a few inches below the surface of the ground, and dry ; instead of being continued to many feet in depth, and ending in water, or very moist earth, as is generally directed in such cases, to render the conductors safe and effectual. The gentlemen, after a minute and careful examination and measurement of all the parts of the building, give a very clear and ample description of them, in their report to the Society ; but which may well be omitted, being particulars of very little consequence, and the case itself unimportant. One hip of the extreme corner of the building, at the greatest distance from the conductors, was struck and set on fire, by a very loud explosion of lightning ; but the fire was quickly extinguished, and little or no damage was sustained. The gentlemen then conclude their report as follows.

Such are the facts we were able to collect from an assiduous examination of the poor-house at Heckingham, and of those witnesses in the neighbourhood who knew any thing of the accident. We have stated the appearances as they presented themselves to us, with all the minuteness that could be preserved without too much crowding the narrative, and independently of any opinions. Whether the earth or the clouds were positive at the time ; whether the top or bottom of the hip was first affected by the stroke ; whether all the lightning took its course through the hip, or part went that way, and part through the conductor ; and how far the conductors were properly constructed, or adequately terminated ; are questions which will naturally suggest themselves to your consideration. (Signed) C. Blagden, and Edw. Nairne.

*XXIII. On the Organ of Hearing in Fishes. By John Hunter, Esq., F. R. S.*  
p. 379.

Reprinted with additions in Mr. J. H.'s *Observations on the Animal Œconomy*, 4to. 1786.

*XXIV. Of a New Electrometer. By Mr. Abraham Brook.* p. 384.

This new electrometer appears to be of very complex structure. It has a broad square wooden board, as a foot to stand on. Into this is fixed an upright glass rod, to insulate the machine from the table the foot stands on. To this upright rod are attached horizontal arms, of brass wire, terminated by large thin copper shells ; the electricity being presented to these balls, they are moved and turned round the upright rod. The degree or strength of the electricity is measured either by an index to a graduated circle, or by different weights that are raised at the extremity of another index or lever.



*XXV. A New Method of Investigating the Sums of Infinite Series. By the Rev. S. Vince, A.M., of Cambridge. p. 389.*

The subject of this paper is divided into 3 parts: the first, Mr. V. says, contains a new and general method of finding the sum of those series which De Moivre has found in one or two particular cases; but whose method, though it be in appearance general, will on trial be found to be absolutely impracticable. The 2d contains the summation of certain series, the last differences of whose numerators become equal to nothing. The 3d contains observations on a correction which is necessary in investigating the sums of certain series by collecting two terms into one, with its application to a variety of cases.

We must however content ourselves with a very abridged state of this paper; retaining only a few specimens of the several ingenious methods above-mentioned: and besides, altering a little the notation of some of the characters or ligatures, for the greater ease and simplicity in printing.

PART 1.—LEM. 1. Let  $r$  be any whole number; then the fluent of  $\frac{x}{1+x^r}$  can always be exhibited by circular arcs and logarithms; but when  $x=1$ , the fluent of the same fluxion will be expressed by the infinite series

$1 - \frac{1}{r+1} + \frac{1}{2r+1} - \frac{1}{3r+1} + \&c.$  the sum of this series therefore can always be found by circular arcs and logarithms.

LEM. 2. To find the sum of the infinite series

$$\frac{a}{1.r+1} - \frac{a+b}{r+1.2r+1} + \frac{a+2b}{2r+1.3r+1} - \&c.$$

Assume  $1 - \frac{1}{r+1} + \frac{1}{2r+1} - \frac{1}{3r+1} + \&c. = s$ ; then by several algebraical processes, Mr. V. finds the sum required to be as follows: viz.  $\frac{a}{1.r+1} - \frac{a+b}{r+1.2r+1} + \frac{a+2b}{2r+1.3r+1} - \&c. = \frac{(2ra - (r+2)b) \times s - ra + (r+1)b}{r^2}$ .

Cor. 1. Hence it appears that the sum of this series can never be exhibited in finite terms, except  $a:b$  as  $r+2:2r$ , in which case the sum is equal to  $\frac{a}{r+2}$ .

Hence, if  $a=3$ ,  $b=2$ , then  $r=1$ ;  $\therefore \frac{3}{1.2} - \frac{5}{2.3} + \frac{7}{3.4} - \&c. = 1$ .

If  $a=1$ ,  $b=4$ , then  $r=\frac{4}{3}$ ;  $\therefore \frac{5}{3.7} - \frac{9}{7.11} + \frac{13}{11.15} - \frac{17}{15.19} + \&c. = \frac{1}{6}$ .

If  $a=4$ ,  $b=3$ , then  $r=\frac{6}{5}$ ;  $\therefore \frac{4}{5.11} - \frac{7}{11.17} + \frac{10}{17.23} - \frac{13}{23.29} + \&c. = \frac{1}{20}$ .

PROP. 1.—To find the sum of

$$\frac{m}{1.r+1.2r+1} + \frac{m+n}{r+1.3r+1.4r+1} + \frac{m+2n}{4r+1.5r+1.6r+1} + \&c.$$

Every series of this kind may be resolved into the following series



$\frac{a}{1 \cdot r + 1} - \frac{a + b}{r + 1 \cdot 2r + 1} + \frac{a + 2b}{2r + 1 \cdot 3r + 1} - \frac{a + 3b}{3r + 1 \cdot 4r + 1} + \&c.$  for if we reduce two terms of this series into one, it will become

$$\frac{2ar - b}{1 \cdot r + 1 \cdot 2r + 1} + \frac{2ra + (2r - 1)b}{2r + 1 \cdot 3r + 1 \cdot 4r + 1} + \frac{2ra + (4r - 1)b}{4r + 1 \cdot 5r + 1 \cdot 6r + 1} + \&c.$$

where the denominators being the same as in the given series, and the numerators also in arithmetic progression, we have only to take  $a$  and  $b$  such quantities that the respective numerators may be also equal; assume therefore  $2ra - b = m$ ,  $2ra + (2r - 1)b = m + n$ ; therefore,  $b = \frac{n}{2r}$ ,  $a = \frac{2rm + n}{4r^2}$ , which

substituted for  $a$  and  $b$  in Lem. 2, gives  $\frac{m}{1 \cdot r + 1 \cdot 2r + 1} + \frac{m + n}{2r + 1 \cdot 3r + 1 \cdot 4r + 1} + \frac{m + 2n}{4r + 1 \cdot 5r + 1 \cdot 6r + 1} + \&c. = \frac{2rm - (r + 1)n}{2r^3} \times s + \frac{(2r + 1)n - 2rm}{4r^3}.$

Let  $r = 1$ , and we have

$$\frac{m}{1 \cdot 2 \cdot 3} + \frac{m + n}{3 \cdot 4 \cdot 5} + \frac{m + 2n}{5 \cdot 6 \cdot 7} + \&c. \dots = (m - n)s + \frac{3n - 2m}{4}.$$

$$\text{If } m = 1, n = 3, \frac{1}{1 \cdot 2 \cdot 3} + \frac{4}{3 \cdot 4 \cdot 5} + \frac{7}{5 \cdot 6 \cdot 7} + \&c. \dots = \frac{7}{4} - 2s;$$

$$m = 1, n = 0, \frac{1}{1 \cdot 2 \cdot 3} + \frac{1}{3 \cdot 4 \cdot 5} + \frac{1}{5 \cdot 6 \cdot 7} + \&c. \dots = s - \frac{1}{2}.$$

$$\text{If } m = 1, n = 1, \frac{1}{1 \cdot 6 \cdot 11} + \frac{2}{11 \cdot 16 \cdot 21} + \frac{3}{21 \cdot 26 \cdot 31} + \&c. = \frac{2}{125} \times s + \frac{1}{500};$$

$$m = 1, n = 0, \frac{1}{1 \cdot 6 \cdot 11} + \frac{1}{11 \cdot 16 \cdot 21} + \frac{1}{21 \cdot 26 \cdot 31} + \&c. = \frac{s}{25} - \frac{1}{50}.$$

Cor. If  $2r : r + 1 :: n : m$ , the sum of the series can be accurately found, and will be equal to  $\frac{m}{2r(r + 1)}$ . Let therefore  $m = r + 1$ , and then  $n = 2r$ ,

consequently  $\frac{1}{1 \cdot 2r + 1} + \frac{1}{2r + 1 \cdot 4r + 1} + \frac{1}{4r + 1 \cdot 6r + 1} + \&c. \dots = \frac{1}{2r}$ ; which is also known from other principles.

PROP. 2.—To find the sum of

$$\frac{m}{r + 1 \cdot 2r + 1 \cdot 3r + 1} + \frac{m + n}{3r + 1 \cdot 4r + 1 \cdot 5r + 1} + \frac{m + 2n}{5r + 1 \cdot 6r + 1 \cdot 7r + 1} + \&c.$$

Proceeding much in the same manner as in prob. 1, Mr. V. finds that the sum of this infinite series is equal to

$$\frac{(2r + 1)n - 2rm}{2r^3} \times s + \frac{2rm - (3r + 1)n}{4r^3} + \frac{2rm - (r - 1)n}{4r^2 \cdot r + 1}$$

Cor. In prop. 1, substitute  $a$  for  $m$ , and  $2b$  for  $n$ , and we have

$$\frac{a}{1 \cdot r + 1 \cdot 2r + 1} + \frac{a + 2b}{2r + 1 \cdot 3r + 1 \cdot 4r + 1} + \frac{a + 4b}{4r + 1 \cdot 5r + 1 \cdot 6r + 1} + \&c. = \frac{ra - (r + 1)b}{r^3} \times s + \frac{(2r + 1)b - ra}{2r^3}.$$

Also in this prop. substitute  $a + b$  for  $m$ , and  $2b$  for  $n$ , and we have

$$\frac{a + b}{r + 1 \cdot 2r + 1 \cdot 3r + 1} + \frac{a + 3b}{3r + 1 \cdot 4r + 1 \cdot 5r + 1} + \&c. = \frac{(r + 1)b - ra}{r^3} \times s + \frac{ra + b}{2r^3} + \frac{ra + b}{2r^2 \times (r + 1)}.$$



Subtract this latter series from the former, and then

$$\frac{a}{1 \cdot r + 1 \cdot 2r + 1} - \frac{a+b}{r + 1 \cdot 2r + 1 \cdot 3r + 1} + \frac{a+2b}{2r + 1 \cdot 3r + 1 \cdot 4r + 1} - \&c. =$$

$$\frac{2ra - (r+1)2b}{r^3} \times s + \frac{(2r+1)b - ra}{r^3} - \frac{ra+2b}{2r^2 \times (r+1)}.$$

Let  $r = 1$ , and we have

$$\frac{a}{1 \cdot 2 \cdot 3} - \frac{a+b}{2 \cdot 3 \cdot 4} + \frac{a+2b}{3 \cdot 4 \cdot 5} - \&c. \dots = (2a - 4b) \times s + \frac{11b - 5a}{4}.$$

If  $a = 1, b = 0, \frac{1}{1 \cdot 2 \cdot 3} - \frac{1}{2 \cdot 3 \cdot 4} + \frac{1}{3 \cdot 4 \cdot 5} - \&c. \dots = 2s - \frac{5}{4};$

$a = 1, b = 2, \frac{1}{1 \cdot 2 \cdot 3} - \frac{3}{2 \cdot 3 \cdot 4} + \frac{5}{3 \cdot 4 \cdot 5} - \&c. \dots = \frac{17}{4} - 6s.$

PROP. 3.—To find the sum of the infinite series  $\frac{m}{1 \cdot r + 1 \cdot 2r + 1 \cdot 3r + 1} +$   
 $\frac{m+n}{2r + 1 \cdot 3r + 1 \cdot 4r + 1 \cdot 5r + 1} + \frac{m+2n}{4r + 1 \cdot 5r + 1 \cdot 6r + 1 \cdot 7r + 1} + \&c.$  By a si-  
 milar process Mr. V. finds the sum of this infinite series will be generally ex-  
 pressed by  $\frac{4rm - (3+2)n}{6r^4} \times s + \frac{(3r+1)n - 2rm}{6r^4} - \frac{rm+n}{6r^3 \cdot (r+1)}.$

Let  $r = 1$ , and we have

$$\frac{m}{1 \cdot 2 \cdot 3 \cdot 4} + \frac{m+n}{3 \cdot 4 \cdot 5 \cdot 6} + \frac{m+2n}{5 \cdot 6 \cdot 7 \cdot 8} + \&c. \dots = \frac{4m - 5n}{6} \times s + \frac{7n - 5m}{12}.$$

If  $m = 1, n = 1, \frac{1}{1 \cdot 2 \cdot 3 \cdot 4} + \frac{1}{3 \cdot 4 \cdot 5 \cdot 6} + \frac{1}{5 \cdot 6 \cdot 7 \cdot 8} + \&c. \dots = \frac{2}{3}s - \frac{5}{12};$

$m = 1, n = 1, \frac{1}{1 \cdot 2 \cdot 3 \cdot 4} + \frac{2}{3 \cdot 4 \cdot 5 \cdot 6} + \frac{3}{5 \cdot 6 \cdot 7 \cdot 8} + \&c. \dots = \frac{1}{6} - \frac{1}{6}s;$

$m = 7, n = 5, \frac{7}{1 \cdot 2 \cdot 3 \cdot 4} + \frac{12}{3 \cdot 4 \cdot 5 \cdot 6} + \frac{17}{5 \cdot 6 \cdot 7 \cdot 8} + \&c. \dots = \frac{1}{2}s;$

Cor. If  $n : m$  as  $4r : 3r + 2$ , the sum of the series can be accurately had;  
 let therefore  $n = 4r$  and  $m = 3r + 2$ , and we shall have

$$\frac{3r+2}{1 \cdot r + 1 \cdot 2r + 1 \cdot 3r + 1} + \frac{7r+2}{2r + 1 \cdot 3r + 1 \cdot 4r + 1 \cdot 5r + 1} + \&c. \dots = \frac{1}{2r \cdot r + 1}.$$

If  $r = 1, \frac{5}{1 \cdot 2 \cdot 3 \cdot 4} + \frac{9}{3 \cdot 4 \cdot 5 \cdot 6} + \frac{13}{5 \cdot 6 \cdot 7 \cdot 8} + \&c. \dots = \frac{1}{4};$

$r = 2, \frac{1}{1 \cdot 3 \cdot 5 \cdot 7} + \frac{2}{5 \cdot 7 \cdot 9 \cdot 11} + \frac{3}{9 \cdot 11 \cdot 13 \cdot 15} + \&c. \dots = \frac{1}{96};$

$r = 6, \frac{5}{1 \cdot 7 \cdot 13 \cdot 19} + \frac{11}{13 \cdot 19 \cdot 25 \cdot 31} + \frac{17}{25 \cdot 31 \cdot 37 \cdot 43} + \&c. \dots = \frac{1}{336}.$

PROP. 4.—To find the sum of the infinite series  $\frac{m}{r + 1 \cdot 2r + 1 \cdot 3r + 1 \cdot 4r + 1}$

$$+ \frac{m+n}{3r + 1 \cdot 4r + 1 \cdot 5r + 1 \cdot 6r + 1} + \frac{m+2n}{5r + 1 \cdot 6r + 1 \cdot 7r + 1 \cdot 8r + 1} + \&c.$$

By a like process also Mr. V. finds that this series is =

$$\frac{(5r+2)n - 4rm}{6r^4} \times s + \frac{2rm - (4r+1)n}{6r^4} + \frac{2rm - (r-2)n}{12 \cdot r^3 \cdot (r+1)} + \frac{4rm - (2r-1)n}{12r^2 \cdot (r+1) \cdot (2r+1)}$$

Cor. 1. By a like process also it is found that

$$\frac{a}{1 \cdot r + 1 \cdot 2r + 1 \cdot 3r + 1} - \frac{a+b}{r + 1 \cdot 2r + 1 \cdot 3r + 1 \cdot 4r + 1} + \&c. =$$

$$\frac{4ra - (3r+2)2b}{3r^4} \times s + \frac{(3r+1)2b - 2ra}{3r^4} - \frac{ra+2b}{r^3 \cdot (r+1)} - \frac{2ra+b}{6r^2 \cdot (r+1) \cdot (2r+1)}.$$



Let  $r = 1$ , and we have

$$\frac{a}{1 \cdot 2 \cdot 3 \cdot 4} - \frac{a+b}{2 \cdot 3 \cdot 4 \cdot 5} + \frac{a+2b}{3 \cdot 4 \cdot 5 \cdot 6} - \&c. \dots = \frac{4a-10b}{3} \times s + \frac{83b-32a}{36}.$$

If  $a = 1, b = 0$ ,  $\frac{1}{1 \cdot 2 \cdot 3 \cdot 4} - \frac{1}{2 \cdot 3 \cdot 4 \cdot 5} + \frac{1}{3 \cdot 4 \cdot 5 \cdot 6} - \&c. \dots = \frac{4}{3}s - \frac{8}{9}.$

$a = 3, b = 1$ ,  $\frac{1}{1 \cdot 2 \cdot 4} - \frac{1}{2 \cdot 3 \cdot 5} + \frac{1}{3 \cdot 4 \cdot 6} - \&c. \dots = \frac{2}{3}s - \frac{13}{36}.$

*Cor. 2.* If  $a : b$  as  $3r + 2 : 2r$ , the sum of the series can be accurately found; take  $\therefore a = 3r + 2$ , and  $b = 2r$ , and we shall have

$$\frac{3r+2}{1 \cdot r + 1 \cdot 2r + 1 \cdot 3r + 1} - \frac{5r+2}{r + 1 \cdot 2r + 1 \cdot 3r + 1 \cdot 4r + 1} + \&c. \dots = \frac{1}{(r+1) \cdot (2r+1)}$$

If  $r = 1$ ,  $\frac{5}{1 \cdot 2 \cdot 3 \cdot 4} - \frac{7}{2 \cdot 3 \cdot 4 \cdot 5} + \frac{9}{3 \cdot 4 \cdot 5 \cdot 6} - \&c. \dots = \frac{1}{60};$

$r = 2$ ,  $\frac{2}{1 \cdot 3 \cdot 5 \cdot 7} - \frac{3}{3 \cdot 5 \cdot 7 \cdot 9} + \frac{4}{5 \cdot 7 \cdot 9 \cdot 11} - \&c. \dots = \frac{1}{15};$

$r = 3$ ,  $\frac{11}{1 \cdot 4 \cdot 7 \cdot 10} - \frac{17}{4 \cdot 7 \cdot 10 \cdot 13} + \frac{23}{7 \cdot 10 \cdot 13 \cdot 16} - \&c. \dots = \frac{1}{28};$

**PROP. 5.**—To find the sum of the infinite series  $\frac{m}{1 \cdot r + 1 \cdot 2r + 1 \cdot 3r + 1 \cdot 4r + 1} + \frac{m+n}{2r + 1 \cdot 3r + 1 \cdot 4r + 1 \cdot 5r + 1 \cdot 6r + 1} + \frac{m+2n}{4r + 1 \cdot 5r + 1 \cdot 6r + 1 \cdot 7r + 1 \cdot 8r + 1} + \&c.$

This series also in like manner is found equal to

$$\frac{2rm - (2r+1)n}{6r^3} \times s + \frac{(4r+1)n - 2rm}{12r^3} - \frac{6rm + 9n}{72 \cdot r^4 (r+1)} - \frac{2rm + n}{24r^3 \cdot (r+1) \cdot (2r+1)}.$$

Let  $r = 1$ , and we have

$$\frac{m}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} + \frac{m+n}{3 \cdot 4 \cdot 5 \cdot 6 \cdot 7} + \frac{m+2n}{5 \cdot 6 \cdot 7 \cdot 8 \cdot 9} + \&c. \dots = \frac{2m-3n}{6} \times s + \frac{25n-16m}{72}.$$

If  $m = 1, n = 0$ ,  $\frac{1}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} + \frac{1}{3 \cdot 4 \cdot 5 \cdot 6 \cdot 7} + \frac{1}{5 \cdot 6 \cdot 7 \cdot 8 \cdot 9} + \&c. \dots = \frac{1}{3}s - \frac{2}{9};$

$m = 1, n = 1$ ,  $\frac{1}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} + \frac{2}{3 \cdot 4 \cdot 5 \cdot 6 \cdot 7} + \frac{3}{5 \cdot 6 \cdot 7 \cdot 8 \cdot 9} + \&c. \dots = \frac{1}{8} - \frac{1}{6}s;$

$m = 4, n = 2$ ,  $\frac{1}{1 \cdot 2 \cdot 3 \cdot 5} + \frac{1}{3 \cdot 4 \cdot 5 \cdot 7} + \frac{1}{5 \cdot 6 \cdot 7 \cdot 9} + \&c. \dots = \frac{1}{3}s - \frac{7}{36}.$

$m = 25, n = 16$ ,  $\frac{25}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} + \frac{41}{3 \cdot 4 \cdot 5 \cdot 6 \cdot 7} + \frac{57}{5 \cdot 6 \cdot 7 \cdot 8 \cdot 9} + \&c. \dots = \frac{1}{3}s.$

*Cor.* If  $n : m$  as  $2r : 2r + 1$ , the sum of the series can be accurately found; assume therefore  $n = 2r$ ,  $m = 2r + 1$ , and we have

$$\frac{1}{1 \cdot r + 1 \cdot 3r + 1 \cdot 4r + 1} + \frac{1}{2r + 1 \cdot 3r + 1 \cdot 5r + 1 \cdot 6r + 1} + \&c. \dots = \frac{1}{6 \cdot r \cdot (r+1) \cdot (2r+1)}$$

If  $r = 1$ ,  $\frac{1}{1 \cdot 2 \cdot 4 \cdot 5} + \frac{1}{3 \cdot 4 \cdot 6 \cdot 7} + \frac{1}{5 \cdot 6 \cdot 8 \cdot 9} + \&c. \dots = \frac{1}{36};$

$r = 3$ ,  $\frac{1}{1 \cdot 4 \cdot 10 \cdot 13} + \frac{1}{7 \cdot 10 \cdot 16 \cdot 19} + \frac{1}{13 \cdot 16 \cdot 22 \cdot 25} + \&c. \dots = \frac{1}{504}.$

Mr. V. concludes this part with pointing out a remarkable property of those series whose sum can be accurately found: viz. that when the number of factors in the denominator is even, the numerator is always equal to the sum of the two middle factors; and when the number of factors is odd, the numerator is equal to the middle factor, and consequently will take it out of the denominator,



and leave a series whose numerators are unity, and whose denominators want the middle factor.

PART II. PROP.—To find the sum of the infinite series

$\frac{p}{n(n+m)\dots n+rm} + \frac{q}{n+m\dots n+(r+1)m} + \frac{s}{n+2m\dots n+(r+2)m} + \&c.$   
when the last differences of the numerators become equal to nothing.

Assume  $a + nb + n(n+m).c + n(n+m).(n+2m).d + \&c.$  to any number ( $r'$ ) of terms; then, if for  $n$  we write  $n+m$ ,  $n+2m$ ,  $n+3m$ ,  $\&c.$  successively, there will result a series of quantities whose  $r'$ th difference is  $= 0$ ; substitute therefore this series of quantities for  $p$ ,  $q$ ,  $s$ ,  $\&c.$  respectively, and the given series becomes of a form which manifestly resolves itself into several other series, the number of which is  $r'$ ; and the sum of each of these being taken by a well known rule, the sum of the given series becomes

$\frac{a}{n(n+m)\dots(n+(r-1)m).m.r} + \frac{b}{n+m\dots(n+(r-1)m).m.(r-1)} + \frac{c}{n+2m\dots(n+(r-1)m).m(r-2)} + \&c.$  where the law of continuation is manifest.

*Exam.* To find the sum of the infinite series

$$\frac{3}{1.2.3.4} + \frac{6}{2.3.4.5} + \frac{10}{3.4.5.6} + \frac{15}{4.5.6.7} + \&c.$$

Here  $n = 1$ ,  $m = 1$ ,  $r = 3$ , and the 3d differences become  $= 0$ ; therefore  $a + b + 2c = 3$ ,  $a + 2b + 6c = 6$ ,  $a + 3b + 12c = 10$ , consequently  $a = 1$ ,  $b = 1$ ,  $c = \frac{1}{2}$ , and therefore the sum sought will be

$$\frac{1}{1.2.3.3} + \frac{1}{2.3.2} + \frac{1}{2.3} = \frac{11}{36}.$$

This proposition may also be applied to find the sum of all those series whose numerators being unity, the denominators shall be deficient by any number of corresponding terms, however taken: for as the product of all such factors must form a progression, whose differences will become equal to nothing, if such products be assumed for the numerators of the given series having its factors completed, another series will be formed equal to the given series, whose sum can be found by this proposition.

*Exam.* To find the sum of the infinite series

$$\frac{1}{1.2.4.6} + \frac{1}{2.3.5.7} + \frac{1}{3.4.6.8} + \&c.$$

By completing the factors in the denominators, and multiplying the numerators by the same quantities, the given series becomes

$\frac{15}{1.2.3.4.5.6} + \frac{24}{2.3.4.5.6.7} + \frac{35}{3.4.5.6.7.8} + \&c.$  in which case  $n = 1$ ,  $m = 1$ ,  $r = 5$ , and the 3d differences become  $= 0$ ; therefore  $a + b + 2c = 15$ ,  $a + 2b + 6c = 24$ ,  $a + 3b + 12c = 35$ , consequently  $a = 8$ ,  $b = 5$ ,  $c = 1$ , and therefore the sum of the series required is

$$\frac{8}{1.2.3.4.5.5} + \frac{5}{2.3.4.5.4} + \frac{1}{3.4.5.3} = \frac{211}{7200}.$$

By this proposition we may also investigate the sum of the series when there



are any number of deficient terms in the denominators, and where the last differences of the numerators become equal to nothing; for if the factors in the denominators be completed, and the numerators be multiplied by the same quantities, their differences will still become equal to nothing.

*Exam.* To find the sum of the infinite series

$$\frac{1}{1.3.4.6} + \frac{3}{2.4.5.7} + \frac{6}{3.5.6.8} + \frac{10}{4.6.7.9} + \frac{15}{5.7.8.10} + \&c.$$

This series, by completing the factors in the denominators and multiplying the numerators by the same quantities, becomes

$$\frac{10}{1.2.3.4.5.6} + \frac{54}{2.3.4.5.6.7} + \frac{168}{3.4.5.6.7.8} + \&c. \text{ in which case } n = 1, m = 1, r = 5, \text{ and as the 5th differences are } = 0,$$

Therefore  $a + b + 2c + 6d + 24e = 10$ ,  $a + 2b + 6c + 24d + 120e = 54$ ,  $a + 3b + 12c + 60d + 360e = 168$ ,  $a + 4b + 20c + 120d + 840e = 400$ ,  $a + 5b + 30c + 210d + 1680e = 810$ ; whence  $a = 0$ ,  $b = 0$ ,  $c = -1$ ,  $d = 0$ ,  $e = \frac{1}{2}$ ; consequently the sum of the given series is = —

$$\frac{1}{3.4.5.6} + \frac{1}{5.2} = \frac{17}{180}.$$

By a method similar to that made use of in this proposition may any number of factors be taken from the denominators of those series delivered in part the 1st, and also from a great variety of others; but as the examples here given must be sufficient to point out the method of proceeding in all other cases, we may proceed to the 3d part.

**PART 3.**—The sum of every converging infinite series, whose terms ultimately become equal to nothing, may always be exhibited by the sum of another series formed by collecting 2 or more terms of the former series into one. This is not true however where the terms of the infinite series continually diverge, or converge to any assignable quantity, and are affected with the signs  $+$ ,  $-$ , alternately: for instance, the series  $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} - \frac{4}{5} + \&c.$  if we collect 2 terms into one, beginning at the 1st term, will become  $-\frac{1}{2.3} - \frac{1}{4.5} - \frac{1}{6.7} + \&c.$  If we begin at the 2d term, it becomes  $\frac{1}{1.2} + \frac{1}{3.4} + \frac{1}{5.6} + \&c.$ ; neither of which gives the sum of the assumed series; but in this, and every other case of the like nature, a correction will be necessary: to determine the value of which, and whence the necessity of it arises, is the subject of this 3d part.

**LEMMA.**—If  $r$  be any quantity whatever, then will  $\frac{1}{2r} = \frac{1}{r} - \frac{1}{r} + \frac{1}{r} - \frac{1}{r} + \&c.$  ad infinitum.

For  $\frac{1}{2r} = \frac{1}{r+r} =$  (by common division)  $\frac{1}{r} - \frac{1}{r} + \frac{1}{r} - \frac{1}{r} + \&c.$  ad infinitum.

**Cor. 1.** Hence  $-\frac{1}{2r} = -\frac{1}{r} + \frac{1}{r} - \frac{1}{r} + \frac{1}{r} - \&c.$  ad infinitum.



Cor. 2. Hence also  $\frac{z}{2v} = \frac{z}{v} - \frac{z}{v} + \frac{z}{v} - \frac{z}{v} + \&c.$  ad infinitum.

and  $-\frac{z}{2v} = -\frac{z}{v} + \frac{z}{v} - \frac{z}{v} + \frac{z}{v} - \&c.$  ad infinitum.

PROP. 1.—If  $\frac{n}{rn+m}$  be the general term of a series formed by writing for  $n$  any series of numbers in arithmetic progression, and whose signs are alternately  $+$  and  $-$ ; then if a series be formed by collecting two terms into one, beginning at the first term, the sum of the series thence arising will be less than the sum of the given series by  $\frac{1}{2r}$ . If a series be formed by beginning at the second term, the sum of this will be greater than the sum of the given series by  $\frac{1}{2r}$ .

For let  $\frac{n}{rn+m} - \frac{n+a}{(n+a)r+m}$  be any two successive terms of the series, which, if we begin to collect at the 1st term, that term being  $+$ , will be two terms to be collected into one, and which will therefore give  $\frac{-am}{(rn+m) \times [(n+a)r+m]}$  for a general term of the resulting series. Let us now make  $n$  infinite, and then the denominator of this term becomes infinite, and the numerator finite; therefore the terms of this latter series at an infinite distance becoming infinitely small, the series will there terminate. Now, by making  $n$  infinite in the given series, the two successive general terms at an infinite distance become  $\frac{1}{r} - \frac{1}{r}$ ; consequently this series is still continued after the other terminates; and the terms of such a continuation will be (as they begin with

$\frac{n}{rn+m} - \frac{n+a}{(n+a)r+m}$  by making  $n$  infinite)  $\frac{1}{r} - \frac{1}{r} + \frac{1}{r} - \frac{1}{r} + \&c.$  which will also be continued ad infin. and whose sum by the lemma is  $\frac{1}{2r}$ ; consequently the

given series exceeds that which is formed by collecting two terms into one, beginning at the first, by  $\frac{1}{2r}$ ; hence the sum of the latter series  $+$   $\frac{1}{2r}$  will be equal to the sum of the former. If we begin to collect at the 2d term, then will  $-\frac{n}{rn+m} + \frac{n+a}{(n+a)r+m}$  be the two successive general terms of the given series to

be collected into one; consequently the continuation of the given series, when  $n$  becomes infinite, will be  $-\frac{1}{r} + \frac{1}{r} - \frac{1}{r} + \frac{1}{r} - \&c.$  ad infinitum, whose sum

by cor. 1 to the lem. is  $-\frac{1}{2r}$ ; in this case therefore, the sum of the given series is less than the sum of the series formed by collecting 2 terms into one, beginning at the 2d term, by  $\frac{1}{2r}$ ; hence the sum of the latter series  $-\frac{1}{2r}$  will be equal to the sum of the former.

Exam. Let the given series be  $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} - \frac{4}{5} + \&c.$

Here  $r = 1$ ,  $n = 1, 2, 3, 4, \&c.$  and  $m = 1$ . Now, if we begin to collect



at the 1st term, the series resolves itself into  $-\frac{1}{2.3} - \frac{1}{4.5} - \frac{1}{6.7} - \&c.$  and the correction, to be added, being  $\frac{1}{2}$ , we have  $-\frac{1}{2.3} - \frac{1}{4.5} - \frac{1}{6.7} - \&c. + \frac{2}{1}$  for the sum of the given series. Now  $-\frac{1}{2.3} - \frac{1}{4.5} - \frac{1}{6.7} - \&c.$  is well known to be equal to  $-1 + \text{hyp. log. of } 2$ ; consequently the sum of the given series is  $= -\frac{1}{2} + \text{hyp. log. of } 2$ .

If we begin to collect at the 2d term, the series becomes  $\frac{1}{1.2} + \frac{1}{3.4} + \frac{1}{5.6} + \&c.$  and the correction, to be subtracted, being  $\frac{1}{2}$  we have  $\frac{1}{1.2} + \frac{1}{3.4} + \frac{1}{5.6} + \&c. - \frac{1}{2}$  for the sum of the given series; but  $\frac{1}{1.2} + \frac{1}{3.4} + \frac{1}{5.6} + \&c.$  is equal to the hyp. log. of 2; therefore the sum of the given series is  $= -\frac{1}{2} + \text{hyp. log. of } 2$ , the same as before.

PROP. 2.—If  $\frac{x+nz}{w+nv}$  be the general term of a series formed by writing for  $n$  any series of numbers in arithmetic progression, and whose terms are alternately  $+$  and  $-$ ; then if a series be formed by collecting two terms into one, beginning at the first term, the sum of the series thence arising will be less than the sum of the given series by  $\frac{z}{2v}$ . If a series be formed by beginning at the second term, the sum of it will be greater than the sum of the given series by  $\frac{z}{2v}$ .

This is proved in a manner similar to the former.

*Exam.* Let the given series be  $\frac{7}{3} - \frac{11}{5} + \frac{15}{7} - \frac{19}{9} + \&c.$

Here  $x = 3$ ,  $z = 2$ ,  $w = 1$ ,  $v = 1$ ,  $n = 2, 4, 6, 8, \&c.$  Now, if we begin to collect at the first term, the series becomes  $\frac{2}{3.5} + \frac{2}{7.9} + \frac{2}{11.13} + \&c.$  and the correction, to be added, being 1, we have  $\frac{2}{3.5} + \frac{2}{7.9} + \frac{2}{11.13} + \&c. + 1$  for the sum of the given series; but if  $\Lambda =$  a circular arc of  $45^\circ$  whose radius is unity, it is well known that  $\frac{2}{3.5} + \frac{2}{7.9} + \frac{2}{11.13} + \&c. = 1 - \Lambda$ ; therefore the sum of the given series is  $2 - \Lambda$ .

Besides the series contained in the foregoing propositions, a great variety of other series might be produced where a correction is necessary, after collecting two terms into one, in order to exhibit the true value of the given series. As the proper correction however may always be found from the principles delivered in the above propositions, that is, by considering what the terms of the given series become at an infinite distance, Mr. V. only adds one or two instances more, and concludes what he at present intended to offer on this subject.

*Exam. 1.* Required to find the sum of the infinite series

$$\frac{3.4}{1.2} - \frac{4.5}{2.3} + \frac{5.6}{3.4} - \frac{6.7}{4.5} + \&c.$$



This, by resolving two terms into one, becomes  $\frac{16}{1.2.3} + \frac{24}{3.4.5} + \frac{32}{5.6.7} - \&c.$ ; and as the terms of the given series continually approach to unity, the correction, to be added, is  $\frac{1}{2}$ , consequently  $\frac{16}{1.2.3} + \frac{24}{3.4.5} + \frac{32}{5.6.7} - \&c. + \frac{1}{2}$  is equal to the sum of the given series; but by prop. 1 part 1, the sum of the series  $\frac{16}{1.2.3} + \frac{24}{3.4.5} + \frac{32}{5.6.7} + \&c.$  is equal to  $8s - 2$  ( $s$  being the hyp. log. 2) consequently the sum of the given series is  $8s - 1\frac{1}{2}$ .

*Exam. 2.* Required to find the sum of the infinite series

$$\frac{1.2}{1.3} - \frac{2.3}{3.5} + \frac{3.4}{5.7} - \frac{4.5}{7.9} + \&c.$$

This series, by resolving two terms into one, becomes  $\frac{4}{1.3.5} + \frac{8}{5.7.9} + \frac{12}{9.11.13} + \&c.$  and as the terms of the given series continually approach to  $\frac{1}{4}$ , the correction, to be added, will be  $\frac{1}{8}$ , therefore  $\frac{4}{1.3.5} + \frac{8}{5.7.9} + \frac{12}{9.11.13} + \&c. + \frac{1}{8}$  is = to the sum of the given series; but by prop. 1 part 1, the sum of  $\frac{4}{1.3.5} + \frac{8}{5.7.9} + \frac{12}{9.11.13} + \&c.$  is equal to  $\frac{1}{4}s + \frac{1}{8}$  ( $s$  being a circular arc of  $45^\circ$ , whose radius is unity) hence the sum of the given series is  $\frac{1}{4}s + \frac{1}{4}$ .

This method is not only applicable to those cases, where the given series resolves itself into another, whose sum is either accurately known or can be expressed by circular arcs and logarithms, but also to those cases where we want to approximate to the value of the given series, as it must, in general, be necessary first to render the terms of the series converging, by collecting two into one, before the operation of approximation begins, and consequently a correction of this latter is necessary in order to exhibit the value of the given series.

## XXVI. A new Method of finding the Equal Roots of an Equation by Division.

By the Rev. John Hellins, Curate of Constantine, in Cornwall. p. 417.

**THEOREM 1.**—If the cubic equation  $x^3 - px^2 + qx - r = 0$  has two equal roots, each of them will be  $x = \frac{pq - 9r}{2pp - 6q}$ .

*Demonst.* For, call the 3 roots  $a$ ,  $a$ , and  $b$ ; then, by the composition of equations we shall have  $x^3 - \frac{2a}{b} \left\{ x^2 + \frac{aa}{2ab} \right\} x - aab = 0$ , where  $2a + b = p$ ,  $aa + 2ab = q$ , and  $aab = r$ ; which values being written in our theorem, we have  $x (= \frac{pq - 9r}{2pp - 6q}) = a$ .

*Exam.* If the equation  $x^3 + 5x^2 - 32x + 36 = 0$  has two equal roots, it is proposed to find them by the above theorem.

Here  $p = -5$ ,  $q = -32$ , and  $r = -36$ ; these values being written in the theorem, we have  $\frac{-5 \times -32 - 9 \times -36}{2 \times 25 - 6 \times -32} = \frac{160 + 324}{50 + 192} = \frac{484}{242} = 2$ , which be-



ing written for  $x$ , the equation becomes  $8 + 20 - 64 + 36$ , which is evidently  $= 0$ ; consequently 2 and 2 are roots of it.

Otherwise 2, the value of  $x$  given by the theorem, being written for it in the quadratic equation  $3x^2 + 10x - 32 = 0$ , the result is  $12 + 20 - 32 = 0$ .

Or, dividing the given cubic by the quadratic  $(x - 2)^2 = x^2 - 4x + 4$ , the quotient is  $x + 9$ ; therefore the 3 roots are 2, 2, and  $-9$ .

**THEOREM 2.**—If the biquadratic equation  $x^4 - px^3 + qx^2 - rx + s = 0$  has two equal roots, make  $A = \frac{12r - 2pq}{3pp - 8q}$ ,  $B = \frac{pr - 16s}{3pp - 8q}$ ,  $C = \frac{4B - 2q}{4A + 3p}$ , and  $D = \frac{r}{4A + 3p}$ , and you will have  $x = \frac{D - B}{A - C}$ .

The investigation of this is given by Mr. Hellins.

*Exam.* If the equation  $x^4 * - 9x^2 + 4x + 12 = 0$  has equal roots, it is proposed to find them.

Here  $p = 0$ ,  $q = -9$ ,  $r = -4$ , and  $s = 12$ ; hence  $A = \frac{12 \times -4}{-8 \times -9} = \frac{-2}{3}$ ;  $B = \frac{-16 \times 12}{-8 \times -9} = \frac{-8}{3}$ ;  $C = \frac{4 \times \frac{-8}{3} + 18}{4 \times \frac{-2}{3}} = \frac{-11}{4}$ ;  $D = \frac{-4}{\frac{-8}{3}} = \frac{3}{2}$ ; and  $\frac{D - B}{A - C} = \frac{\frac{3}{2} + \frac{8}{3}}{\frac{-2}{3} + \frac{11}{4}} = \frac{18 + 32}{-8 + 33} = \frac{50}{25} = 2$ ; which being written for  $x$ , the equation becomes  $16 - 36 + 8 + 12 = 0$ ; therefore 2 is one of the roots.

**THEOREM 3.**—If the sursolid equation  $x^5 - px^4 + qx^3 - rx^2 + sx - t = 0$  has two roots equal to each other, and you make  $A = \frac{15r - 3pq}{4pp - 10q}$ ,  $B = \frac{2pr - 20s}{4pp - 10q}$ ,  $C = \frac{25t - ps}{4pp - 10q}$ ,  $D = \frac{5B - 3q}{5A + 4p}$ ,  $E = \frac{5C + 2r}{5A + 4p}$ ,  $F = \frac{s}{5A + 4p}$ ,  $G = \frac{B - E}{A - D}$ ,  $H = \frac{F + C}{A - D}$ ,  $I = \frac{B - H}{A - G}$ , and  $K = \frac{C}{A - G}$ , then shall one of the equal values of  $x$  be  $= \frac{H - K}{I - G}$ .

The investigation of this theorem is altogether similar to that of the last.

*Exam.* Given  $x^5 + x^3 - x^2 + 0.09433 = 0$ , to find  $x$ , two values of it being equal to each other.

Here  $p = 0$ ,  $q = 1$ ,  $r = 1$ ,  $s = 0$ ,  $t = -0.09433$ , and we get

$A = -1.5$	$E = -0.4238$	$I = -0.0972$
$B = 0$	$F = 0$	$K = -0.185$
$C = +0.2358$	$G = -0.2231$	and $x = \frac{H - K}{I - G} = 0.48.$
$D = +0.4$	$H = -0.1241$	

It has indeed been supposed, that the number of equations that have equal roots is but small, and consequently that the chief use of the rules for finding their roots, is to get limits and approximations to the roots of equations in general. That use, it must be allowed, were it the only one, is sufficient to pay for investigating them. But if the equations that have equal roots should hereafter be found not so few as has been generally received, then the use of the above theorems will become more extensive.



*XXVII. Some further Considerations on the Influence of the Vegetable Kingdom on the Animal Creation. By John Ingenhousz, F. R. S., &c. p. 426.*

On being informed, Dr. I. says, a few months ago, as well by private letters as from the Critical Review, that his doctrine was quite overturned by the 5th volume of Dr. Priestley, and by an experiment quoted in Mr. Cavallo's book on Air; he invited some friends to assist in some decisive experiments, here related. He told them the whole result which was to be expected from them, if his system was founded on nature, explaining to them before-hand the theory of these results, and promising, at the same time, that, if the result should fail, he should himself be the first to discredit his own system. Dr. I. had, he says, the satisfaction to convince them that the result did fully answer his prediction and expectation. These experiments are the following, all made in a hot-house of the Botanical Garden in the winter of 1782.

Dr. I. exposed to the sun-shine 6 globular glass vessels, each containing about 160 cubic inches of space, all filled with pump-water, which was boiled during more than 2 hours, and poured quite hot into the glass vessels, on purpose to prevent any access of air to the water.

*Exper. 1.*—In 2 of these vessels he put as much of the *conferva rivularis* (a water plant, classed by Linnæus among the cryptogamia) as was sufficient to take up the space of about an inch square.—*Exper. 2.* In the next 2 vessels he suspended, by threads tied to bits of cork, some pieces of woollen and silk cloth of different colours, as white, scarlet, green, and brown, having previously wetted them in some boiled water, on purpose to free them from all air.—*Exper. 3.* In the 2 remaining vessels he placed nothing at all.—*Exper. 4.* In another vessel of the same form and size he put some of the *conferva rivularis*, and filled it with pump-water. All these globular vessels were inverted, with their orifices immersed in vessels filled with quicksilver, for the purpose of preventing effectually any communication between the contents of the vessels and the atmosphere.

*Result of Exper. 1.*—The first 2 days neither of the vessels contained any air, and even the small quantity of air, which here and there adhered in the form of a bubble to the fibres of the vegetable when it was shut up in the vessel, had entirely disappeared. The 3d day, in the morning, some air bubbles began to rise from every part of the *conferva* in both glasses; and in the afternoon of the same day, a great quantity of air bubbles rose continually from it. Dr. I. took at that time the vegetable out of one of these vessels, and plunged a wax taper, just extinguished, into the orifice of this vessel, to see if the air, already extricated from the *conferva*, was dephlogisticated. The wax taper took flame immediately with an uncommon splendour. After this he poured half of the water from the globular vessel into a common bottle, and corked it. He inverted this bottle afterwards in an earthen vessel filled with boiled water; and



placed this apparatus near the fire till the water in the bottle began to boil; after which he cooled the whole, and found a good quantity of air collected in the bottle, which proved to be dephlogisticated. When he drew the vegetable out of the glass vessel the water sparkled almost like Seltzer water, or like water impregnated by art with fixed air. The vegetable which was still kept in the 2d bottle of exper. 1, continued to yield air in the sun-shine, till it ceased to throw up any more air, towards the 7th or 8th day of its being shut up in the vessels. When, after this time, this globular vessel was shook, the water became full of small air bubbles, which for the most part rose to the inverted bottom of the vessel, great part settling on the vegetable, which appeared all covered with them. This sparkling air, which became visible by shaking the glass, could not but be air originally produced by the conferva, and so loosely joined with the water, that it disengaged itself in a great measure from it by the motion of the vessel. After the 10th day the vegetable began to appear withered, yellow, and began to die. Dr. I. found about 8 cubic inches of dephlogisticated air collected in the vessel. This proved to be of a very eminent quality, its goodness being of  $352^{\circ}$ ; that is to say, from a mixture of 1 measure of this air, and as many measures of nitrous air as were necessary to complete the full saturation, there were destroyed 3 measures and  $\frac{5}{100}$  of a measure, the test being made with Abbé Fontana's eudiometer, employed in the manner described in Dr. I.'s book on Vegetables, p. 278 et seq. The quality of this air was superior to that of any air he ever got from this plant in fresh pump water, its goodness proving, in general, to be from  $260$  to  $330^{\circ}$ , in the hot house. This was during the winter, for he never had been able to obtain such fine air from this vegetable in the summer; the reason of which he intended to explain elsewhere.

*Result of exper. 2.*—No air at all was produced in the vessel containing the pieces of cloth, during 3 weeks exposure to the sun-shine. For, boiled water, having lost its air, could yield none, at least till after a long time, when some degree of corruption should take place in the animal substance, viz. the pieces of cloth.

*Result of exper. 3.*—Not a particle of air appeared in this vessel, though it stood about 2 months on the same place. For, boiled water having no air, the sun could extricate none from it.

*Result of exper. 4.*—The conferva began to yield air bubbles the very same day, a little while after its exposure to the sun. The next day it threw up an immense quantity of them. The 5th day it began to throw up less, and ceased entirely about the 7th day, when the quantity of about 14 cubic inches of dephlogisticated air, of an excellent quality, though less fine than that obtained in exper. 1, was collected. The water sparkled, like Seltzer water, by the vessel being shook. This water being exposed to the fire, in an inverted vessel,



yielded a good quantity of air, which was so far dephlogisticated as to be able to kindle a wax taper just extinguished. After the 10th day the vegetable began to die.

The vegetable very soon threw up air bubbles; because this water, being in its natural state, and thus saturated with air, could not absorb much of the air issuing from the vegetable, which air must, of course, soon rise up in visible bubbles. A great deal more air was collected than in exper. 1, because less of the air issuing from the plant was absorbed by this water than in exper. 1. The air obtained was not so good as that obtained in exper. 1; because the air in this experiment was somewhat infected by the air issuing from the water, which was but common air. The water sparkled when the vessel was shook, because this water, though it had probably lost some of its own air, yet had assumed a great deal of air from the plant, which air disengages itself from the water very easily, just as fixed air does; the more so when the water is moved. This water yielded, by heat, true dephlogisticated air; whereas the same water, when it has not been exposed to the action of a vegetable, yields by heat nothing but common air. The reason of it is, that the air elaborated by the plant, with which this water was saturated, was real dephlogisticated air. The vegetable at last languished and began to die, because the water was impregnated with dephlogisticated air, which being an excrement of the plant is hurtful to its constitution. Besides, this water had at last lost the most part of, or perhaps all, its own stock of common air; and with this all the nutritive nourishing and phlogistic particles, which were taken in by the plant, and was therefore become less fit to keep up vegetable life.

All these experiments were repeated frequently, and always with the same general results. Dr. I. therefore thinks the abovementioned facts will be considered as quite sufficient to put his doctrine out of all further question. And after having demonstrated, as he thinks, in the clearest manner, that vegetables diffuse through our atmosphere, in the sun-shine, a continual shower of this beneficial, this truly vital air; and that plants immersed in water, far from robbing it of all air, impregnate it fully with a better and more salubrious air; let us not pass, he says, so wonderful, and hitherto not even so much as suspected, an operation of nature, without admiring the designs of that infinite wisdom, who has employed such hidden, such wonderful, and at the same time such beneficial means to preserve from destruction the living beings which inhabit our earth; and let us consider, whether it would not be worth while to attempt drawing some benefit from this new discovery, by making use of vessels of water, in which some leaves of vegetables have been exposed in the sun-shine; by placing such vessels in our rooms; by stirring the water; by sprinkling with it our floors, instead of using for this purpose common water; by placing within



our houses, instead of flower-pots, dishes containing some *conserva rivularis*, a plant to be met with almost every where, shooting forth with the utmost luxuriance in all water basins, in all tubs and vessels in which water is kept. Is it possible, after all this, he says, not to believe, that the Creator has multiplied this vegetable with a similar view to our benefit? This benefit we may now, with some confidence, apply to our preservation, by honouring this vegetable with a place in those of our own rooms which are exposed to the sun, and keeping it alive as long as we please; which may be done by only pouring every day fresh water on it, and squeezing gently now and then out of it the dephlogisticated air with which the whole mass swells up almost as soon as the sun casts its rays on it. The water itself, in which it has been immersed, will now perhaps be considered as too precious to be thrown away, as useless and deprived of that very principal of animal life, of which he had demonstrated it to be highly pregnant.

*XXVIII. A Microscopic Description of the Eyes of the Monoculus Polyphemus Linnæi.\* By Mr. William André, Surgeon. p. 440.*

The wonderful structure of the eyes of insects in general, most commonly illustrated by that of the libellula, or dragon-fly, cannot fail of striking with astonishment the naturalist who investigates the works of the great Creator in his most minute productions. According to Leuwenhoeck, Hook, and others, the corneæ of most insects are made up of an infinite number of small, transparent, horny lenses, each resembling, in some degree, a small magnifying glass. This structure prevails in the corneæ of insects in general; but the monoculus polyphemus, or king crab, is, among others, an exception to this rule.

The monoculus polyphemus, or king crab, is a crustaceous animal found in all the seas surrounding the continent of America and the West-India islands, and which frequently grows to a very large size. We shall describe so much of the monoculus only as is necessary to point out the situation of the eyes, which have been considered as 2 in number only, though in reality they are 4. The largest piece of the crustaceous covering of this animal, when separated from the rest of the shell, has very much the shape of a barber's basin, or the forepart of a woman's bonnet. The eyes are a part of the shell, or as Linneus expresses it, they are testæ innati.† They may be distinguished by the terms large and small, or lateral and anterior. If the shell were divided fairly in half, the large eyes would be nearly in the centre of each piece, and the small ones on

\* In a preceding part of the Philos. Trans., viz. Vol. 20, N<sup>o</sup> 246, p. 394, is an interesting observation relative to the eyes of this animal, by Mr. Petiver. See vol. 4, p. 325, of these abridgments.

† This being the case, the eyes can enjoy no motion; in which particular, as well as in some others, the monoculus polyphemus differs from the genus of crabs, whose eyes are placed on petioles, or stalks, and are moveable,—Orig.



the divided edge near the fore part of the shell. The large eyes are at a great distance from each other; but the small ones are close together. It will appear hereafter, that the large eyes are made up of a great number of small, transparent, amber-like cones, and that the small ones are composed of one such cone only; so that they may be divided into eyes with many cones, and eyes with a single cone. The large eyes, or those with many cones, appear as 2 transparent spots about the size and nearly of the shape of a kidney bean, the concave edges looking towards each other, and the convex towards the edge of the shell. If they be examined attentively, we may discern on their surface a number of small depressions, which point out the centre of each cone. The small eyes, or those with a single cone, look like two small transparent spots, not larger than a pin's head; these, from their minuteness, are easily overlooked. See fig. 8, pl. 5, where AA show the large eyes, and BB the small ones.

The appearances now described may be seen on the external surface of the shell with the naked eye; but in order to proceed to a further investigation of the subject, the cornea must be removed from the shell, and applied to a single microscope with a very strong light. The internal surface of the large eyes, examined with the microscope, is found to be thick set with a great number (about 1000) of small, transparent cones, of an amber colour, the bases of which stand downward, and their points upwards next the eye of the observer. The cones in general have an oblique direction, except some in the middle of the cornea, about 30 in number, the direction of which is perpendicular. The centre of every cone being the most transparent part, and that through which the light passes; on that account the perpendicular or central cones always appear beautifully illuminated at their points. They are all so disposed, as that a certain number of them receive the light from whatever point it may issue, and transmit it to the immediate organ of sight, which we may reasonably suppose is placed underneath them; but this last circumstance can only be determined in a recent subject, which Mr. A. has never been so lucky as to see. The cones are not all of the same length; those on the edges of the cornea are the longest, whence they gradually diminish as they approach the centre, where they are not above half the length of those on the edges: see fig. 9.

As these cones so easily transmit the light through their substance, when Mr. A. first examined them, he thought they were tubes; but having afterwards viewed them broken in different directions, he was convinced they are solid transparent bodies. If they be viewed with a deep magnifier, every cone appears divided transversely by 2 or 3 internal septa or partitions. This appearance is owing to the cones themselves being made up of several cones, one within another, the septa or partitions being nothing more than the apices or points of the external cones; but this will be further explained by considering that the



cornea of the monocus may be divided into layers, the number of which however he could not ascertain; but he once met with a cornea in which the internal layer and its cones were separated from the external lamina and their cones. A portion of the internal layer is shown fig. 11; and the cones, very much magnified, with their septa or partitions, are exhibited fig. 12.

It is very well known, that all crustaceous animals cast their shells once a year, and are left with a soft, tender covering, which, after some time, acquires the hardness of the former shell. As the cornea in these animals is a part of the shell, it is reasonable to suppose, that the internal layer is left with the soft covering, containing the rudiments of the future cornea; and this is the more probable, from Mr. A. having met with an eye where the internal layer was separated from the more external ones: see fig. 11.

The structure of the small eyes being less elaborate than that of the large ones, their internal appearance, when placed in a microscope, will be described in a few words. They consist of an oval, transparent, horny plate, of an amber colour, in the centre of which stands a single cone, through which and the oval plate the light passes: see fig. 10. These small eyes are analogous to those small eyes of other insects which entomologists have called stemmata. The lenticular structure of the corneæ of insects in general certainly assists in condensing or strengthening the light, in its passage to the immediate organ of sight. It is probable, that the cones in the monocus have the same effect. Whether they answer that purpose, in a more or less perfect manner than the lenses in the generality of insects, is what Mr. A. cannot determine.

*Explanation of the figures.*—Fig. 8, pl. 5, the monocus polyphemus. AA The large eyes. BB the small ones. Fig. 9, one of the large eyes magnified. Fig. 10, One of the small eyes magnified. Fig. 11, a portion of the internal layer magnified. Fig. 12, the cones magnified with their septa or partitions.

END OF THE SEVENTY-SECOND VOLUME OF THE ORIGINAL.

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*I. On the Name of the New Planet, in an Extract of a Letter from William Herschel, Esq., F. R. S. Vol. 73, Anno 1783. p. 1.*

By the observations of the most eminent astronomers in Europe it appears, that the new star, which I had the honour of pointing out to them in March, 1781, is a primary planet of our solar system.\* A body so nearly related to us

\* The observations on this new planet, at first suspected to be a comet, are abridged at p. 154, of this volume. Dr. Herschel, the discoverer, here calls it the Gergium Sidus, or Georgian planet, in honour of his Majesty; by which name it is commonly distinguished in this country. But, in other countries it is often called by other names: as Ouranus, Uranus, Herschel, &c. Its astronomical mark or character is ♃. By later observations and calculations it has been determined, that



by its similar condition and situation, in the unbounded expanse of the starry heavens, must often be the subject of the conversation, not only of astronomers, but of every lover of science in general. This consideration then makes it necessary to give it a name, by which it may be distinguished from the rest of the planets and fixed stars. In the fabulous ages of ancient times the appellations of Mercury, Venus, Mars, Jupiter, and Saturn, were given to the planets, as being the names of their principal heroes and divinities. In the present more philosophical æra, it would hardly be allowable to have recourse to the same method, and call on Juno, Apollo, Pallas, or Minerva, for a name to our new heavenly body. The first consideration in any particular event, or remarkable incident, seems to be its chronology: if in any future age it should be asked, when this last-found planet was discovered? It would be a very satisfactory answer to say, "In the reign of King George the Third." As a philosopher then, the name of *Georgium Sidus* presents itself to me, as an appellation which will conveniently convey the information of the time and country where and when it was brought to view.

*II. On the Diameter and Magnitude of the Georgium Sidus; with a Description of the Dark and Lucid Disc and Periphery Micrometers. By Wm. Herschel, Esq., F. R. S. p. 4.*

In this paper Dr. H. describes several ingenious methods which he practised, to obtain nearly the angle or apparent magnitude of his new planet. The measures of its diameter, which were delivered in his first paper, in 1781, differ considerably from each other. However, if we set aside the first 3, on a supposition that every minute object, which is much smaller than what we are frequently used to see, will at first sight appear less than it really is; and take a mean of the remaining observations, we shall have  $4'' 36\frac{1}{4}'''$  for the diameter of the planet. On comparing the measures then with this mean, we find but two of them that differ somewhat more than half a second from it; the rest are almost all within a quarter of a second of that measure. This agreement, in the dimensions of any other planet, would appear very considerable; but not being satisfied, when he thought it possible to obtain much more accurate mea-

the diameter of this planet is about 35,109 miles, or  $4\frac{4}{15}$  times that of the earth; its distance from the sun 1800 millions of miles, or above 19 times the earth's distance; and that the period of its revolution in its orbit round the sun, is 83 years, 140 days, 17 hours. Dr. Herschel has also discovered 6 satellites or moons belonging to this planet, whose periodical revolutions are nearly as annexed; their orbits are nearly perpendicular to the plane of the ecliptic; and they all perform their revolutions in their orbits contrary to the order of the signs, that is, their real motion is retrograde.

1st.....	5 <sup>d</sup>	21 <sup>h</sup>	25 <sup>m</sup>
2d.....	8	18	0
3d....	10	23	4
4th. ..	13	12	0
5th. ..	38	1	49
6th. ..	107	16	40



tures, he employed the lamp-micrometer in preference to the former. The first time he used it on this occasion, he perceived, that if, instead of 2 lucid points, we could have an entire lucid disc to resemble the planet, the measures would certainly be still more complete. The difficulty of dilating and contracting a figure that should always remain a circle, appeared very considerable, though Nature, with her usual simplicity, holds out to us a pattern in the iris of the eye, which, simple as it appears, is not one of the least admirable of her inimitable works. However, he recollected, that it was not absolutely requisite to have every insensible degree of magnitude; since, by changing the distance, he could without much inconvenience make every little intermediate gradation between a set of circles of a proper size, that might be prepared for the purpose. Dr. H. intending to put this design into practice, he contrived the following apparatus.

A large lantern, of the construction of those small ones that are used with his lamp-micrometer, must have a place for 3 flames in the middle, that we may have the quantity of light required, by lighting 1, 2, or all of them. The grooves, instead of brass sliding doors, must be wide enough to admit a paste-board, and 3 or 4 thicknesses of paper. He prepared a set of circles, cut out in paste-board, increasing by 10ths of an inch, from 2 inches to 5 in diameter, and these were made to fit into the grooves of the lamp. A good number of pieces, some of white, others of light blue paper, of the same size with the pasteboards, were also cut out, and several of them oiled, to render them more transparent. This apparatus being ready, we are to place behind the paste-board circle, next to the light, 1, 2, or more, either blue or white, dry or oiled papers; and by means of one or more flames, to obtain an appearance perfectly resembling the disc we would compare it with. It will be found, that more or less altitude of the object, and higher or lower powers of the instrument, require a different assortment of papers and lights, which must by no means be neglected: for if any fallacy can be suspected in the use of this apparatus, it is in the degree of light we must look for it. In a few experiments Dr. H. tried with these lucid discs, where he placed several of them together, and illuminated them at once, it was found, that but very little more light will make a circle appear of the same size with another, which is 1, or even 2-tenths of an inch less in diameter.

The method of using the artificial discs is the same which has been described with the lamp-micrometer, of which this apparatus may be called a branch. We are only to observe, that the planet we would measure should be caused to go either just under, or just over, the illuminated circle. It may indeed also be suffered to pass across it; but in this case, the lights will be so blended together, that we cannot easily form a proper judgment of their magnitudes. The general apparatus employed being now sufficiently explained, several alterations that were



occasionally introduced are mentioned in the observations and experiments on the Georgium Sidus, as they follow, in the order of time in which they were made, viz. on the light, the diameter, and the magnitude of that planet.

And first, on Oct. 22, 1781, the planet was perfectly defined with a power of 227; had a fine, bright, steady light; of the colour of Jupiter, or approaching to the light of the moon. Next, on a great many days, in the course of that and the following year, and with several variations in the apparatus and mode of observing, Dr. H. deduced the several measures of the planet's apparent diameter, as in the annexed table. His method of deducing the apparent diameter from the observation, was commonly thus: dividing the breadth of the image, in inches, by its distance from the eye, gives the tangent of the magnified angle; then the angle answering to this tangent being divided by the number denoting the power of the telescope, gives the apparent diameter of the planet sought. For instance, if the breadth of the magnified image measure 2.4 inches, at the distance of 431 inches from the eye: then  $\frac{2.4}{431} = .0055684$ , the tangent of  $19' 8''$ ; which divided by 227, the power of the telescope, gives  $5''.06$  for the planet's apparent diameter.

Dr. H. then concludes: I intend to pursue these experiments still further, especially in the time of the planet's opposition, and am therefore unwilling as yet to draw a final conclusion from the several measures. In a subject of such delicacy we cannot have too many facts to regulate our judgment. Thus much however we may in general surmise, that the diameter of the Georgium Sidus cannot well be much less, nor perhaps much larger, than about 4 seconds. From this, if we would anticipate more exact calculations hereafter to be made, we may gather that the real diameter of that planet must be between 4 and 5 times that of the earth: for by the calculations of M. de la Lande, contained in a letter he has favoured me with, the distance of the Georgium Sidus is stated at 18.913, that of the earth being 1. And if we take the latter to be seen, at the sun, under an angle of  $17''$ , it would subtend no more than  $.''898$ , when removed to the orbit of the Georgium Sidus. Hence we obtain  $\frac{4}{.898} = 4.454$ ; which number expresses how often the real diameter of the Georgium Sidus exceeds that of the earth.

*III. Conclusion of the Experiments and Observations concerning the Attractive Powers of the Mineral Acids. By Richard Kirwan, Esq., F. R. S. p. 15.*

Having found, as exactly as I was able, (says Mr. K.), the quantity of each of the mineral acids taken up at the point of saturation by alkalis and earths, and also that taken up by phlogiston, when these acids are by it converted into



an aërial form, I next endeavoured to find how much of these acids was taken up at the point of saturation by each of the metallic substances, and for this purpose procured the most saturated solution possible of each metallic substance soluble in any of these acids. These solutions did not indeed immediately answer my purpose, as they constantly retained an excess of acid; yet as they were the foundation of my subsequent observations, and as the experiments themselves are in many respects useful to be known, I shall here briefly relate their result, and confine myself to those circumstances singly that relate to my future investigations, or that have not heretofore been satisfactorily explained. The acids I used were dephlogisticated so far as to be colourless; the metals were for the most part very fine filings, or reduced in a mortar to a fine powder. They were added little by little to their respective menstrooms, much more being thus dissolved than if the whole was thrown in at once; and the solution was performed in glass phials with bent tubes.

*Solution of iron in the vitriolic acid.*—100 grs. of bar-iron, in the temperature of  $56^{\circ}$ , require for their solution, 190 grs. of real acid, whose proportion to that of the water with which it should be diluted, is as 1 to 8, 10, or 12. It would act on iron, though its proportion were greater or lesser, but not so vigorously. If towards the end a heat of  $200^{\circ}$  were applied, 123 grs. of real acid would be sufficient. The air produced by this solution is entirely inflammable, and generally amounts to 155 cubic inches.

100 grs. of the vitriol crystallized contain 25 of iron, 20 of real acid, and 55 of water. When calcined nearly to redness these crystals lose about 40 of water.

*Iron in the nitrous acid.*—100 grs. of iron, to be perfectly dissolved, and not barely calcined, require 142 grs. of real nitrous acid, so diluted as that its proportion to water should be as 1 to 13 or 14; and when this last proportion is used, the heat of a candle may be applied for a few seconds, and the access of common air prevented. In this case not above 18 cubic inches of nitrous air are produced, all the rest is absorbed by the solution, and no red vapours appear. But if the proportion of acid and water be as 1 to 8 or 10, and heat be applied, a much greater quantity of iron will be dephlogisticated, though very little of it be held in solution; and by this means I have obtained from 100 grs. of iron 83.87 cubic inches of nitrous air; and by distilling the solution a still greater quantity may be obtained, which was absorbed by the solution.

*Iron in the marine acid.*—100 grs. of iron require 215 of real marine acid for their solution. The proportion of acid to that of water in the spirit of salt I used was as 1 to 4. When it is as 1 to 5, it effervesces too violently. Heat is rather prejudicial, as it volatilizes the acid. No marine air flies off, and the quantity of inflammable air is just the same as if dilute vitriolic acid were used.



*Copper in the vitriolic acid.*—100 grs. of copper require nearly 183 grs. real vitriolic acid for their solution. The proportion of acid to that of water being as 1 to  $\frac{1}{6}$ , or at least as 1 to  $\frac{7}{10}$ , and a strong heat must also be applied. The dephlogistication of 128 grs. of copper, treated in this manner, affords 11 cubic inches of inflammable air, and nearly 65 of vitriolic air. 100 grs. of vitriol of copper contain 27 of copper, 30 of acid, and 43 of water, of which it loses about 28 by evaporation or slight calcination. The solution of 100 grs. of copper affords 373 of blue vitriol.

*Copper in nitrous acid.*—100 grs. of copper require 130 of real nitrous acid to dissolve them. If the acid be so far diluted as that its proportion to that of water be as 1 to 14, the assistance of heat will be necessary, otherwise not. This solution affords  $67\frac{1}{2}$  cubic inches of nitrous air. The calces of copper are also soluble in this acid.

*Copper in marine acid.*—100 grs. of copper require 1190 grs. of real marine acid to dissolve them, and also the assistance of a moderate heat, the proportion of acid to that of water being as 1 to  $4\frac{1}{3}$ , that is, its specific gravity being 1.186, if a greater heat be used, more of the acid will be requisite, as much will be dissipated. If the acid be more concentrated, it will act more vigorously. The calces of copper are also soluble in this acid, though not so easily as in the nitrous acid.

*Tin in the vitriolic acid.*—100 grs. of tin require for their perfect solution 872 grs. of real vitriolic acid, whose proportion to water should not be less than as 1 to  $\frac{9}{10}$ , and also the assistance of a strong heat; when the action of the acid has ceased, some hot water should be added to the turbid solution, and the whole again heated. This solution affords 70 cubic inches of inflammable air. Tin is also soluble in a more dilute acid, but not in so great quantity. The calces of tin (except that precipitated from marine acid by fixed alkalis) are insoluble in this acid.

*Tin in the nitrous acid.*—100 grs. of tin require, for their perfect solution, 1200 grs. of real nitrous acid, whose proportion to water should be at least as 1 to 25, and the heat not exceeding  $60^{\circ}$ : the quantity of air afforded by such solution is only 10 cubic inches, and it is not nitrous. The solution is not permanent; for in a few days it deposits a whitish calx, and if the weather be warm bursts the phial. The calces of tin are insoluble in this acid.

*Tin in the marine acid.*—100 grs. of tin require, for their solution, 413 of real marine acid, whose proportion to water is as 1 to  $4\frac{1}{2}$ , and also the assistance of a moderate heat. This solution affords about 90 cubic inches of inflammable air and 10 of marine air. The calces of tin are nearly insoluble in this acid.

*Lead in the vitriolic acid.*—100 grs. of lead require, for their solution, 600



grs. of real acid, whose proportion to water is not less than that of 1 to  $\frac{7}{10}$ , and better if the quantity of water be still less; and hence, as with regard to copper, a greater quantity of lead should be employed than is expected to be dissolved. A strong heat is also requisite, and hot water should be added to the calcined mass, though sparingly, as it occasions some precipitation. This metal is also soluble, but in a very small degree, in dilute vitriolic acid; for it effervesces with spirit of vitriol whose specific gravity is only 1.275. The calces of lead are something more soluble in this acid. 100 grs. of vitriol of lead, formed by precipitation, contain 73 of lead, 17 of real acid, and 10 of water. Vitriol of lead, formed by direct solution, contains a large proportion of acid.

*Lead in the nitrous acid.*—100 grs. of lead require, for their solution, about 78 grs. of real acid, whose proportion to that of water may be as 1 to 11 or 12, and the assistance of heat towards the end. This solution affords but 8 cubic inches of nitrous air. The calces of lead are also soluble in this acid; but if much dephlogisticated they become less soluble. 100 grs. of minium require 81 grs. of real acid. 100 grs. of nitrous salt of lead contain about 60 of lead.

*Lead in the marine acid.*—100 grs. of lead require 600 grs. of real acid to dissolve them, when the specific gravity of the spirit of salt is 1.141, and also the assistance of heat, by which much of the acid is dissipated. A stronger acid would dissolve more. The calces of lead are more soluble in this acid than genuine lead. 100 grs. minium require 327 grs. of real acid; but white lead is much less soluble. 100 grs. of horn lead, formed by precipitation, contain 72 of lead, 18 of marine acid, and 10 of water.

*Silver in the vitriolic acid.*—100 grs. of pure silver require, to dissolve them, 530 grs. of real vitriolic acid, whose proportion to water is not less than that of 1 to  $\frac{6}{10}$ , and when such a concentrated acid is used, it acts slightly even in the temperature of 60°; but for a copious solution a moderate heat is requisite. This solution affords 30 cubic inches of vitriolic air. Standard silver affords more air and requires more acid for its solution. The calces of silver (that is, silver precipitated from its solution in nitrous acid by fixed alkalis, and well-washed, but which still retains some nitrous acid), are soluble even in dilute vitriolic acid, without the assistance of heat. 100 grs. of vitriol of silver, formed by precipitation, contain 74 grs. of silver, about 17 of real acid, and 9 of water.

*Silver in the nitrous acid.*—100 grs. of the purest silver require, for their solution, 36 of mere nitrous acid, diluted with water in the proportion of 1 part real acid to 6 of water, applying heat only when the solution is almost saturate. If spirit of nitre be much more or much less dilute, it will not act without the assistance of heat. The last portions of silver, thus taken up, afford no air. Standard silver requires about 38 grs. of real acid to dissolve the same proportion.



of it. And the solution of it affords 20 cubic inches of nitrous air; whereas 100 grs. of silver, revived from luna cornea, afford about 14.

*Silver in the marine acid.*—I have not been able to dissolve silver, in its metallic state, in spirit of salt, yet I believe it may be effected, if sufficient time be allowed, as Mr. Bayen, in his Treatise on Tin, p. 201, says, he dissolved  $3\frac{1}{2}$  grs. of silver by digesting it for some days in 2 ounces of strong spirit of salt. Leaf silver is also said to be corroded by strong spirit of salt, 1 Newm. 70. The dephlogisticated marine acid also dissolves it, according to the observations of Messrs. Scheele and Bergman: and so does the phlogisticated in a vaporous state. 100 grs. of horn silver contain 75 of silver, nearly 18 of acid, and 7 of water.

*Gold in aqua regia.*—I made several experiments with aqua regia, in which the nitrous and marine acids were mixed in different proportions, and found that to succeed best, in which the quantity of real marine acid was to that of the nitrous as 3 to 1, and both as concentrated as possible; though if both be very concentrated, it is hard to mix them so as to prevent a great quantity from escaping, as they effervesce very violently some time after mixture. 100 grs. of gold require 246 grs. real acid for their solution, the 2 acids being in the above-mentioned proportion. The specific gravity of the nitrous acid used was 1.465, and that of the marine 1.178. The solution is better promoted by allowing it sufficient time than by applying heat. The heat used did not exceed 90 or 100°. Very little air is produced, and the solution is very slow. Aqua regia made with common salt or sal ammoniac and spirit of nitre is much less aqueous, than that resulting from an immediate combination of both acids; and hence is the fittest for the production of crystals of gold. Gold is also soluble in the dephlogisticated marine acid, but in very small quantity, unless this acid be in a vaporous state, for in a liquid state it is too aqueous. In vitriolic and nitrous acids it is also insoluble; but the calces of gold are easily soluble in the marine acid, very slightly in the nitrous, and scarce at all in the vitriolic. Gold in its metallic state may be diffused through, but not dissolved, by the concentrated nitrous acid.

*Mercury in vitriolic acid.*—100 grs. of quicksilver require, for their solution, 230 grs. of real vitriolic acid, whose proportion to that of water is at least as 1 to  $\frac{8}{10}$ , and also a strong heat. The air produced is vitriolic. Precipitate per se is still less soluble. 100 grs. of vitriol of mercury, produced by precipitation, contain 77 of mercury, 19 of acid, and 4 of water.

*Mercury in nitrous acid.*—100 grs. of mercury are dissolved by 28 grs. of real nitrous acid, whose proportion to that of water is as 1 to 1 and  $\frac{5}{10}$ , and without the assistance of heat. Mercury is also soluble, but in smaller quantity, in a much more dilute acid, with the assistance of heat. The product of air is about



12 cubic inches or less, if heat be not applied. Precipitate per se is much more difficultly dissolved by nitrous acid than genuine mercury, which I attribute to the attraction of the aërial acid contained in the precipitate.

*Mercury in marine acid.*—The marine acid, in its common phlogisticated state, does not act on mercury, at least in its usual state of concentration; but Mr. Homberg, in the Paris Memoirs for the year 1700, assures us, he dissolved mercury in marine acid, whose specific gravity was 1.300, by keeping it some months in digestion. The authors of the Cours de Chymie de Dijon affirm also its solubility in this acid, though in very small quantity. The dephlogisticated marine acid, in a vaporous state, certainly acts on it, though while in a liquid state it is too weak, by reason of its dilution. Precipitate per se is also soluble in marine acid, with the assistance of heat. 100 grs. of sublimate corrosive contain 77 of mercury, 16 of real acid, and 6 of water. 100 grs. of mercurius dulcis contain 86 of mercury, and 14 of acid and water.

*Zinc in vitriolic acid.*—100 grs. of zinc require, for their solution, 100 grs. of real acid, whose proportion to that of water may be as 1 to 8, 10, or 12, applying heat towards the end, when the acid is almost saturated. A small quantity of black powder always remains undissolved. The product of inflammable air is 100 cubic inches. It is soluble in the concentrated vitriolic acid, with the aid of heat. 100 grs. of vitriol of zinc contain 20 of zinc, 22 of acid, and 58 of water. The calces of zinc, if not exceedingly dephlogisticated, are also soluble in this acid.

*Zinc in nitrous acid.*—100 grs. of zinc require, for their solution, 125 grs. of real nitrous acid, whose proportion to that of water is as 1 to 12, applying from time to time a slight heat. If a concentrated acid be used, less will be dissolved, as much of the acid will escape during the effervescence. I could procure no nitrous air from the solution by any management, as the nitrous acid is in part decomposed during the operation. The calces of zinc, if not too much dephlogisticated, are also soluble in this acid.

*Zinc in marine acid.*—The same quantity of zinc requires of this acid 210 grs. the proportion of real acid in the menstruum being as 1 to 9, and using from time to time a slight heat. If a less dilute acid be used, more real acid will be requisite, as much of it will escape during the effervescence. The calces of zinc are also soluble in this acid.

*Bismuth in vitriolic acid.*—200 grs. of oil of vitriol, whose specific gravity was 1.863, dissolved but 3 grs. of bismuth in a strong heat; but slightly dephlogisticated a greater quantity. 400 grs. of spirit of vitriol, whose specific gravity was 1.200, dissolved only 1 grain. The calces of bismuth are much more soluble. The solution of the 3 grs. afforded 4 cubic inches of vitriolic air.

*Bismuth in nitrous acid.*—The solution of 100 grs. of bismuth require but



100 grs. of real nitrous acid, whose proportion to water should be as 1 to 8 or 9. In this last case, a gentle heat may be applied. This solution affords 44 cubic inches of nitrous air. The calces of bismuth are also soluble in this acid.

*Bismuth in marine acid.*—400 grs. of spirit of salt, whose specific gravity is 1.220, dissolved only 3 or 4 grs. of bismuth.

*Nickel in vitriolic acid.*—100 grs. of concentrated vitriolic acid dissolve about 4 of nickel, with the assistance of a strong heat. The calces of nickel are much more soluble.

*Nickel in nitrous acid.*—100 grs. of nickel require for their solution 112 grs. of nitrous acid, whose proportion to water is as 1 to 11 or 12, assisted with a moderate heat. A concentrated acid acts so rapidly that much is dissipated. The product of nitrous air is 79 cubic inches. The calces of nickel are also soluble in this acid.

*Nickel in marine acid.*—200 grs. of spirit of salt, whose specific gravity is 1.220, dissolved 4 or 5 grs. of nickel, without the assistance of heat. A weaker acid dissolves less, and requires the assistance of heat. In all these cases of difficult solution more of the metal will be taken up by distillation and cohobation; but the proportion will be difficult to assign. The calces of nickel are also difficultly soluble in this acid.

*Cobalt in vitriolic acid.*—100 grs. of cobalt require 450 grs. of real acid, whose proportion to its water is not less than 1 to  $\frac{7}{10}$ , and a heat of  $270^{\circ}$  at least. By pouring warm water on the dephlogisticated mass a solution is obtained. The calces of cobalt are still more soluble; even a dilute acid will serve.

*Cobalt in nitrous acid.*—100 grs. of cobalt require 220 grs. of real nitrous acid, whose proportion to water is as 1 to 4, giving towards the end a heat of  $180^{\circ}$ . The calces of cobalt are soluble in this acid.

*Cobalt in marine acid.*—100 grs. of spirit of salt, whose specific gravity is 1.178, dissolve, with the assistance of heat,  $2\frac{1}{2}$  grs. of cobalt. A more concentrated acid will dissolve more. The calces of cobalt are more soluble in this acid.

*Regulus of antimony in vitriolic acid.*—100 grs. of regulus of antimony require for their solution 725 grs. of real acid, whose proportion to water is as 1 to  $\frac{7}{10}$ , and a heat of  $400^{\circ}$ . More regulus should be employed than is expected to be dissolved, and the resulting salt requires a large quantity of water to dissolve it; for the concentrated acid lets fall much when water is added to it. A less concentrated acid will also dissolve this semi-metal, but in smaller quantity. The calces of antimony, even diaphoretic antimony, are something more soluble.

*Regulus of antimony in nitrous acid.*—100 grs. of this semi-metal require 900 grs. of real nitrous acid, whose proportion to water is as 1 to 12, aided with a



heat of  $110^{\circ}$ . The solution, however, becomes turbid in a few days. The calces of antimony are soluble in a much less degree.

*Regulus of antimony in marine acid.*—100 grs. of spirit of salt, whose specific gravity is 1.220, dissolve about 1 gr. of regulus, with the assistance of a slight heat. Spirit of salt, whose specific gravity is 1.178, also acts on it, but dissolves still less. I believe the concentrated acid would, in a long time, and with the help of a gentle heat, dissolve much more of it. The calces of antimony are much more soluble in this acid.

*Regulus of arsenic in vitriolic acid.*—200 grs. of oil of vitriol, whose specific gravity is 1.871, dissolve 18 of regulus of arsenic in a heat of  $250^{\circ}$ . Of these about 7 crystallize on cooling, and are soluble in a large quantity of water. The calces of arsenic are more soluble in this acid.

*Regulus of arsenic in nitrous acid.*—100 grs. of this semi-metal require 140 grs. of real nitrous acid, whose proportion to water is as 1 to 11, and the assistance of heat. It is soluble in a less or more concentrated acid, but in a lesser degree. This solution affords 102 cubic inches of nitrous air. The barometer at 30, and the thermometer at 60. The calces of arsenic are also soluble in this acid.

*Regulus of arsenic in marine acid.*—100 grs. of spirit of salt, whose specific gravity is 1.220, dissolve  $1\frac{1}{2}$  grs. of regulus of arsenic; the marine acid, in its common dilute state, that is, whose specific gravity is under 1.17, does not at all affect it. The calces of arsenic are less soluble in this acid than in the vitriolic or nitrous.

The advantages resulting from these inquiries are very considerable, not only in promoting chemical science, which, being a physical analysis of bodies, essentially requires an exact determination, as well of the quantity and proportion, as of the quality of the constituent parts of bodies, but also in the practical way. Thus, in the first place, it is well known, that several important processes are very inaccurately described by ancient chemical writers, and even by some of a modern date: they frequently, for instance, describe the acid they employed by reference to the quantity of fixed alkali, earth, or metal, a given quantity of such acid was capable of neutralizing or dissolving. Now the foregoing observations immediately inform us of the quantity of real acid capable of performing that effect; the remainder, therefore, must have been water; and the quantity of real acid and water being known, the specific gravity is easily found by the help of the foregoing tables, and thus an acid of the same strength may be formed.

2dly. The importance of this knowledge in the art of pharmacy is very obvious, especially with regard to medicines formed of metallic substances, whose powers depend on the proportion of their ingredients, and their action on each other.



3dly. This degree of precision must tend considerably to the improvement of the arts of dying and enamelling, the processes by which many of their ingredients are procured being at present much too vague.

4thly. The uses of this knowledge in the examination of mineral waters, and in assaying of ores, have been amply proved in the elaborate treatises which the celebrated Bergman has lately given us on these subjects. And I may further add, that the knowledge of the quantity of acid requisite for the solution of different metallic substances may also furnish us with a new criterion for distinguishing them from each other, and the purer from their alloys, and in some cases inform us of the quantity and quality of the alloy.

But the end which of late I had principally in view, was to ascertain and measure the degrees of affinity or attraction that subsist between the mineral acids, and the various bases with which they may be combined, a subject of the greatest importance, as it is on this foundation that chemistry, considered as a science, must finally rest. Chemical affinity or attraction is that power by which the invisible particles of different bodies intermix and unite with each other so intimately as to be inseparable by mere mechanical means. In this respect it differs from magnetic and electrical attraction. It also differs from attraction of cohesion in this, that the latter takes place between particles of almost all sorts of bodies whose surfaces are brought into immediate contact with each other; for chemical attraction does not act with that degree of indifference, but causes a body already united to another to quit that other and unite with a third, and hence it is called elective attraction. Hence attraction of cohesion often takes place between bodies that have no chemical attraction to each other; thus regulus of cobalt and bismuth have no chemical attraction to each other, for they will not unite in fusion, yet they cohere to each other so strongly, that they can be separated only by a stroke of a hammer.

Hence bodies, which refuse to unite to each other chemically when they are most minutely divided, as when both are in a vaporous or ærial state, or when both are in a liquid state, may be judged, in the first case, to have none; or in the second case to have at best but a very small affinity to each other. But those that unite, when one of them only is in a liquid state, may be said to have a strong affinity to each other, and it is thus that acids unite to alkalis, earths, and metals, for the most part.

The discovery of the quantity of real acid in each of the mineral acid liquors, and the proportion of real acid taken up by a given quantity of each basis at the point of saturation, led me unexpectedly to what seems to be the true method of investigating the quantity of attraction which each acid bears to the several bases to which it is capable of uniting; for it was impossible not to perceive, 1st, That the quantity of real acid, necessary to saturate a given weight of each basis, is



inversely as the affinity of each basis to such acid. 2dly, That the quantity of each basis, requisite to saturate a given quantity of each acid, is directly as the affinity of such acid to each basis. Thus 100 grs. of each of the acids require for their saturation a greater quantity of fixed alkali than of calcareous earths, more of this earth than of volatile alkali, more of this alkali than of magnesia, and more of magnesia than of earth of alum, as may be seen in the following table.

*Quantity of basis taken up by 100 grs. of each of the mineral acids.*

	Veg. fixed alkali. Grs.	Mineral alkali. Grs.	Calcareous earth. Grs.	Volatile alkali. Grs.	Mag- nesia. Grs.	Earth of alum. Grs.
Vitriolic acid.....	215	165	110	90	80	75
Nitrous acid.....	215	165	96	87	75	65
Marine acid.....	215	158	89	79	71	55

As these numbers agree with what common experience teaches us concerning the affinity of these acids with their respective bases, they may be considered as adequate expressions of the quantity of that affinity, and I shall in future use them as such. Thus the affinity of the vitriolic acid to fixed vegetable alkali, that is, the force with which they unite, or tend to unite, to each other, is to the affinity with which that same acid unites to calcareous earth, as 215 grs. to 110; and to that which the nitrous acid bears to calcareous earth as 215 grs. to 96, &c.

*Of the affinity of the mineral acids to metallic substances.*—Having established the agreement between the quantity of any alkaline or terrene basis, taken up at the point of saturation by a given weight of any of the 3 mineral acids, and the quantity of affinity which each of these acids bears to such basis, I naturally extended my views to metallic substances, to try whether this coincidence could be traced with regard to them also; but the difficulties that occurred in this inquiry were so great, that the same degree of certainty must not be expected as in the foregoing part.

Metallic substances, when freest from all foreign mixture, are obtained either in a reguline state, or in that of a calx. These calces, if formed by fire, are constantly combined with more or less of the aërial acid, which is very difficultly extracted from them, and very soon re-absorbed; and if formed by solution, they as constantly retain a portion of their solvent or precipitant, so that the precise weight of the really metallic part is difficultly ascertained. But though this should easily be effected, still they would for the most part be unfit for my purpose; because most of them, when much dephlogisticated, are insoluble in some or all the acids: hence I chose metals in their metallic state for the subject of my experiments. These consist of specifically different earths and phlogiston, and of this they must lose a part before they can be dissolved in acids; but, be-



sides that which escapes in an aërial form, much more of it, though separated from the metallic earth, is yet retained in the solution by the compound of acid and calx. It is this calx, thus differently dephlogisticated by the different acids, whose proportion I endeavoured to ascertain.

The great difficulty that occurred in this inquiry was, that of finding the exact quantity of acid necessary to saturate the metallic substances; for all metallic solutions turn solution of litmus red, and consequently contain an excess of acid. And the reason is, because the salts, formed by a due proportion of metallic calx and acid, are nearly insoluble in liquids that do not contain a further quantity of acid; and in some cases this quantity, and even its proportion to the aqueous part of the liquor, must be very considerable, as in solutions of bismuth. Hence I in vain endeavoured, by caustic alkalis and lime-water, to deprive these solutions of this excess; for when deprived even of only part of it, many of the metals precipitated, and all would, if deprived of the whole of it. On this account I was obliged to use different methods. The vitriolic solutions of tin, bismuth, regulus of antimony, nickel, and regulus of arsenic, containing a large excess of acid, I saturated part of it with caustic volatile alkali before I tried them with the infusion of litmus, and I used the same expedient with the nitrous solution of iron; lead, tin, and regulus of antimony, and all the marine solutions. The proportion of vitriolic and marine acid taken up by lead, silver, and mercury, I determined by computing the quantity of real acid necessary to precipitate these metals from their solutions in the nitrous acid; and of all the determinations these appeared to be the most exact. However, as all the vitriols of these metals are, though in a slight degree, soluble in the nitrous acid, I was obliged to rectify the result from other considerations, and the same necessity occurred with regard to the marine salts of lead and mercury.

The result of these experiments was, that 100 grs. of each of these acids take up, at the point of saturation of each metallic substance, dephlogisticated to such a degree as is necessary for its solution in each acid, the quantities expressed in the following table, which denote their degree of affinity to each metal.

*Table of the affinity of the three mineral acids to metallic substances.*

100 grs.	Iron.	Copp.	Tin.	Lead.	Silver.	Merc.	Zinc.	Bism.	Nickel.	Cobalt.	Reg. of an.	Reg. of ars.
Vitriolic acid	270	260	138	412	390	432	318	250 310	320	360	200	260
Nitrous acid	255	255	120	365	375	416	304	290	300	350	194	220
Marine acid	265	265	130	400	420	438	312	250 320	275 310	370	198	290

*Of the precipitation of metals by each other from the mineral acids.*—I am now come to the last point of my inquiry, and the most difficult to be set forth with that degree of precision which I have been enabled to attain in the former



parts; for it is first necessary to find the quantity of phlogiston in each of them, not only in general, but according to their various degrees of dephlogistication by each of the acids. In this last particular I cannot assert that I have attained any thing like a certainty, yet I hope what I advance may not be useless to chemical readers, as it is not altogether groundless, as it contradicts no chemical fact, but, on the contrary, is agreeable to many, and affords a ready solution of all the phenomena.

*Of the absolute quantity of phlogiston in metals.*—The proportion of phlogiston in metallic substances, relatively to each other, has been investigated in so masterly a manner by Mr. Bergman, that I lay it down as the ground of my inquiries. After his discovery all that remained was to find the absolute quantity of it in any one metal; for then, by an easy calculation, it may be determined in all the rest. The substance I chose for this purpose was regulus of arsenic, as being most capable of dephlogistication by nitrous acid, though not altogether so. From 100 grs. of regulus of arsenic, dissolved in dilute nitrous acid, as already seen, 102 cubic inches of nitrous air and  $\frac{4}{10}$  are obtained, barometer at 30°, thermometer at 60°. I must add, that I made the experiment on 5 grs. only, so that the calculation relates only to the quantity of air which 100 grs. should give. I repeated the experiment three times with the same success. I attempted getting more air from the residuum left by a gentle evaporation, but though fresh spirit of nitre grew red with it, the quantity of air was quite inconsiderable.

Now this quantity of nitrous air contains 6.86 grs. of phlogiston, according to the calculation to be seen in my former paper; and hence I conclude, that 100 grs. of regulus of arsenic contains 6.86 grs. of phlogiston. This regulus was made by Mr. Wolfe, and perfectly bright. Hence the relative proportion of phlogiston in metals being, as found by Mr. Bergman, and set forth in the 1st column of the annexed table, the absolute quantity will be as shown in the 2d column.

100 grs. of	Relat. quan. of phl. gist.	Absol. quan.
Gold .....	394	24.82
Copper .....	312	19.65
Cobalt .....	270	17.01
Iron .....	233	14.67
Zinc .....	182	11.46
Nickel .....	156	9.82
Regulus of antimony..	120	7.56
Tin .....	114	7.18
Regulus of arsenic....	109	6.86
Silver.....	100	6.30
Mercury .....	74	4.56
Bismuth .....	57	3.59
Lead .....	43	2.70

This point being of some importance, Mr. K. endeavoured to ascertain it still further by other experiments, unnecessary to be here repeated.

*Of the affinity of metallic calces to phlogiston.*—That inflammable air, or phlogiston, is condensed to a very considerable degree by uniting to any metallic substance, so that its specific gravity is not only equal, but much superior, to that of the metallic earth with which it combines, may easily be concluded from the example of fixed air, which, by uniting to calcareous earth, acquires a spe-



cific gravity equal to that of gold; and hence, that metallic earth which condenses phlogiston most, and in greatest quantity, uniting to it most closely, may be said to have the greatest affinity to it; so that if we could find the specific gravity of a calx perfectly pure, both from phlogiston and fixed air, we could, by comparing its density with that of the same calx when metallized, know the density which phlogiston acquires by its union with such calx; but to procure such calces has hitherto proved impossible, as, during their dephlogistication, they combine with fixed air, or some particles of their menstruum; and hence their absolute weight is increased, though their specific gravity be somewhat diminished. From this last circumstance it appears, that the specific gravity of calces differs much less from that of their respective metals, than does the specific gravity which the phlogiston acquires by its union with those calces, from that which it possesses in its uncombined state; in the same manner as the density of quick-lime differs much less from that of lime-stone, than does the density which fixed air acquires by its union with quick-lime from that which belongs to it in its aërial state; and hence, instead of deducing the quantity of affinity of metallic calces to phlogiston from the following proposition, viz. that the affinity of metallic calces to phlogiston is in a compound ratio of its quantity and density in each metal, I am obliged to deduce it from this other, viz. that the affinity of metallic calces to phlogiston is directly as the specific gravity of the respective metals, and inversely as the quantity of calx contained in a given weight of those metals. This latter proposition is an approximation to the former, founded on this truth, that the larger the quantity of phlogiston in any metal is, the smaller is the quantity of calx in a given weight of that metal; and that the density which the phlogiston acquires, is as the specific gravity of the metal. This latter proposition, however, is not exactly true, for this density is much greater; yet it is the nearest approximation I can make, and its defect is sensible only with regard to those metals which contain a considerable quantity of phlogiston, viz. gold, copper, cobalt, and iron: with regard to the rest it is of no importance.

Then the specific gravity of metals being as represented in the 1st column of the annexed table, the affinity of their calces to phlogiston will be as is shown in the 2d column. The 3d column expresses these affinities in numbers homogeneous with those which express the affinities of acids with their basis.

	Specific gravity.		Affinity of the calces to phlogiston.
Gold.....	19	..	0.25 .. 1041
Mercury .....	14	..	0.147 .. 612
Silver .....	11.091	..	0.118 .. 491
Lead.....	11.33	..	0.116 .. 483
Copper.....	8.8	..	0.109 .. 454
Bismuth .....	9.6	..	0.099 .. 412
Cobalt .....	7.7	..	0.092 .. 383
Iron .....	7.7	..	0.090 .. 375
Regulus of arsenic ..	8.31	..	0.089 .. 370
Zinc.....	7.24	..	0.0817 .. 340
Nickel.....	7.33	..	0.0812 .. 333
Tin .....	7.	..	0.075 .. 312
Regulus of antimony	6.86	..	0.074 .. 308

Here we see, that the calx of lead has a greater affinity to phlogiston than the



calces of any of the imperfect metals, and hence its use in cupellation; for after it has lost its own phlogiston, it extracts that of the base metals, and thus promotes their calcination and vitrification.

Though the numbers in the second column express tolerably well the greater or less affinity of metallic calces to phlogiston, yet they have this inconvenience, that they are not homogeneous with those that express the affinities of acids to other bases, which limits their use to a narrow compass, being, on that account, incomparable with those that express the affinities of acids: I therefore endeavoured to find a coincidence between them in some one instance, in order to reduce them to the same standard, as will be seen in the numbers in the 3d column, which are homogeneous to those which express the affinities of acids to their basis.

The third point necessary for the explanation of the phenomena attending the solution of metals, and their precipitation by each other, is to determine the proportion of phlogiston which they lose by solution in each of the acids; and the affinity which their calces bear to the part so lost. Mr. K. was not able to determine this by any direct experiment; for though he might determine the part which escapes in the form of air, yet he could not that which is equally separated from the metal, but retained in the solution; yet from various collateral considerations he made out the proportion of phlogiston probably separated from the metals by the different acids.

*Of solutions in the vitriolic acid.*—This acid dissolves iron and zinc, without the assistance of heat; because its affinity to their calces is greater than the affinity which these calces bear to that portion of phlogiston which they must lose before they can unite to the acid; but all other metallic substances unite to this acid only where it is concentrated and heated.

*Of solutions in the nitrous acid.*—The nitrous acid has less affinity to all metallic substances than either the vitriolic or marine. It has also less affinity to them than they have to that portion of phlogiston which they must lose before they can unite to it; yet it dissolves them all (gold and platina excepted) even without the aid of heat, because it unites itself to phlogiston unless too dilute; and the heat produced by its union with phlogiston is sufficient to promote the solution.

*Of solutions in the marine acid.*—This acid is known to dephlogisticate metals less than any other. Where the portion of phlogiston, necessary to be separated, is more strongly attracted than the acid itself, it can operate no solution, or at least very slowly, without the aid of heat; nor even where the attraction of acid is stronger to the calx than that of the portion of phlogiston it separates, if the proportion of acid to such calx be very small; because so small a quantity of acid does not contain fire enough to volatilize the phlogiston; and hence heat is



necessary for the solution of lead in this acid. The dephlogisticated acid acts more powerfully.

*Of precipitations of and by iron.*—The mutual precipitations of iron and copper from the vitriolic acid by each other, have been well explained in a general manner by Mr. Monnet and Mr. Bergman; I shall here show the reason of these precipitations more distinctly. If a piece of copper be put into a saturate solution of iron, fresh made, no precipitation will happen, nor will any of the copper be dissolved in 12 hours, nor even in a longer time, if the access of air to the solution be prevented; but if the solution be exposed to the open air, the addition of a volatile alkali will show the copper to have been acted on in 24 hours, or sooner if heat be applied, and a calx of iron is precipitated.

With regard to a solution of iron in the marine acid, though exposed to the open air, copper precipitates nothing from it in 24 hours. But if a clean piece of iron be put into a solution of copper in the vitriolic acid, the copper is immediately precipitated. It is needless to add, that copper is in the same manner precipitated by iron from the nitrous and marine acids. Hence the practice of extracting copper from some mineral waters by means of iron. These waters therefore furnish afterwards, by evaporation, vitriol of iron; but it is remarkable that this vitriol is much paler than the common, and less fit for dyeing, 2 Schlutter 507. The reason of which is, that it is more dephlogisticated, not only because old iron is chiefly used, but because copper, containing more phlogiston than an equal weight of iron, deprives it of more of its phlogiston than it would lose if barely dissolved in the vitriolic acid. Cast iron, according to Schlutter, will scarcely precipitate a solution of copper; and in effect Mr. Bergman has found that it contains less phlogiston than bar iron. I have always found silver to be easily precipitated from its solution in the nitrous acid by iron. With regard to the nitrous acid, I found that zinc does not precipitate iron; but, on the contrary, iron precipitates zinc; but in a short time the acid re-dissolves the zinc, and lets fall the iron, which evidently proceeds from the too great dephlogistication of the calx of iron. But zinc precipitates iron from the marine, though with difficulty; for after 24 hours the galls still struck a black. Iron does not precipitate zinc from the vitriolic acid.

Most metallic substances, precipitated by iron from the nitrous acid, are in some measure re-dissolved shortly after, as the nitrous acid soon dephlogisticates the iron too much, then lets it fall, and re-acts on the other metals and re-dissolves them. The precipitation of the argillaceous earth from alum by iron is owing to the excess of acid in the alum which first dephlogisticates the iron; and when this is dephlogisticated, it attracts the acid more strongly. Earth of alum, on the other hand, precipitates iron when the solution of iron is dephlo-



gisticated by heat. It may also produce this effect by depriving iron of its excess of acid which keeps it in solution.

*Of precipitations of and by copper.*—When silver is dissolved in the nitrous acid, and a piece of copper is put into the solution, it sometimes happens that the silver is not precipitated, as Dr. Lewis has observed. This happens either when the nitrous acid is supersaturated with silver having taken up some in its metallic form, as already observed; or when the silver is not much dephlogisticated, for then its affinity to phlogiston, which is the principal cause of its precipitation, is less than 491; therefore, the remedy is to heat it and add more acid, by which it is dephlogisticated further. However, the nitrous acid always retains a little silver.

*Of precipitations of and by tin.*—Tin is not precipitated, in its metallic form, by any metallic substance; and the reason is, because its precipitation is not the effect of a double affinity, but of the single greater affinity of its menstruum to every other metallic earth. Metals that are precipitated from the nitrous acid by tin, are afterwards re-dissolved, because the acid soon quits the tin, it becoming too much dephlogisticated.

*Of precipitations of and by lead.*—Metals dissolved in the vitriolic and marine acids, and precipitable by lead, according to the indication of the balance of affinities, are yet slowly precipitated, because the first portions of lead that are dissolved form salts of difficult solution, which cover its surface, and protect it from the further action of the acid; and yet it contains so little phlogiston, that a great deal of it must be dissolved before it gives out enough to precipitate the dissolved metals.

*Of precipitations of and by mercury.*—Though the difference between the quiescent and divellent powers be very small, yet mercury is quickly precipitated from the vitriolic acid by copper; because the attraction of the calx of mercury to phlogiston is very strong, and a very small proportion of that contained in copper is sufficient to revive it. Silver does not precipitate mercury from the vitriolic acid, unless it contains copper; yet if silver and turpeth mineral be distilled, the mercury will pass in its metallic form, Wenzel 42; which shows that the affinity of calx of mercury to phlogiston is increased by heat. Mercury precipitates silver from the nitrous acid, not by virtue of the superiority of the usual divellent powers, but by reason of the attraction of mercury and silver to each other, for they form partly an amalgama and partly vegetate, and scarce any of either remains in the solution. The same thing happens, that is, they vegetate, if solutions of both metals in the same acid be mixed together. Silver does not precipitate mercury from the solution of sublimate corrosive; but, on the contrary, mercury precipitates silver from the marine acid: and if a solution of horn silver in volatile alkali be triturated with mercury, the silver will be freed



from its acid and calomel formed, 1 Margraaf 284; and yet, if calomel and silver be distilled, the mercury will pass in its metallic form, and horn silver will be formed, *ibid.* 286.

*Of precipitations of and by bismuth.*—With respect to the vitriolic acid I have made the sum of the quiescent and divellent powers equal, though in fact sometimes the one preponderates and sometimes the other. Bismuth precipitates nothing from vitriol of copper in 16 hours; nor does copper from vitriol of bismuth. Copper is said to precipitate bismuth from the nitrous acid; but I have also seen copper precipitated from this in its metallic form by bismuth. The variations proceed from the different dephlogistication of copper.

*Of precipitations of and by nickel.*—Unless nickel be pulverised it scarcely precipitates any metal. Zinc precipitates a black powder from the solution of nickel in the vitriolic and nitrous acid, which Mr. Bergman has shown to consist of arsenic, nickel, and a little of the zinc itself. The arsenic attracting the calx of nickel; but zinc precipitates nickel from the marine acid.

The solution of iron in vitriolic acid acts on nickel, and that of nickel in this same acid acts on iron; but neither precipitates the other in 24 hours; but on longer rest, iron seems to have the advantage; but iron clearly precipitates nickel from the nitrous acid; and though nickel seems also to precipitate iron, yet this arises only from the gradual dephlogistication of the iron.

Nickel precipitates copper in its metallic form from the vitriolic acid. It also precipitates copper from the nitrous and marine acids; but copper precipitates arsenic from a nitrous solution of nickel. The vitriolic and nitrous solutions of lead seem to act in specie on nickel, that is, to dissolve it without any decomposition, the calces uniting to each other. The vitriolic and nitrous solutions of nickel for some time act on lead in the same manner; but at last nickel seems to have the advantage. With regard to the marine acid, lead seems to have the advantage, though a black precipitate is seen, whichever of them is put into the solution of the other.

Nickel readily precipitates bismuth from the vitriolic and nitrous acids; but as to the marine I found each of these semi-metals soluble in the solution of the other, yet nickel precipitates bismuth very slowly, and only as to part; and bismuth precipitates a red powder, which I take to be ochre, from the solution of nickel.

Nickel and tin are slightly acted on, each by the salt of the other.

*Of precipitations of and by cobalt.*—Cobalt is not precipitated either from the vitriolic or nitrous acid by zinc; but it seemed to be precipitated by zinc from the marine acid. Though iron precipitates cobalt from the three acids, yet I found much of the cobalt retained both by the vitriolic and nitrous acids, particularly the latter, which, after letting fall the cobalt, afterwards re-takes it, and lets fall



the dephlogisticated calx of iron. Nickel also, though it does not precipitate cobalt itself, as appears by the remaining redness of the solution, yet constantly precipitates some other heterogeneous substance from it. The solution of cobalt in the marine acid becomes colourless by the addition of nickel. Bismuth is soluble in the vitriolic and nitrous solutions of cobalt, and causes a small white precipitate, but does not affect the true cobaltic part. These solutions in vitriolic acid cannot be attributed to an excess of acid, as they are made in a dilute acid, and without heat. Copper also precipitates a white substance from the nitrous solution of cobalt, which I take to be arsenic.

*Of precipitations of and by regulus of antimony.*—Copper neither precipitates, nor is precipitated from, the vitriolic acid by regulus of antimony, at least in 3 days; but vitriol of antimony in specie dissolves it slowly. The regulus is also acted on by vitriol of lead, for it becomes red after remaining 16 hours in the solution of that vitriol; and lead scarcely precipitates it from the vitriolic acid. Powdered regulus precipitates vitriol of mercury very slightly. Bismuth neither precipitates, nor is precipitated by this regulus from the vitriolic acid in 24 hours. Though tin precipitates this regulus from the nitrous acid, yet if the regulus be put into a solution of tin in this acid, in 16 hours neither will be found in the solution, either by reason of the dephlogistication, or of the union of the calces to each other. Iron does not precipitate this regulus entirely from the marine acid; but a triple salt seems to be formed, consisting of the acid and both calces. The regulus is also soluble in marine salt of iron. Neither does copper precipitate the regulus from marine acid in 16 hours; and if the regulus be put into marine salt of copper it will be dissolved, and volatile alkalis will not give a blue but a yellowish white precipitate, so that here also a triple salt is formed.

*Of precipitations of and by regulus of arsenic.*—The solutions of arsenic act in most cases like two acids: thus iron, copper, lead, nickel, and zinc, are acted on by vitriol of arsenic (that is, its solution in vitriolic acid) but scarce give any precipitate. Neither does iron precipitate arsenic from the nitrous acid, but copper does, and even silver gives a slight white precipitate; but regulus of arsenic precipitates silver completely in 16 hours. Hence the former precipitate seems to be a triple salt. Mercury also slightly precipitates arsenic from the nitrous acid, and seems to unite to it, yet is itself precipitated by regulus of arsenic in 24 hours. Bismuth forms a slight precipitate in the nitrous solution of arsenic; but regulus of arsenic forms a copious precipitate in the nitrous solution of bismuth; so that I believe the calces unite. Nickel does not precipitate arsenic from the nitrous acid, but both calces unite; but regulus of arsenic produces a copious precipitate in the nitrous solution of nickel, yet the liquor continues green; so that certainly the nickel is not precipitated; the white precipitate in this case seems to be slightly dephlogisticated arsenic. This regulus also



causes a white precipitate in the nitrous solution of cobalt, but the liquor still continues red.

With regard to the marine acid, copper precipitates the regulus, but volatile alkalis do not strike a blue with this solution; which shows that the copper unites with the arsenic. Iron also precipitates the arsenic. Tin is soluble in marine solution of arsenic; but I could observe no precipitate, nor does regulus of arsenic precipitate tin. Neither bismuth nor the regulus of arsenic precipitate each other from the marine acid in 16 hours. Regulus of antimony is also acted on by the marine solution of arsenic, though it causes no precipitate, nor does the regulus of arsenic precipitate it.

*IV. On a Species of Sarcocoele of a most astonishing Size in a Black Man in the Island of Senegal; with some Account of its being an endemial Disease in the Country of Galam. By J. P. Schotte, M. D.. p. 85.*

There are certain diseases which are peculiar to certain countries only, and are thence called endemial ones of such particular countries where they occur. The more progress we make in the discoveries of countries, the more we are convinced of this fact, and the greater is the number of those diseases that become known. Their formation may depend on climate, food, water, hereditary disposition, and other causes. Many endemial diseases of the most distant countries have been described by ingenious travellers; but as the Europeans have not yet penetrated into the interior parts of many countries, it is probable, that there may be several more of this kind, entirely unknown to us. A disease of this class, which I have seen at Senegal (says Dr. S.) and which, as far as I know, has not yet been mentioned by any author, convinces me of what I have advanced; and as it is a remarkable one, I think a short description of it may not be unacceptable to the curious in physic.

Mr. Bishopp, surgeon in chief of the province of Senegambia (who now resides in London) telling me one day, that he was going to see a poor black man of the Bambara nation, afflicted with a most extraordinary and dreadful disease in his testicles, I accompanied him, being glad of the opportunity of seeing it. We entered the hut, and saw the man lying on a negro-bed, elevated about a foot from the ground. He said to Mr. Bishopp, that there was again an ulcer on his scrotum, which had made him take the liberty to request his attendance. I looked at the scrotum, and found it of an astonishing size; but the place where he lay being dark, the hut having no windows, and those people having no candles, he was asked, if he could not walk towards the door, that we might see better. He answered, that he would try; but this was attended with much difficulty. A long cotton sheet was first spread on the ground before the bed, which being done, he took, with both his hands, the enormous scrotum, moved



it gradually on the border of the bed, let it slide down gently, and put it into the middle of the sheet: after this he took the two ends of the sheet, passed them up the fore part of his body, over his shoulders, and had them tied behind his neck. He then got up, placing the right hand on his right thigh, and holding the sheet with the left hand, and proceeded in this manner, with his knees a little bent, slowly towards the door, partly sliding the scrotum on the ground, and partly supporting it with his neck by means of the sheet. I was astonished at its enormous size, when I saw it in the light, and yet I neglected to measure it, thinking at the time, as is often the case, that I should have opportunities enough to do it; but the sudden invasion of the island by the French prevented me afterwards from performing it. However, according to my guess, and without any exaggeration, the whole mass might be about 2 feet and a half long, from the os pubis to its lower extremity, and about 18 inches in diameter across from thigh to thigh.

Its weight I will only state at 50 pounds, as it was estimated by Mr. Bishopp, though I believe it to have been more, and indeed from its dimensions, and from its being a solid mass, it must certainly have exceeded that weight. - It was of an oblong form, and resembled in some measure the shape of the scrotum of a bull. It felt very hard to the touch, and the skin of it was so tight, that it could not be pinched by the fingers. The penis was quite hid in the bulk, as generally happens when the scrotum is much extended, and may be easily comprehended by those who have seen large ruptures. The skin of the perinæum and of the abdomen was drawn downwards, the navel being nearer to the os pubis than in the natural state. There was a large aperture formed by the skin about a foot downwards from the os pubis, rather inclining towards the right side, out of which the urine came, which however did not run in a stream, but came irregularly from all the interior sides of the aperture. When he made water, he inclined the mass, which rested on the ground, a little forwards, and he held a wooden bowl close underneath the aperture, into which the urine was immediately received, that it might not run along the mass, and occasion excoriation.

There was an ulcer on the anterior part of the scrotum, rather towards the left side, of about 2 inches long, and 1 inch broad and deep. He said that it had begun with a pustule or boile, which being broken had gradually increased to this extent. The pus which came from it was white, thick, and of a good kind. The bottom of it was red, and, when touched with the probe, gave him very acute pain. The edges of it were not very callous, and in appearance it did not much differ from an ulcer of a good kind in any other fleshy part of the body. No other remedies were applied to it but those generally used in common ulcers. It was filled up from the bottom with lint; a pledget of basilicum was put over



it, and the edges were now and then touched with blue vitriol. By those means granulations began to shoot from all sides, the sore filled up gradually, and a cicatrix was formed. He had had smaller ulcers of this kind in other parts of the scrotum before this time, which Mr. Bishopp said he had treated with the same success.

The man was rather thin than fat, and might be about 50 years old. His abdomen seemed rather empty, and appeared drawn in towards the spine; yet I do not think, that any of the intestines had descended into the scrotum, or if any had passed down, the annuli of the abdomen must have been so dilated as not to occasion the least obstruction in them; for he never had, to my knowledge, any of those complaints or symptoms which attend ruptures. Besides, ruptures are not very common among the blacks about Senegal; indeed I can say, that I never saw one of them.

This man, it seems, had been purchased up the river as a slave, when he was about the age of puberty, and brought down to Senegal, where he was kept as a house-servant by an opulent inhabitant. He was for some years healthy and well; but afterwards his testicles began to swell insensibly, without inflammation, pain, or any other inconvenience. They increased gradually, though slowly, and became some years after of such a bulk, that he was neither able to walk nor perform his usual work. That he might however not be quite idle, as he was otherwise a stout and able fellow, he used to cut bars of iron into pieces of a foot long, which bear a certain price at Senegal, and go among the blacks like current money. This he could do sitting with a chisel and hammer, and a small anvil placed before him on the ground, his legs bent under him, and the scrotum resting on the ground. Mr. Bishopp had seen him perform this work for many years; at last however the scrotum increased to such a degree, that the great bulk prevented him from doing it any longer. From the time that the disorder had first begun to shew itself to the time I saw him, 25 years had elapsed; he was alive when I left the island in February 1779, and may be so now.

This man was the only one I ever saw afflicted with this disease at Senegal; but I am credibly informed, that it is endemial in a country which goes, among the blacks at Senegal, by the general name of Galam, and of which this man was a native. This country lies east of Senegal, at the distance of about 900 English miles, and its inhabitants are called Bambaras. I have been told by those inhabitants of Senegal, who go annually in the rainy season in a fleet of small craft to Galam for trade, that this disease is particularly common among the chiefs or noblemen of that country.

When I was at Fort James in the river Gambia, for a short time in the year 1776, I was told by some Marahbuts, or Mahometan priests, of the Mandinga



nation, that this disease was now and then to be met with among the chiefs of their nation, and that they knew no cure for it.\* I have no reason to discredit their assertion, and what makes it more probable is, that the Mandinga and Bambara nations seem to be nearly related to each other in outward appearance, customs, and language, though not entirely in religious matters; for many of the Mandingas are Mahometans, which the Bambaras are not. Their languages

\* It is to be observed, that those Marahbuts apply themselves, besides religious matters, to the study of physic; but only as far as it rests on experience alone, without entering into the investigation of the causes of diseases. They are also often called on by the kings and chiefs to give their opinion in points of law and equity. Most of them are well versed in the Arabic language of the Mauritanic dialect, and they are the only people of letters among the blacks; for none of the black nations about Senegal and Gambia have even an alphabet, much less any writings in their own languages. I believe the selling of charms constitutes the greatest part of their revenue: and the more reputation one of them has acquired, the dearer he sells them. Those charms usually consist in nothing but a few lines taken from the Koran, written on a little piece of paper, which, after being sewed up very nicely in leather or cloth, the buyers wear about their bodies. They are to defend and protect them in dangers; but, as one charm has only the power of protecting them against one single kind of danger, they are obliged to have a great many of them, in order to have a protection against every probable danger that may befall them; hence many of the blacks are covered with them in different parts of the body; and they have such a strong faith in them, that when they are surprized in the night-time by an enemy, they will not take up arms for their own defence, though in the most imminent danger, till they have dressed themselves with those charms, and then they will meet him undauntedly. This faith in charms, however, is a corruption of the Mahometan religion, and the Moors, who live on the north side of the river Senegal, observing it in its purity, make no use of them. The Marahbuts of the black nations, as well as those of the Moors, are also the principal merchants and the most opulent people among them, and the gum trade on the river Senegal is chiefly carried on by those of the Moors. The Marahbuts are also the only people who can travel with any safety into distant kingdoms, which no layman can well do without running the risk of being made a slave. Their religious profession protects them every where; they are even respected among those nations who are not Mahometans; and they are considered by them as godly and virtuous people, and men of wisdom. They make proselytes in the Mahometan religion every where; and I am inclined to believe, that they will extend and spread it in time all over Africa. I have seen some Marahbuts of the Pool or Fool nation at Senegal who were pretty well versed in the old testament, and knew partly the history of the institutor of the new one. One day as I was talking with them on the writings of Moses, happening not rightly to recollect the lineage from Adam to Abraham, one of them flattened the sand, made it even, and drew with his fingers on it the genealogy from Adam down to Jacob, which, to the best of my recollection, corresponded with that given by Moses. While he was doing this, I looked at him with pleasure and satisfaction, because it resembled so much the rude simplicity of the earlier ages. The Marahbuts reason in general exceedingly well on such subjects as they are acquainted with; but they have a way, like the eastern nations, of adducing parables or similes in their arguments which do not always bear the strictest resemblance to the case in hand, though they are very persuasive with such people as are not capable of investigating the points in which they differ from the case in question. I was always much delighted with their conversation, and was often sorry that I was not master of their different languages, and able to converse with them without an interpreter. The Marahbuts of the Moors are more learned and ingenious in every respect than those of the black nations; but I had not much opportunity of conversing with them, as they were not allowed to reside on the island.—Orig.



resemble each other so nearly that a Bambara from Galam, and a Mandinga from the kingdom of Barra, which extends from the sea-coast along the north side of part of the river Gambia, can partly understand each other. Both nations have also a custom of marking their children in various manners by incisions in the skin, and that of filing their fore teeth (incisores) till they become quite pointed, which I imagine they consider as being handsome.

As the disease, according to the information I received, begins with a gradual swelling of the testicles without any pain or inflammation, I am inclined to consider it as a sarcocoele. Heister, in his *Surgical Institutions*, says, that the disease begins and increases mostly in the same manner, when it affects the testicles themselves; but that he never saw any of them much larger than a man's fist. This difference in the size does not, in my opinion, alter the disease; for we know, that the bronchocele is hardly known in some countries, that it is of a moderate size in some others, and that in others again it has been seen to increase to such an enormous bulk as to hang down over the breast and belly; yet this difference of size does not alter the nature of the disease, and it still retains the same name.

It is difficult to point out the causes of such a sarcocoele, as consists in the spontaneous tumefaction of the testicles themselves; neither do I find any satisfactory ones assigned by the author I have just now quoted; and as I have not been in Galam, I can hardly say any thing probable concerning those of the disease I have described. I shall, however, suggest the following. As polygamy is lawful and customary among the Bambaras, as well as among all the other nations about the river Senegal and Gambia, and as the riches and consequence of a man are estimated by the number of wives that he keeps, the chiefs of the people have always a great number of them. I have been told, that the Batcherees of Galam have their victuals most immoderately seasoned with Cayenne pepper; and I know myself, that the opulent people of the Mandinga nation make the same abuse of it. This may perhaps be done with a view to its operating as a provocative; for it has a peculiar effect on the seminal vessels, and will produce erections, attended with a dull pain and turgescency in the testicles: I was therefore inclined to think, that the immoderate use of this pepper might partly be the cause of this disease; but then again this could not be the case in the man I saw at Senegal, where none, or at least very little of it, is used. The most probable cause of it seemed to be an hereditary disposition; for, as it only begins to show itself about the age of 25 and 30, a man may be father of a great many children before it takes place, and as it seems to be confined to families of the principal people of the Bambara nation, it may be, that the man I saw afflicted with it at Senegal was descended from such a family, and made a slave.



in his younger years by some fatal accident or other, as is often the case in those countries.

*V. A Description of a New Construction of Eye-glasses for such Telescopes as may be applied to Mathematical Instruments. By Mr. Ramsden. p. 94.*

To correct the errors in eye-glasses, arising from their spherical figure, and also from the different refrangibility of light, it has been held absolutely necessary to have 2, placed in such manner, that the image formed by the object-glass of the telescope should be between them; but in those telescopes that are applied to mathematical instruments, the interference of the first eye-glass before the image is formed is productive of many bad consequences; should that eye-glass have the least shake or motion whatever, it totally alters the adjustment of the instrument; and the diminishing also of the image by this position, obliging us to shorten the focus of the nearer eye-glass, the wires in the focus of the telescope are thereby considerably more magnified than they would have been with the same power, had both the eye-glasses been put between the image and the eye.

Many defects in the micrometer with moveable wires are caused by the construction of the eye-glasses of the telescope to which it is applied. If only one eye-glass be used, the field is so contracted, that it is impossible to measure the diameter of the sun or moon with precision, if the telescope magnifies above 30 times; and if, to enlarge the field, we use the present construction of 2 eye-glasses, the consequence is yet worse; because equal spaces between the wires will not then correspond to equal spaces on the objects it represents, as those conversant in the theory of optics well know; and this inequality depending on the form, position, and refractive power of the first eye-glass, it will be impossible to have data sufficiently exact to allow for that error.

Those who were sensible of this defect have thought to correct it by the application of an achromatic eye-glass, on the principle of that kind of object-glass, not supposing it possible to correct the aberrations from the different refrangibility of light, and also from the spherical figure of the lenses by any other means than combining a concave lens with the convex ones; but the violent and contrary refractions from the necessary large size of the lenses, in proportion to their focal lengths, not only occasioned great loss of light, but rendered it impossible to correct the spherical aberration so as to obtain an angle of vision much larger than could be had by a single eye-glass; yet, however absurd it may have appeared to attempt correcting both aberrations, when the lenses are both convex, and are on the near sides of the wires, the following observations will show the practicability of it, and may throw some light on the theory of eye-glasses which seems hitherto not well understood.



Sir Isaac Newton has shown, in his *Lectiones Opticæ*, in that section *De Phænomenis Lucis per Prisma in Oculum transmissæ*, that the appearance of colours on the edges of objects, when viewed through a prism, depends on the proportion of the distance between the prism and the object, compared with that between the prism and the eye, that is to say, the nearer the object is brought to the prism, the less will be the *bordre* of colours on the contours of the object. To apply this to practice, says Mr. R., I placed a plano convex lens *a* (fig. 1, pl. 6,) with its plane side near an object, or an image *IN* formed by the object-glass of a telescope, and thus magnified the image which, from the position of the lens, was sensibly free from colour; but the respective foci of a lens so placed being very near each other, and on the same side, the emergent pencils diverge on the eye, and give indistinct vision: this was remedied by placing a second lens *k* a little within the focus of the former, the combined foci of the two lenses being in the place of the image, the rays were thus made to fall parallel on the eye, and to show the object *IN* distinctly. If, by putting the lens *a* very near the image, any imperfection in it become too visible, that distance may be considerably increased, without producing any bad effect; for theory, as well as experiment, shows, that a small aberration from the different refrangibility of light is of little consequence, compared with the same quantity of aberration caused by the spherical figure of the lenses; but even that colouring may be corrected in the nearer eye-glass: for let a ray (fig. 2) from an object *o*, by passing through a lens *B*, be separated into colours, *ac* being the direction of the violet rays, and *at* that of the red; if another lens be put at *c*, the violet rays passing through its centre will suffer no refraction, while those of the red, passing at some distance from thence, are refracted, and the emergent red and violet will be parallel, when the mean refracting angle of the lenses, at the incidence of each pencil, are to each other inversely as the diameters of those pencils. If we attend to this position of the eye-glasses, it will be found equally advantageous for obviating the spherical aberration of an oblique pencil as that from colour. In both, where there is a necessity for having a large portion of a sphere, we have only to make the pencil on such lens as small as possible, and we may regulate the direction of the rays in each pencil at pleasure when they approach the axis of the telescope.

To illustrate this, let us compare the effect of the spherical aberration of a lens on an oblique pencil in this position, with that produced by the same lens, placed as usual at its focal distance from the image. Let *AC*, fig. 3, represent the semi-object-glass of a telescope, *CT* its axis, *E* an eye-glass, and *F* the common focus of both the object-glass and eye-glass. Let *AFH* be an oblique pencil of homogeneous rays, *G* and *H* the points where the axis and the extreme rays pass through the eye-glass: the aberration of this pencil from the spherical



figure of the lens  $E$  will be  $EG^3 - EH^3$ ; but as the lens approaches towards  $F$ ,  $EG$  and  $EH$  becoming equal, this cause of aberration vanishes accordingly. The effects of the lens  $k$  will be altogether insensible from the smallness of its aperture; or it might be corrected in the figure of the object-glass, by making its aberration negative as much as this is affirmative.

It has been usual to consider that form and position of the eye-glasses best that would make the pencils from every part of the field intersect each other in the axis of the telescope at the place of the eye; but this will be found of little consequence, seeing the diameter of a pencil here is generally much less than the pupil, nothing more is requisite than that the eye may take in the pencils from the different parts of the field at the same time: but the field of a telescope will be most perfect when the construction of the eye-glasses is such, that the focus of an extreme and of a central pencil are at the same distance from the eye. The disposition above described will be found conformable to that idea.

Let  $AB$  fig. 4, represent an image formed by the object-glass of a telescope,  $v$  the first eye-glass, as already described, with its plane side towards the image; let  $AC$  be the axis of a pencil of rays incident on the first surface of the lens  $v$ , and  $Ae$  an extreme ray of the same pencil. Take  $CF$  to  $CA$ , as the sine of incidence out of the air into glass, to the sine of refraction, and  $F$  will be the focus of this pencil after passing through the first surface of the lens  $v$ . From the point  $F$  draw the angle  $CFE$ , the incident pencil on the second surface of this lens, continue the lines  $FC$  and  $FE$  to  $b$  and  $r$  respectively, and draw the perpendiculars  $or$  and  $ok$ , on the point  $c$  describe the arc  $nd$ , and making  $cd$  to  $ab$ , as the sine of refraction out of glass into air, to the sine of incidence, draw  $cd$  continued till it cuts the axis in  $p$ . In like manner, on a centre  $e$  describe the arc  $mg$ , and making  $yg$  to  $or$  as the sine of the angle of refraction to that of incidence, draw the line  $ega$ ; continue it and the line  $cd$  backward till they meet each other in  $h$ , and it will be the focus of the emergent pencil from the second surface of the lens  $v$ . On the axis  $CF$  set off the distance  $cs$  equal to  $ch$ , and draw  $es$  and  $ce$ . Now it is evident from the figure, that the focus of the emergent pencil will be nearer to  $c$  than the object itself, in proportion as the angle  $cse$  exceeds the angle  $cae$ . Thus, from the great angle of incidence of the oblique pencil on the second surface of the lens, the focus of the emergent pencil is brought nearer to  $p$  the second eye-glass, while that of the principal pencil remains the same, or very nearly so; and the image will become more distinct towards the edge of the field the nearer  $ph$  and  $pr$  approach to equality.

To give a proper demonstration and theorem for the exact form of the first lens, according to its distance from the image, would require more leisure than is consistent with the situation of one not very conversant with mathematics. That distance in proportion to the focal length of the lens, so that any una-



voidable defect in it may become invisible, must be determined by experiment. If any variation be made in the form of this lens, it will be better to make the plane side rather a little convex than concave. By the latter the image would be distorted by the too great obliquity of the rays near the extremity of the lens.

Thus we have a system of eye-glasses which may be taken out of the telescope, in order to wipe them at pleasure. Or the magnifying power of the telescope may be varied without affecting the line of collimation, or in any manner altering the adjustment of the instrument to which such telescopes may be applied, with many other advantages. In the present improved state of telescopes too, the disagreeable appearance of the wires from the great power of the eye-glasses is in a great degree remedied. The same principle may be usefully employed in many other cases. What is herein contained is only to be considered as an explanation of this very useful construction, and which is given in hopes that some person of more abilities in the science of optics will favour us with a general theorem, in order that its application may be more universal.

*VI. Account of several Lunar Rainbows. By Marmaduke Tunstall, Esq.*  
F. R. S. p. 100.

The first iris was seen the 27th February, 1782, between 7 and 8, in tolerable distinct colours, similar to a solar one, but more faint; the orange colour seemed to predominate. The night was windy, and though there was then a drizzling rain and dark cloud, in which the rainbow was reflected, it proved afterwards a light frost.

The 2d lunar iris happened July the 30th, 1782, about 11 o'clock, which lasted about a quarter of an hour, without colours. The last was on Friday, Oct. the 18th, 1782, perhaps the most extraordinary one of the kind ever seen. It was first visible about 9 o'clock, and continued, though with very different degrees of brilliancy, till past 2. At first, though a strongly marked bow, it was without colours; but afterwards they were very conspicuous and vivid in the same form as in the solar, though fainter; the red, green, and purple, were most distinguishable. The wind was very high most part of the time, accompanied with a drizzling rain. One particular, rather singular, in the 2d, viz. of July the 30th, was its being 6 days after the full of the moon, and the last, though of so long a duration, was 3 days before the full; that of the 27th of February was exactly at the full, which used to be judged the only time they could be seen; though in the Encyclopedie there is an account that Weidler observed one in 1719, in the first quarter of the moon, with faint colours, and in very calm weather.

*VII. Account of an Earthquake. By John Lloyd, Esq.* p. 104.

On Saturday Oct. 5, 1782, between 8 and 9 o'clock in the evening, a shock



of an earthquake was felt in several parts of Wales, by many persons, though not generally. At Mold, in the county of Flint, it was distinctly perceived by a gentleman, at that time in a house quite out of the town, and seemed attended with a rumbling noise, like a carriage going over a pavement; and at the same time some China cups and saucers rattled very much, that were on a table in the room with him. At the palace, at Bangor, it was perceived by all the bishop's family at about 39 minutes past eight o'clock, with the same kind of rumbling and a double vibration. Many other persons in that neighbourhood were sensible of it. In many places in the isle of Anglesey it was strongly felt; at Bodorgan, the seat of Owen P. Meyrick, Esq., it was thought by the family that a carriage had driven up to the door. In answer to some inquiries made, Mr. L. received the following account from an ingenious friend, who was concerned in the great copper mine at Paris Mountain, and was at that time within a mile of the mine at his own house.

“ I perceived the earthquake to begin at Amlwoh at 40<sup>m</sup> past 8 o'clock at night, on Saturday the 5th of October. The shock was great and alarming. The house was shaken terribly, and underwent several vibrations for the continuance of near a quarter of a minute. I thought it moved from N. E. to S. W. but was not certain. It was attended with a rumbling noise, as loud as thunder, and like it just before it ceases.”

*VIII. Of a New Eudiometer. By Mr. Cavendish, F. R. S. p. 106.*

Dr. Priestley's discovery of the method of determining the degree of phlogistication of air by means of nitrous air, has occasioned many instruments to be contrived for the more certain and commodious performance of this experiment; but that invented by the Abbé Fontana is by much the most accurate of any hitherto published. There are many ingenious contrivances in his apparatus for obviating the smaller errors which this experiment is liable to; but the great improvement consists in this, that as the tube is long and narrow, and the orifice of the funnel not much less than the bore of the tube, and the measure is made so as to deliver its contents very quick, the air rises slowly up the tube in one continued column; so that there is time to take the tube off the funnel, and to shake it before the airs come quite in contact, by which means the diminution is much greater and much more certain than it would otherwise be. For instance, if equal measures of nitrous and common air are mixed in this manner, the bulk of the mixture will, in general, be about one measure; whereas, if the airs are suffered to remain in contact about  $\frac{1}{4}$  of a minute before they are shaken, the bulk of the mixture will be hardly less than 1 measure and  $\frac{2}{10}$ , and will be very different according as it is suffered to remain a little more or a little less time before it is shaken. In like manner, if, through any fault in the apparatus,



the air rises in bubbles, as in that case it is almost impossible to shake the tube soon enough, the diminution is less than it ought to be. Another great advantage in this manner of mixing is, that the mixture receives its full diminution in the short time during which it is shaken, and is not sensibly altered in bulk after that; whereas, if the airs be suffered to remain some time in contact before they are shaken, they will continue diminishing for many hours.

The reason of the abovementioned differences seems to be, that in the Abbé Fontana's method the water is shaken briskly up and down in the tube while the airs are mixing, by which each small portion of the nitrous air must be in contact with water, either at the instant it mixes with the common air, or at least immediately after; and it should seem, that when the airs are in contact with water during the mixing, the diminution is much greater and more certain than when there is no water ready to absorb the nitrous acid produced by the mixture. This induced me to try whether the diminution would not be still more certain and regular if one of the two kinds of air was added slowly to the other in small bubbles, while the vessel containing the latter was kept continually shaking. I was not disappointed in my expectations, as I think this method is really more accurate than the Abbé Fontana's; and besides, in the course of my experiments I had occasion to observe a circumstance which is necessary to be attended to by those who would examine the purity of air with exactness by any kind of eudiometer, as well as some others which tend very much to explain many of the phenomena attending the mixture of common and nitrous air.

The apparatus I use is as follows. A (fig. 5, pl. 6), is a cylindrical glass vessel, with brass caps at top and bottom: to the upper cap is fitted a brass cock B; the bottom cap is open, but is made to fit close to the brass socket dd, and is fixed in it in the same manner as a bayonet is on a musket. The socket dd has a small hole E in its bottom, and is fastened to the board of my tub by the bent brass EFG, in such manner that b, the top of the cock, is about half an inch under water: consequently if the vessel A is placed in its socket, with any quantity of air in it, and the cock is then opened, the air will run out by the cock, but will do so very slowly, as it can escape no faster than the water can enter by the small hole E to supply its place.

Besides this vessel, I have three glass bottles like M (fig. 6), each with a flat brass cap at bottom to make it stand steady, and a ring at top to suspend it by, and also some measures of different sizes such as N (fig. 7); these are of glass with a flat brass cap at bottom and a wooden handle. In using them they are filled with the air wanted to be measured, and then set on the brass nob C fitted on the board of my tub below the surface of the water, which drives out some of the air, and leaves only the proper quantity. This measure is easier made, and more expeditious in using, than the Abbé Fontana's, and I believe is equally



accurate; but if it were not it would not signify, as I determine the exact quantity of air used by weight.

There are 2 different methods of proceeding which I have used; the first is to add the respirable air slowly to the nitrous; and the other, to add the nitrous air in the same manner to the respirable. The first is what I have commonly used, and which I shall first describe. In this method a proper quantity of nitrous air is put into one of the bottles *m*, by means of one of the measures above described, and a proper quantity of respirable is let into the vessel *A*, by first filling it with this air, and then setting it on the knob *c*, as was done by the measure. The vessel *A* is then fixed in the socket, and the bottle *m* placed with its mouth over the cock. Then on opening the cock, the air in the vessel *A* runs slowly in small bubbles into the bottle *m*, which is kept shaking all the time by moving it backwards and forwards horizontally while the mouth still remains over the cock.

Notwithstanding the precautions used by the Abbé Fontana in measuring the quantity of air used, I have sometimes found that method liable to very considerable errors, owing to more water sticking to the sides of the measure and tube at one time than at another: for this reason I determine the quantities of air used and the diminution, by weighing the vessels containing it under water in this manner. From one end of a balance, placed so as to hang over the tub of water, is suspended a forked wire, to each end of which fork is fixed a fine copper wire; and in trying the experiment the vessel *A*, with the respirable air in it, is first weighed, by suspending it from one of these copper wires, in such manner as to remain entirely under water. The bottle *m*, with the proper quantity of nitrous air in it, is then hung on in the same manner to the other wire, and the weight of both together found. The air is then let out of the vessel *A* into the bottle *m*, and the weight of both vessels together found again, by which the diminution of bulk which they suffer on mixing is known. Lastly, the bottle *m* is taken off, and the vessel *A* weighed again by itself, which gives the quantity of respirable air used. It is needless to determine the quantity of nitrous air by weight; because, as the quantity used is always sufficient to produce the full diminution, a small difference in the quantity makes no sensible difference in the diminution. In this manner of determining the quantities by weight, care should be taken to proportion the lengths of the copper wires in such manner that the surface of the water in *A* and *m* shall be on the same level when both have the usual quantity of air in them, as otherwise some errors will arise from the air being more compressed in one than in the other. This precaution indeed does not entirely take away the error, as the level of the water in *m* is not the same after the airs are mixed as it was before; but in vessels of the same size as mine, the error thence arising can never amount to the 500th part of the whole, which is not worth regarding; and



indeed if it were much greater, it would be of very little consequence, as it would be always the same in trying the same kind of air.

The vessel A holds 282 grains of water, and is the quantity which I shall distinguish by the name of 1 measure. I have 3 bottles for mixing the airs in, with a measure B for the nitrous air adapted to each. The first bottle holds 3 measures, and the corresponding measure  $1\frac{1}{4}$ ; the 2d bottle holds 6; and the corresponding measure  $2\frac{1}{2}$ ; and the 3d bottle holds 12, and the corresponding measure 5. The first bottle and measure is used in trying common air, or air not better than that; the other two in trying dephlogisticated air. The quantity of respirable air used, as before said, is always the same, namely, 1 measure; consequently, in trying common air, I use  $1\frac{1}{4}$  measure of nitrous air to 1 of common; and in trying very pure dephlogisticated air, I use 5 measures of nitrous air to 1 of the dephlogisticated. I believe there is no air so much dephlogisticated as to require a greater proportion of nitrous than that. The way by which I judge whether the quantity of nitrous air used is sufficient, is by the bulk of the two airs when mixed, for if that is not less than one measure, that is, than the respirable air alone, it is a sign that the quantity of nitrous air is sufficient, or that it is sufficient to produce the full diminution, unless it be very impure.

Though the quantity of respirable air used will be always nearly the same, as being put in by measure; yet it will commonly be not exactly so, for which reason the observed diminution will commonly require some correction; for example, suppose that the observed diminution was 2.353 measures, and that the quantity of respirable air was found to be .985 of a measure; then the observed diminution must be increased by  $\frac{1}{1000}$  of the whole or .035, in order to have the true diminution, or that which would have been produced if the respirable air used had been exactly 1 measure; consequently the true diminution is 2.388.

The method of weighing, before described, is that which I use in trying air much different in purity from common air; but in trying common air, I use a shorter method, namely, I do not weigh the vessel A at all, but only weigh the bottle M with the nitrous air in it; then mix the airs, and again weigh the same bottle with the mixture in it, and find the increase of weight. This, added to 1 measure, is very nearly the true diminution, whether the quantity of common air used was a little more or a little less than 1 measure. The reason of this is, that as the diminution produced on mixing common and nitrous air is only a little greater than the bulk of the common air, the bulk of the mixture will be very nearly the same, whether the bulk of the common air is a little greater or a little less than 1 measure: for example, let us first suppose, that the quantity of common air used is exactly 1 measure, and that the diminution of bulk on mixing is 1.08 of a measure, then must the increase of weight of the bottle M, on



adding the common air, be .08 of a measure. Let us now suppose, that the quantity of common air used is 1.02 of a measure, then will the diminution, on adding the common air, be  $1.08 \times \frac{1.02}{1.00}$  or 1.1016 of a measure, and consequently the increase of weight of the bottle M will be  $1.1016 - 1.02$  or .0816 of a measure, which is very nearly the same as if the common air used had been exactly one measure.

In the 2d method of proceeding, or that in which the nitrous air is added to the respirable, I use always the same bottle, namely, that which holds 3 measures, and use always 1 measure of respirable air; and in trying common air use the same vessel A as in the first method; but for dephlogisticated air I use one that holds  $3\frac{3}{4}$  measures. In trying the experiment, I first weigh the bottle M without any air in it, and then weigh it again with the respirable air in it, which gives the quantity of respirable air used. I next put the nitrous air into the vessel A, and weigh that and the bottle M together, and then having mixed the airs, weigh them again, which gives the diminution.

From what has been just said, it appears, that in this method of proceeding I use a less quantity of nitrous air in trying the same kind of respirable air than in the former; the reason of which is, that the same quantity of nitrous air goes further in phlogisticating a given quantity of respirable air in this than in the former method. In both these methods, I express the test of the air by the diminution which they suffer in mixing; for example, if the diminution on mixing them be 2 measures and  $\frac{3.53}{1000}$ , I call its test 2.353, and so on.

There is a considerable difference in the diminution, according to the nature of the water, which is a very great inconvenience, and seems to be the chief cause of uncertainty in trying the purity of air; but it is by no means peculiar to this method, as I have found as great a difference in Fontana's method, according as I have filled the tube with different waters. But it shows plainly, how little all the experiments which have hitherto been made, for determining the variations in the purity of the atmosphere, can be relied on, as I do not know that any one before has been attentive to the nature of the water he has used, and the difference proceeding from the difference of waters is much greater than any I have yet found in the purity of air. The best way I know of obviating this inconvenience, is to be careful always to use the same kind of water: that which I always use is distilled, as being most certain to be always alike. I should have used rain water, as being easier procured, if it had not been that this water is sometimes apt to froth, which I have never known distilled water do.

Mr. C. adds several other cautionary observations, which occurred in his practice in making experiments with his eudiometer; and then proceeds as follows.

During the last half of the year 1781, I tried the air of near 60 different days, in order to find whether it was sensibly more phlogisticated at one time



than another; but found no difference that I could be sure of, though the wind and weather on those days were very various; some of them being very clear and fair, others very wet, and others very foggy. My way was, to fill bottles with glass stoppers every now and then with air from without doors, and preserve them stopped and inverted into water, till I had got 7 or 8, and then take their test; and whenever I observed their test, I filled 2 bottles, one of which was tried that day, and the other was kept till the next time of trying, in order to see how nearly the test of the same air, tried on different days, would agree. The experiment was always made with distilled water, and care was always taken to observe the diminution which nitrous air suffered by being shaken in the water. The heat of the water in the tub also was commonly set down. Most of the bottles were tried only in the first method; but some of them were also tried by the second, and by the method of Fontana.

I would by all means recommend it to those who desire to compare the air of different places and seasons, to fill bottles with the air of those places, and to try them at the same time and place, rather than to try them at the time they were filled, as all the errors to which this experiment is liable, as well those which proceed from small differences in the manner of trying the experiment, as those which proceed from a difference in the nature of the water and nitrous air, will commonly be much less when the different parcels of air are tried at the same time and place, than at different ones; provided only, that air can be kept in this manner a sufficient time without being injured, which I believe it may, if the bottles are pretty large, and care be taken that they, as well as the water used in filling them with air, are perfectly clean. I have tried air kept in the above-mentioned manner for upwards of three quarters of a year in bottles holding about a pint, which I have no reason to think was at all injured; but then I have tried some kept not more than one third part of that time which seemed to have been a little impaired, though I do not know what it could be owing to, unless it was that the bottles were smaller, namely, holding less than  $\frac{1}{4}$  of a pint, and that in all of them, except 2, which were smaller than the rest, the stopper which, however, fitted in very tight, was tied down by a piece of bladder.

I made some experiments also to try whether the air was sensibly more dephlogisticated at one time of the day than another, but could not find any difference. I also made several trials with a view to examine whether there was any difference between the air of London and the country, by filling bottles with air on the same day, and nearly at the same hour, at Marlborough-street and at Kensington. The result was, that sometimes the air of London appeared rather the purer, and sometimes that of Kensington; but the difference was never more than might proceed from the error of the experiment; and by taking a mean of all, there did not appear to be any difference between them. The number of



days compared was 20, and a great part of them taken in winter, when there are a greater number of fires, and on days when there was very little wind to blow away the smoke.

It is very much to be wished, that those gentlemen who make experiments on factitious airs, and have occasion to ascertain their purity by the nitrous test, would reduce their observations to one common scale; as the different instruments employed for that purpose differ so much, that at present it is almost impossible to compare the observations of one person with those of another. This may be done, as there seems to be so very little difference in the purity of common air at different times and places, by assuming common air and perfectly phlogisticated air as fixed points. Thus, if the test of any air be found to be the same as that of a mixture of equal parts of common and phlogisticated air, I would say, that it was half as good as common air: or, for shortness, I would say, that its standard was  $\frac{1}{2}$ : and, in general, if its test was the same as that of a mixture of one part of common air and  $x$  of phlogisticated air, I would say, that its standard was  $\frac{1}{1+x}$ . In like manner, if one part of this air would bear being mixed with  $x$  of phlogisticated air, in order to make its test the same as that of common air, I would say, that it was  $1+x$  times as good as common air, or that its standard was  $1+x$ ; consequently, if common air, as Mr. Scheele and Lavoisier suppose, consist of a mixture of dephlogisticated and phlogisticated air, the standard of any air is in proportion to the quantity of pure dephlogisticated air in it. In order to find what test on the eudiometer answers to different standards below that of common air, all that is wanted is, to mix common and perfectly phlogisticated air in different proportions, and to take the test of those mixtures; but in standards above that of common air, it is necessary to procure some good dephlogisticated air, and to find its standard by trying what proportion of phlogisticated air it must be mixed with, in order to have the same test as common air, and then to mix this dephlogisticated air with different proportions of phlogisticated air, and find the test of those mixtures.\*

Perfectly phlogisticated air may be conveniently procured by putting some solution of liver of sulphur into a bottle of air well stopped, and shaking it frequently till the air is no longer diminished, which, unless it is shaken very frequently, will take up some days. Care must be taken however, to loosen the

\* The rule for computing the standard of any mixture of dephlogisticated and phlogisticated air is as follows. Suppose that the test of a mixture of  $D$  parts of dephlogisticated air with  $P$  of phlogisticated air, is the same as that of common air; then is the standard of the dephlogisticated air  $\frac{D+P}{D}$ . Let now  $d$  parts of this dephlogisticated air be mixed with  $\phi$  parts of phlogisticated air, the

standard of the mixture will be  $\frac{D+P}{D} \times \frac{d}{d+\phi}$ .—Orig.



stopper now and then, so as to let in air to supply the place of the diminished air. In order to know when the air is as much diminished as it can be, the best way is, when the air is supposed to be nearly phlogisticated, to place the bottle with its mouth under water, still keeping it stopped, and to loosen the stopper now and then, while under water, so as to let in water to supply the place of the diminished air, by which means the alteration of weight of the bottle shows whether the air is diminished or not. If the solution of liver of sulphur be made by boiling together fixed alkali, lime, and flowers of sulphur, which is the most convenient way of procuring it, the air phlogisticated by it will be perfectly free from fixed air: whether it will be so if the liver of sulphur be made without lime, I am not sure. A still more convenient way however, of procuring phlogisticated air, is by a mixture of iron filings and sulphur; and, as far as I can perceive, the air procured this way is as completely phlogisticated as that prepared by liver of sulphur.

Where the impurities mixed with the air have any considerable smell, our sense of smelling may be able to discover them, though the quantity is vastly too small to phlogisticate the air in such a degree as to be perceived by the nitrous test, even though those impurities impart their phlogiston to the air very freely. For instance, the great and instantaneous power of nitrous air in phlogisticating common air is well known; and yet 10 ounce measures of nitrous air, mixed with the air of a room upwards of 12 feet each way, is sufficient to communicate a strong smell to it, though its effects in phlogisticating the air must be utterly insensible to the nicest eudiometer; for that quantity of nitrous air is not more than the 140000th part of the air of the room, and therefore can hardly alter its test by more than  $\frac{3}{140000}$  or  $\frac{1}{47000}$  part. Liver of sulphur also phlogisticates the air very freely, and yet the air of a room will acquire a very strong smell from a quantity of it vastly too small to phlogisticate it in any sensible degree. In like manner it is certain, that putrifying animal and vegetable substances, paint mixed with oil, and flowers, have a great tendency to phlogisticate the air; and yet it has been found, that the air of a privy, of a fresh painted room, and of a room in which such a number of flowers were kept as to be very disagreeable to many persons, was not sensibly more phlogisticated than common air. There is no reason to suppose from these instances, either that these substances have not more tendency to phlogisticate the air, or that nitrous air is not a true test of its phlogistication, as both these points have been sufficiently proved by experiment; it only shows, that our sense of smelling can, in many cases, perceive infinitely smaller alterations in the purity of the air than can be perceived by the nitrous test, and that in most rooms the air is so frequently changed, that a considerable quantity of phlogisticating materials may be kept in them without sensibly impairing the air. But it must be observed, that



the nitrous test shows the degree of phlogistication of air, and that only; whereas our sense of smelling cannot be considered as any test of its phlogistication, as there are many ways of phlogisticating air without imparting much smell to it; and I believe there are many strong smelling substances which do not sensibly phlogisticate it.

*IX. Experiments on the Resistance of the Air. By Richard Lovell Edgworth, Esq., F. R. S. p. 136.*

Many experiments have been tried to ascertain the force and velocity of the wind, with a view to the construction and management of different engines, and more particularly for the purposes of navigation. Several machines, which have been employed in these inquiries, are described in the Transactions of the R. S., and in the memoirs of foreign academies; but the most accurate that I have seen was invented by the late Sir Charles Knowles; and from a number of experiments made with it, he had constructed tables, showing at one view the force of the wind on each sail of a ship at every degree of velocity, from 1 to 90 miles an hour. But these calculations, and many more of a similar nature, that are to be met with in Belidor's *Architecture Hydraulique*, and other books, are founded on a supposition that the effect of the wind is directly as the surface on which it acts. If, for instance, its force be estimated as 1 on one square yard, its force on 2 square yards should be estimated as 2, on 3 square yards as 3, &c.; but in fact this proportion is not to be depended on, nor must the resistance of surfaces be estimated merely by their extent; as several other circumstances must be taken into consideration.

No figures can resemble each other more than a parallelogram and a square having the same superficial contents, as they are both bounded by 4 straight lines meeting at right angles, yet they oppose different degrees of resistance to the air. If 2 similar cards, for instance, be placed opposite the wind, one on its end, and the other on its side, and both inclined to the same angle, the wind will have the greater effect on the card that is placed edgewise. To determine the difference of resistance between these two surfaces, and to ascertain the effect of other figures moving through the air, I tried the following experiments. The first 2 are to be found in Mr. Robins's *Treatise on Gunnery*; but I thought it proper to repeat them, that they might be more readily compared with others made with the same apparatus, especially as Mr. Robins made use of a machine constructed on a smaller scale than mine, and turning on friction wheels, which are not proper for machines of this nature, nor indeed for any purpose, where a uniform motion is required.

Having fastened a strong joist of wood from one side of a large room to the other, so as to form a kind of bridge at some distance from the floor, I erected



a perpendicular shaft or roller, which turned freely in brass sockets, fixed into the floor and bridge, on pivots of hardened steel  $\frac{1}{8}$  of an inch in diameter. On each side of this roller was extended an arm of deal, feather-edged, and supported by stays of the same material, feathered in the same manner, to oppose as little surface as possible to the air when in motion. Round the upper part of this roller was wound a string of cat-gut, which, passing over pulleys properly disposed, was fastened to a scale that descended into the well of an adjoining staircase. The extremity of these arms described a space of more than 40 feet in every revolution, the weight descending in the same time only 6 inches. The time in all the following experiments was the same; and, as each revolution was performed in 4 seconds, the velocity of the end of the arm on which the surface was fixed, was at the rate of about 7 miles an hour.\*

The first figure that I tried was a parallelogram of tin, 9 inches long, and 4 inches wide. Its longer side was placed parallel to the floor, at the extremity of one of the arms. Its shorter sides were inclined to an angle of 45 degrees from the perpendicular, and in this situation it was carried round with its surface against the air. After † suffering it to revolve till its motion was become uniform, I put as much weight into the scale as moved it with a velocity of 5 turns in 20 seconds. I then changed the situation of the parallelogram, placing its shorter sides parallel to the floor, and inclined to the same angle as before. I now found, that more weight was required to produce the same velocity, though the quantity of surface was the same as in the preceding experiment. The weight necessary to put the machine alone in motion, with the velocity above-mentioned, was  $2\frac{1}{2}$  lb. When it carried the parallelogram with one of its shorter ‡ sides downwards, it required  $4\frac{1}{2}$  lb. additional weight; and when the parallelogram was reversed, another half pound was barely sufficient to give it the same velocity. The difference therefore, occasioned by placing the same parallelogram with its longer or shorter sides inclined from the direction of its motion, was equal to  $\frac{1}{10}$  of the greatest resistance.

It is to be observed, that in these two experiments the mean velocity of the plane was not the same, as its extremity extended farther from the centre of the machine in one than in the other. This is strictly true; but the size of the parallelogram bore so small a proportion to the length of the radius to which it was fastened, that the error arising from this circumstance is scarcely perceptible,

\* This contrivance of machinery is manifestly of nearly the same nature as that invented by Mr. Robins for the same use, and described in the 1st volume of his works, p. 200, new edition, in the year 1805.

† Instead of saying after suffering it to revolve he put the weight into the scale, this ingenious gentleman must have meant to say, that after putting in the weight he suffered the machine to revolve.

‡ Query, longer?



and the advantage being in favour of that which required the least weight, I did not think it necessary to bring it into account.

Having formed a general idea of the reason of the difference in these experiments, it occurred to me, that there would be a greater disproportion between the resistance of some other figures, which Mr. Robins had not tried; and having put a rhomboid, in the form of a lozenge, 9 inches long, and 4 broad, in the place of the parallelogram; the difference was increased from  $\frac{1}{16}$  to  $\frac{1}{7}$  of the weight employed to give them the required velocity.

Pursuing the same reasoning that led me to the last experiment, it occurred, that even against figures of exactly the same shape, the resistance of the air, when the dimensions of the figures were enlarged, would not be increased in the same proportion as the size of the planes, but in a much higher ratio; and that, by bending the planes as a sail, the resistance would be still further increased, though the section of air, that would be intercepted by the planes, must by these means be considerably lessened. The result far surpassed my expectations. A square of tin, containing 16 square inches, placed perpendicularly, was resisted as  $2\frac{1}{4}$ . A square, containing 64 inches, or 4 times the former quantity, instead of meeting with a resistance as 10, or 4 times the former resistance, required no less than 14 lb. to give it the same velocity.

I now placed the parallelogram of 9 inches long on the arms of the machine, with its shorter sides parallel to the horizon, bending it to such an arch that its chord measured 8 inches, and inclining it to an angle of 45 degrees. And though the section of air that it intercepted was by these means diminished  $\frac{1}{9}$ , yet the resistance was increased from 5 to  $5\frac{1}{2}$ . And when the parallelogram was bent yet more, and its chord contracted almost to 7 inches, the resistance was increased to  $5\frac{3}{4}$ . I mention these numbers in gross to avoid confusion; but in the table at the end of this paper, the measures and weights are set down exactly.

Dr. Hook, whose name must be respected by every experimental philosopher, was aware, that though he thought he could demonstrate that flat sails were preferable to such as were curved and hollowed by the wind, yet until proper experiments had been tried, nothing could be positively determined. He says, somewhere in his posthumous works, "That he was surprised at the obstinacy of seamen, in continuing, after what appeared the clearest demonstration to the contrary, to prefer bellying or bunting sails to such as were hauled taught; but that he would, at some future time, add the test of experiment to mathematical investigation." He reasoned on a supposition, that the air in motion followed the same laws as light; and that it was reflected from surfaces with the angle of reflection equal to the angle of incidence, which is not the case, as it never makes an angle with the plane; but is always reflected in curves. Mons. Parent,



and other mathematicians, have fallen into the same mistake. No demonstration of this sort was more commonly known or received among practical mechanics, than that the best angle for the sails of a wind-mill, at the beginning of their motion, was an angle of 45 degrees; and that the maximum of an under-shot water-wheel was when it moved with  $\frac{1}{3}$  of the velocity of the water: but Mr. Smeaton, in an excellent paper in the Phil. Trans. has refuted this opinion by the clearest experiments.

I had intended to diversify these experiments, and to extend them to a more interesting subject of inquiry, to determine the best shape of sails, and the angle to which they should be set, to obtain the greatest progressive effect with the least leeway; but, as a more complicated apparatus than I could at present procure is necessary for this purpose, I determined to offer the slight progress I have made, in hopes that some gentleman, more conversant and more interested than myself in these inquiries, may pursue them with success and advantage to the public. I shall only remark, that the general cause of the different resistance of the air on surfaces of different shapes, is the stagnation of that fluid near the middle of the plane on which it strikes. The shape and size of the portion thus stagnated, differs from the shape and angle of the plane. The elasticity of the air permits the parts in motion to compress those which are first stopped or retarded by the plane, and forms, as it were, a new surface of a different shape, for the reception of those particles which succeed. With the assistance of a good solar microscope the curves of the air striking against different surfaces may be delineated, and when the general facts are once clearly ascertained, mathematicians will have an ample field for curious and useful speculation.

TABLE.\*

	Turns.	Time.	Weight.	
Machine alone .....	5	4	2	8
With a parallelogram of 9 inches long and 4 broad, one of its longer sides parallel to the horizon and the parallelogram inclined to an angle of 45°,	5	4	7	0
Ditto, with one of its shorter sides downwards .....	5	4	7	9
With a lozenge 9 inches long and 4 broad, with its longer side parallel to the horizon .....	5	4	5	8
Ditto reversed .....	5	4	6	0
With a square piece of tin, four inches each side .....	5	4	5	0
Ditto, 8 inches square .....	5	4	16	6
With the former parallelogram, placed with one of its shortest sides downwards, inclined to an angle of 45°, and bent into an arch whose chord was 8 inches long .....	5	4	8	0
Ditto bent to an arch, the chord of which was 7 $\frac{1}{4}$ inches .....	5	4	8	5

\* See a great many more experiments of this nature in Dr. Hutton's Dictionary, vol. 1, pp. 364, 365.



*X. An Answer to the Objections stated by M. de la Lande, in the Memoirs of the French Academy for 1776, against the Solar Spots being Excavations in the Luminous Matter of the Sun, with a short Examination of the Views entertained by him on that Subject. By Alex. Wilson, M. D. Prof. of Practical Astron. Glasgow. p. 144.*

In the first part of his paper, published in the Philos. Trans. for 1774, Dr. W. explained how, from the lucky accident of seeing the great solar spot of Nov. 1769, in a certain critical situation on the disk, its real nature was obtruded on his thoughts by a train of appearances the most obvious and unequivocal. It may there also be seen how, from phenomena perfectly similar in spots of the usual size, he was led to a general conclusion, and to believe that all spots, small as well as great, which consist of a dark nucleus, and surrounding umbra, are excavations in the luminous matter of the sun. Having however lately seen, in the Memoirs of the French Academy for 1776, published in 1779, that his paper on the Solar Spots has come under the notice of a member of that illustrious body, M. de la Lande, Dr. W. here intends very freely to offer what arguments occur to him in favour of the solar spots being such as he had described. First of all, it has been urged, as an objection of great weight, that the absence of the umbra on one side, when spots are near the limb, as so fully explained in Dr. W.'s paper, is not constant. As to the fact, it may be there seen, that he was sufficiently aware of it, having stated 3 cases from his own observations, when he did not perceive this change to take place. The Rev. Mr. Wollaston is the only person who, in the Philos. Trans. has bestowed any remarks on his publication; and though he with great candour acknowledges, that generally the umbra changes in the manner Dr. W. had determined, yet he expresses a difficulty as to the Dr.'s conclusions, on account of this circumstance not obtaining universally.

Under similar expressions, says Dr. W. M. de la Lande produces from his own observations, which appear to have been long continued, only 3 cases of the same kind, and from the ancient observations of Mess. Cassini and De La Hire, 4 more; which Dr. W. objects against as not sufficiently described or insisted on by these astronomers. But even admitting this anomaly to be much more frequent than can be contended for, still such cases, Dr. W. says, can only be brought as so many exceptions from a certain general law, or uniformity of appearance, from which the condition of by far the greater number of spots is most undeniably deduced. The utmost therefore that can hence be alleged is, that some few spots differ from all the rest, or from the multitude, and are not like these excavations in the sun. Such cases or exceptions will not surely warrant the conclusion, that no spot can be an excavation. This would be to reverse all the rules of a just induction, by opposing to an irrefragable general argument the force of one extremely limited and feeble.



It comes therefore to be inquired, how far spots which, when near the middle of the disk, appear equal and similar in all things, may yet differ from one another considered as excavations, or as possessing the third dimension of depth, and how far the peculiar circumstances by which they may disagree, can contribute to make some resist this change of the umbra, when near the limb, much more than others. In order to this, Dr. W. supposes two spots which occupy a space on the sun corresponding to equal arches, but of different depths, and inclinations of their sloping sides, and different distances from the sun's limb; and he shows geometrically how, according as the angle of inclination is less, the respective spot will go nearer to the limb than the other, before the side of the umbra vanishes. But those very exceptions to the general phenomena which we are at present examining are of this kind, and may perhaps proceed wholly from the shallowness and the very gradual shelving of some few spots which break out in certain tracts of the sun's body over which the luminous matter lies very thinly mantled. If therefore, on such principles it can be shown, he says, that spots, similar to the rest, may sometimes go to the limb without the one umbra contracting sensibly more than the other, the objection at present considered will be entirely removed, and it will be allowable to conclude, that even these few spots are excavations like all the rest, though shallower, as it would be quite unphilosophical to multiply distinctions concerning their nature, where there is found no necessity for so doing.

	Farthest umbra supposed to vanish when distant from the limb.	Depth of nucleus in English miles and seconds.	Apparent breadth of nearest umbra.
I. ....	1' 0" .....	4.54" .....	2118 ..... 8.58
II. ....	0 30 .....	3.09 .....	1412 ..... 6.02
III. ....	0 15 .....	2.09 .....	975 ..... 4.13
IV. ....	0 8 .....	1.44 .....	672 ..... 2.87

Now, because in every aspect of a spot, the real breadth of either the farthest or nearest umbra must be to the projected or apparent breadth, as radius to the sine of the angle which this respective plane makes with the visual ray, it follows, that at any time before the spot comes so near the limb as is expressed in the above examples, the apparent breadth of the nearest and farthest umbra cannot differ so much as by the quantity there set down for the apparent breadth of the nearest, when the other is supposed to vanish. Regarding, therefore, the farthest and nearest umbra of the spot in case 4, as two neighbouring visible objects which turn narrower by degrees as the spot goes toward the limb, we should undoubtedly judge that they contract as to sense alike, since so long as the farthest could be perceived, the other cannot appear to exceed it by a quantity that we could distinguish; and by the time the plane of the former coincides with the



visual ray, the extreme nearness to the limb would prevent our forming any certain judgment of either.

From this last example, therefore, it appears manifest that a spot, answering to the description and conditions there mentioned, or one a little more shallow, would approach the limb, and finally go off the disk, without that peculiar change of the umbra on one side, which is so obvious on common occasions, notwithstanding it were an excavation, whose nucleus or bottom is so many miles below the level of the surface.

In 4 cases, stated by Dr. W. the distance of the remotest part of the nucleus from the sun's limb when the visual ray coming from it is just interrupted by the lip of the excavation, or, in other words, the distance of the nucleus from the limb when it is totally hid was also computed. These distances are as follow :

Case 1. ....	16".93	Case 3. ....	4".70
2. ....	8 .90	4. ....	2 .70

and it is remarkable from the last two, how very near the limb a shallow spot, of not more than 40" in diameter, may come before the nucleus wholly disappears.

Perhaps it may be urged, that very shallow spots ought always to be known from the rest, and to discover themselves, by a surrounding umbra very narrow compared to the extent of the nucleus; but we know far too little of the qualities of the luminous matter, and of the proximate causes of the spots, to say any thing at all on a point of this kind. The breadth of the umbra is, as assumed in the computations, commonly about equal to that of the nucleus, though sometimes it varies more or less; but how far these relative dimensions indicate depth or shallowness must be expounded only by observation, and not by any vague or imperfect notions of the nature and constitution of the sun. The mention of a pit or hollow or excavation several thousands of miles deep, reaching to that extent down through a luminous matter to darker regions, is ready to strike the imagination in a manner unfavourable to a just conception of the nature of the solar spots as now described. On first thoughts it may look strange, how the sides and bottom of such vast abysses can remain so very long in sight, while by the sun's rotation they are made to present themselves more and more obliquely to our view. But when it is considered, how extremely inconsiderable their greatest depth is, compared to the diameter of the sun, and how very wide and shelving they are, all difficulties of this sort will be entirely removed. Dr. W. here objects to an unfair or incorrect sketch of a figure to represent a case of a spot by M. de la Lande, and adds, that as his design, on the present occasion, is to write and to explain matters in a popular way, rather than to astronomers, it will be proper to assist the conceptions of those who are but little versed in mathematical principles by such diagrams as will show things in their just pro-



portions. To this end therefore he exhibits a figure drawn in just proportional dimensions; by which we may see how very small a portion of the sun's body is made up of the luminous matter when supposed every where 3967 English miles deep.

For his own amusement too, Dr. W. says he had pursued this subject further in the way of ocular proof, by a model of the sun and of the spots on his body according to their proper dimensions. This he put into a convenient wooden frame, and viewed it afar off when set upon a stand, while the globe was turned slowly round, and subtended an angle at the telescope equal to the apparent diameter of the sun. By an object-glass micrometer he then took the distances from the limb when the farthest umbra of different spots vanished, as also the distances of the nuclei just when disappearing. The apparent subtense of the umbra next the limb was also measured in this way, with the visible extension of some large spots within the disk; when the extreme limits of the nearest umbra coincided with the limb. In all these experiments, he says, the effect was very striking, and the phenomena remarkably consonant to calculation, and to what he had often seen on the real sun in the heavens.

But to proceed; what has now been insisted on at so much length concerning the shallowness and the more gradual shelving of some few spots, will also apply to another objection, which M. de la Lande views in a strong light. Here we find quoted the great spot in 1719, seen by M. Cassini; and, for the 2d time, that of June 3, 1703, seen by M. de la Hire; both which, on their arrival at the limb, are said to have made an indentation or dark notch in the disk; and this phenomenon is mentioned as absolutely incompatible with spots being below the surface. It is most true, says Dr. W. that if we look for any thing like this, when the plane which coincides with the external boundary of the spot passes through the eye, the way that M. de la Lande considers the matter, it must be very large indeed before the disk could be perceived deficient by any dark segment. But may not a spot, even no larger than M. Cassini's, considered as an excavation, make, in a manner very different from this, something like a notch; for, by the way, this phenomenon is not in the Mem. Acad. nor any where else, that I know of, described with any sort of precision. After showing how this may happen, Dr. W. adds, I do not imagine therefore that the phenomena of notches in the disk, so inconsiderable and dubious as these seem to be, are by any means a proof of projecting nuclei, or that they are not reconcileable to spots being depressions in the sun. A large shallow excavation, with the sloping sides or umbra darker than common, may be more or less perceptible at the limb.

In reasoning concerning the nature of the spots, and particularly about their 3d dimension or depth, the only arguments which are admissible, and which



carry with them a perfect conviction, are those grounded on the principles of optical projection. If, for example, by far the greater number of them be excavations, some thousands of miles deep, certain changes of the umbra would be observable when near the limb. Were they very shallow, or quite superficial, both sides of the umbra would as to sense contract alike in their progress toward the limb. Again, if the nucleus extended much above the common level while the surrounding umbra was superficial, we should behold manifest indications of this by such an opaque body, when seen very obliquely, being projected across the farthest side of the umbra, and by hiding the whole or part of it before the time it would otherwise disappear. According to this or that condition of the spot, such things must infallibly obtain by the known laws of vision; and hence arguments resting on such principles may be denominated optical ones. On the other hand, when spots are contemplated near the middle of the disk, a great variety of changes are observed in them, which depend not upon position, but on certain physical causes producing real alterations in their form and dimensions. It is plain, that arguments derived from the consideration of such changes, and which, on that account, may be called physical arguments, can assist us but little in investigating their 3d dimensions; and, from the nature of the thing, must be liable to great uncertainty.

Again: but whatever be their defects, no doubts ought to arise, on such grounds, of the spots being themselves what direct observation declares them, namely, excavations in the sun. Whether their first production and subsequent numberless changes depend on the eructation of elastic vapour from below, or on eddies or whirlpools commencing at the surface, or on the dissolving of the luminous matter in the solar atmosphere, as clouds are melted and again given out by our air; or, if the reader pleases, on the annihilation and reproduction of parts of this resplendent covering; is left for theory to guess at. Though, however, many difficulties should occur in an attempt of this kind, it would certainly be unreasonable on that account to call in question the 3d dimension of the spots, as previously determined by arguments which are liable to no fallacy, and which are unconnected with every kind of theoretical reasoning.

As I conceive it, however, of some importance to have the distinction above treated of perfectly understood in future, I now purposely avoid entering on any theoretical ground whatever. My wish therefore is, that the author of the *Memoire* may acquit me of every thing not perfectly respectful, though I do not follow him through that train of objection founded on vague and incompetent physical arguments, which is to be met with in p. 511, &c. By further considering the particulars hinted at in p. 21 and 29, of my paper, several difficulties, perhaps, may be removed; but we forbear any illustration of this kind, chiefly to evince how little we concern ourselves whether those who do not like the princi-



ples assumed in part 2, or the conclusions drawn from them; in short, those who will call that part a theory, and who think it a bad one, may, if they please, mend it, or contrive a new and a better one of their own. But so long as they cannot, by irrefragable optical arguments, set aside the induction laid down in part 1, we must demand of them, so to fabricate their theories as to account for the various circumstances of the spots, considered as things which possess 3 dimensions, viz. length, breadth, and depth, or, in other words, as excavations in the luminous matter of the sun. This fact is the only one, says Dr. W., I am solicitous to maintain, or to contend for; and for a very good reason, because I consider it as actually demonstrated by competent observations. As such, to indulge for a moment in a figure, it would be a pity not to rescue it from being drawn into the eddy of some treacherous theory, the nature of all which is to sweep into their vortex, and finally to precipitate to the bottom, every thing which obstructs their impetuous career. Sir Isaac Newton, perceiving too well this proneness to system, has laid down his 4th rule of philosophising, that arguments of induction may not be evaded by hypothesis. It will become us therefore, in all things, and in the present subject in particular, to have respect to so excellent a precept. In speaking hereafter of the solar spots, let us separate what things claim to be heard as matters of fact from what rest on the sandy foundations of mere theory, and no longer confound them together.

It remains now only to make a few strictures on M. de la Lande's theory of the solar spots, humbly submitting them to the consideration of the reader. The import of it is, "that the spots as phenomena arise from dark bodies like rocks, which by an alternate flux and reflux of the liquid igneous matter of the sun sometimes raise their heads above the general surface. That part of the opaque rock, which at any time thus stands above, gives the appearance of the nucleus, while those parts, which in each lie only a little under the igneous matter, appear to us as the surrounding umbra."

In the first place it may be remarked, "that the whole proceeds on mere supposition." This indeed the author himself very readily acknowledges. Though, therefore, it could not be disputed by arguments derived from observation, yet conjecture of any kind, if equally plausible, might fitly be employed to set aside its credit. Now all observers, and all good representations of the spots, bear testimony to the exterior boundary of the umbra being always well defined, and to the umbra itself being less and less shady the nearer it comes to the nucleus. Now it may be asked, how this could possibly be, according to M. de la Lande's theory? If the umbra be occasioned by our seeing parts of the opaque rock, which lie a little under the surface of the igneous matter, should it not always be darkest next the nucleus, and from the nucleus outward should it not wax more and more bright, and at last lose itself in the general lustre of the sun's surface, and



not terminate all at once at the darkest shade, as in fact it does? These few incongruities, which meet us as it were in the very threshold of the theory, are so very palpable, that of themselves they raise unsurmountable doubts. For, generally speaking, the umbra immediately contiguous to the nucleus, instead of being very dark, as it ought to be, from our seeing the immersed parts of the opaque rock through a thin stratum of the igneous matter, is, on the contrary, very nearly of the same splendour as the external surface.

Concerning the nucleus, or that part of the opaque rock which stands above the surface of the sun, M. de la Lande produces no optical arguments in support of this 3d dimension or height. Neither does he say any thing particular as to the degree of elevation above the surface. But from what has been already hinted in the course of this paper, it appears, that if this were any thing sensible, it ought to be discovered by phenomena very opposite to those which we have found to be so general. Again, a flux and reflux of the igneous matter, so considerable as sometimes to produce a great number of spots all over the middle zone, might affect the apparent diameter of the sun, making that which passes through his equator less than the polar one, by the retreat of the igneous matter towards those regions where no spots ever appear. But as a difference of this kind of nearly a thousandth part of the whole would be perceivable, as we learn from M. de la Lande's own observations, compared with those of Mr. Short, in *Histoire Acad.* 1760, p. 123, it would seem, that the theory had also this difficulty to combat. Further, when among spots very near each other, some are observed to be increasing, while others are diminishing, how is it possible that this can be the effect of such a supposed flux and reflux? This last inconsistency is mentioned by the author himself, who endeavours to avoid it, by making a new demand on the general fund of hypothesis, deriving from thence such qualities of the igneous matter as the case seems to require; and such must be the method of proceeding in all systems merely theoretical.

But it is unnecessary to pursue at more length illusive speculations of this kind, especially as we lie under a conviction, founded on fact, of the theory being utterly erroneous. It hardly differs in any respect from that proposed by M. de la Hire, and a little amended by the writer of the *Histoire de L'Acad.* for 1707, p. 111.

The writer of the *Histoire de L'Acad.* for 1719, p. 76, after reviewing the merits of this theory, and comparing it with several phenomena of the spots which had been observed for the 4 preceeding years, pronounces it unsatisfactory: And indeed views, much of the same kind, were even entertained by some so long ago as the days of Scheiner, as we find mentioned by that indefatigable author in his *Rosa Ursina*, p. 746.



*XI. Of the Earthquakes which happened in Italy, from February to May, 1783.**By Sir Wm. Hamilton, F. R. S. &c. p. 169.*

Sir Wm. H. here gives, he says, some little idea of the infinite damage done, and of the various phenomena exhibited, by the earthquakes, (which began the 5th of Feb. 1783, and continued to be felt to the day he was writing, viz. May 23) in the two Calabrias, at Messina, and in the parts of Sicily nearest to the continent. From the most authentic reports, and accounts received at the offices of his Sicilian Majesty's secretary of state, he gathered in general, that the part of Calabria, which has been most affected by this heavy calamity, is that which is comprehended between the 38th and 39th degree of latitude, being the foot or extreme point of the continent; that the greatest force of the earthquakes seemed to have exerted itself from the foot of those mountains of the Apennines called the Monte Deio, Monte Sacro, and Monte Caulone, extending westward to the Tyrrene sea; that the towns, villages and farm-houses, nearest these mountains, situated either on hills or in the plain, were totally ruined by the first shock of the 5th of February about noon; and that the greatest mortality was there; that in proportion as the towns and villages were at a greater distance from this centre, the damage they received was less considerable; but that even those more distant towns had been greatly damaged by the subsequent shocks of the earthquake, and especially by those of the 7th, the 26th, and 28th of February, and that of the 1st of March; that from the first shock, the 5th of February, the earth continued to be in a continual tremour, more or less; and that the shocks were more sensibly felt at times in some parts of the afflicted provinces than in others; that the motion of the earth had been various, and, according to the Italian denomination, vorticoso, orizzontale, and oscillatorio, that is, either whirling like a vortex, horizontal, or by pulsations, or beatings from the bottom upward; that this variety of motion had increased the apprehensions of the unfortunate inhabitants of those parts, who expected every moment that the earth would open under their feet, and swallow them up; that the rains had been continual and violent, often accompanied with lightning and irregular and furious gusts of wind; that from all these causes the face of the earth of that part of Calabria above-mentioned was entirely altered, particularly on the westward side of the mountains above named; that many openings and cracks had been made in those parts; that some hills had been lowered, and others quite levelled; that in the plains, deep chasms had been made, by which many roads were rendered impassable; that huge mountains had been split asunder, and parts of them driven to a considerable distance; that deep vallies had been filled up by the mountains, which formed those vallies, having been detached by the violence of the earthquakes, and joined together; that the course of some rivers had been altered; that many springs of water had appeared in places that were perfectly dry before;



and that in other parts, springs that had been constant had totally disappeared; that near Laureano in Calabria Ultra, a singular phenomenon had been produced, that the surface of two whole tenements or tracts of land, with large olive and mulberry-trees on them, situated in a valley perfectly level, had been detached by the earthquake, and transplanted, the trees still remaining in their places, to the distance of about a mile from their first situations; and that from the spot on which they formerly stood hot water had sprung up to a considerable height, mixed with sand of a ferruginous nature; that near this place also some countrymen and shepherds had been swallowed up with their teams of oxen and their flocks of goats and sheep; in short, that beginning from the city of Amantea, situated on the coast of the Tyrrene sea in Calabria Citra, and going along the westward coast to Cape Spartivento in Calabria Ultra, and then up the eastern coast as far as the Cape d'Alice (a part of Calabria Citra on the Ionian sea), there is not a town or village, either on the coast or inland, but what is either totally destroyed, or has suffered more or less, amounting in all to near 400, what are called here *paeses*. A village containing less than 100 inhabitants is not counted as a *paese*.

The greatest mortality fell upon those towns and countries situated in the plain on the western side of the mountains Dejo, Sacro, and Caulone. At Casal Nuovo, the Princess Gerace, and upwards of 4000 of the inhabitants, lost their lives; at Bagnara, the number of dead amounts to 3017; Radicina and Palmi count their loss at about 3000 each; Terranuova about 1400; Seminari still more. The sum total of the mortality in both Calabrias and in Sicily, by the earthquakes alone, according to the returns in the secretary of state's office at Naples, is 32,367; but there was good reason to believe that, including strangers, the number of lives lost must have been considerably greater, 40,000 at least may be allowed, and Sir Wm. believed, without any exaggeration. From the same office it was stated that the inhabitants of Scilla, on the first shock of the earthquake, the 5th of February, had escaped from their houses on the rock, and, following the example of their prince, taken shelter on the sea-shore; but that in the night-time the same shock, which had raised and agitated the sea so violently, and done so much damage on the point of the Faro of Messina, had acted with still greater violence there, for that the wave (which was falsely represented to have been boiling hot, and that many people had been scalded by its rising to a great height) went furiously 3 miles inland, and swept off in its return 2473 of the inhabitants of Scilla, with the prince at their head, who were at that time either on the Scilla strand, or in boats near the shore.

All accounts agreed, that of the number of shocks which have been felt since the beginning of this formidable earthquake, amounting to some hundreds, the most violent, and of the longest duration, were those of the 5th of February at



19 $\frac{1}{2}$  (according to the Italian way of counting the hours); of the 6th of Feb., at 7 hours in the night; of the 27th of Feb., at 11 $\frac{1}{4}$  in the morning; of the 1st of March, at 8 $\frac{1}{2}$  in the night; and that of the 28th of March, at 1 $\frac{1}{2}$  in the night. It was this last shock that affected most the upper part of Calabria Ultra, and the lower part of the Citra, an authentic description of which will be seen hereafter, in a letter received from the Marquis Ippolito, an accurate observer residing at Catanzaro in the Upper Calabria. The first and the last shocks must have been tremendous indeed, and only these two were sensibly felt in the capital, Naples.

The accounts which this government has received from the province of Cosenza, are less melancholy than those from the province of Calabria Ultra. From Cape Suvero to the Cape of Cetraro on the western coast, the inland countries, as well as those on the coast, are said to have suffered more or less in proportion to their proximity to the supposed centre of the earthquakes; and it has been constantly observed, that its greatest violence has been exerted, and still continued to be so, on the western side of the Appennines, precisely the celebrated Sila of the ancient Brutii, and that all those countries situated to the eastward of the Sila had felt the shocks of the earthquake, but without having received any damage from them. In the province of Cosenza there does not appear to be above 100 lives lost. In the last account from the most afflicted part of Calabria Ultra, two singular phenomena are mentioned. At about the distance of three miles from the ruined city of Oppido, there was a hill, the soil of which is a sandy clay, about 500 palms high, and 1300 in circumference at its basis. It was said, that this hill, by the shock of the 5th of February, was carried to the distance of about 4 miles from the spot where it stood, into a plain called the Campo di Bassano. At the same time the hill on which the town of Oppido stood, which extended about 3 miles, divided in two, and as its situation was between two rivers, its ruins filled up the valley, and stopped the course of those rivers, two great lakes are already formed, and are daily increasing, which lakes, if means are not found to drain them, and give the rivers their due course, in a short time must greatly infect the air.

From Sicily the accounts of the most serious nature were, those of the destruction of the greatest part of the noble city of Messina, by the shock of the 5th of February, and of the remaining parts by the subsequent ones;—that the quay in the port had sunk considerably, and was in some places a palm and a half under water;—that the superb building, called the Palazzata, which gave the port a more magnificent appearance than any port in Europe can boast of, had been entirely ruined;—that the Lazaret had been greatly damaged; but that the citadel had suffered little;—that the mother church had fallen; in short, that Messina was half destroyed;—that the tower at the point of the entrance of



the Faro was half destroyed; and that the same wave, that had done such mischief at Scilla, had passed over the point of land at the Faro, and carried off about 24 people. The viceroy of Sicily likewise gave an account of some damage done by the earthquakes, but nothing considerable, at Melazzo, Patti, Terra di Santa Lucia, Castro Reale, and in the island of Lipari.

Such was the intelligence Sir Wm. was possessed of by those reports; but as he was particularly curious on the subject of volcanos, and was persuaded in his own mind, from the earthquakes being confined to one spot, that some great chemical operation of nature, of the volcanic sort, was the real cause of them; in order to clear up many points, and to come at truth, he took the sudden resolution to employ about 20 days in making the tour of such parts of Calabria Ultra and Sicily as had been, and were still, most affected by the earthquakes, and examining with his own eyes the phenomena abovementioned. He accordingly hired for that purpose a Maltese Speronara for himself, and a Neapolitan Felucca for his servants, leaving Naples the 2d of May, he sailed round the coasts of the Calabrias, that had been afflicted with this grievous misfortune; occasionally landing in different parts, and making incursions inland, to learn, by his own eyes and ears, some particulars of such mighty mischiefs: by which means the foregoing general accounts were mostly confirmed, with some slight variations, and many curious particular circumstances. At most places he perceived ruined towns and houses, and that most of the inhabitants were in barracks, which are just such sort of buildings as the booths of our country fairs, though indeed many as he had seen were more like our pig-sties. In several of the parts, from the barracks having been ill-constructed, and many of them situated in a very unwholesome spot, an epidemical disorder had taken place, and carried off many, and was still in fatal force while he was there. And he feared, as the heats should increase, the same misfortune would attend most parts of the unfortunate Calabria, as also the city of Messina. All reports agreed, that every shock of the earthquake seemed to come with a rumbling noise from the westward, beginning usually with the horizontal motion, and ending with the vorticose, which is the motion that has ruined most of the buildings in this province. He found it a general observation also, that before a shock of an earthquake, the clouds seemed to be fixed and motionless; and that immediately after a heavy shower of rain, a shock quickly followed. He spoke with many who were thrown down by the violence of some of the shocks; and several peasants in the country said that the motion of the earth was so violent, that the heads of the largest trees almost touched the ground from side to side; that during a shock, oxen and horses extended their legs wide asunder not to be thrown down, and that they gave evident signs of being sensible of the approach of each shock. Sir Wm. himself observed, that in the parts that have suffered



most by the earthquakes, the braying of an ass, the neighing of a horse, or the cackling of a goose, always drove people out of their barracks, and was the occasion of many pater-nosters and ave-marias being repeated, in expectation of a shock. From Monteleone he descended into the plain, having passed through many towns and villages which had been more or less ruined according to their vicinity to the plain. The town of Mileto, situated in a bottom, he saw was totally destroyed, and not a house standing. Here, as well as in several other parts, he mentions most remarkable instances of animals being able to live long without food, of which there have been many examples during these present earthquakes. At Soriano 2 fattened hogs, that had remained buried under a heap of ruins, were taken out alive the 42d day; they were lean and weak, but soon recovered. It was evident to his observation, that all habitations situated on high grounds, the soil of which is a gritty sand stone, somewhat like a granite, but without the consistence, had suffered less than those situated in the plain, which are universally levelled to the ground. The soil of the plain is a sandy clay, white, red, or brown; but the white prevails most, and is full of marine shells, particularly scollop shells. He was told that, during the earthquake of the 5th of February, from several hollow spots a fountain of water mixed with sand, had been driven to a considerable height. Sir Wm. spoke to a peasant here, who was present, and was covered with the water and sand; but assured him, that it was not hot, as had been represented. Before this appearance, he said, the river was dry; but soon after returned and overflowed its banks. Sir Wm. afterwards found, that the same phenomenon had been constant with respect to all the other rivers in the plain during the formidable shock of the 5th of February. He thinks this phenomenon is easily explained by supposing the first impulse of the earthquake to have come from the bottom upwards, which all the inhabitants of the plains attest to be fact; the surface of the plain suddenly rising, the rivers, which are not deep, would naturally disappear, and the plain, returning with violence to its former level, the rivers must naturally have returned, and overflowed, at the same time that the sudden depression of the boggy grounds would as naturally force out the water that lay hid under their surface. He observed in the other parts, where this sort of phenomenon had been exhibited, that the ground was always low and rushy. It had been remarked at Rosarno, and the same remark had been constantly repeated to him in every ruined town that he visited, that the male dead were generally found under the ruins in the attitude of struggling against the danger; but that the female attitude was usually with hands clasped over their heads, as giving themselves up to despair, unless they had children near them; in which case they were always found clasping the children in their arms, or in some attitude which



indicated their anxious care to protect them : a strong instance of the maternal tenderness of the sex !

Speaking of the 2 tenements, called the Macini and Vaticano, mentioned in the former part of this letter, and which were said to have changed their situation by the earthquake ; Sir Wm. says, the fact is true, and easily to be accounted for. These tenements were situated in a valley surrounded by high grounds, and the surface of the earth, which has been removed, had been probably long undermined by little rivulets, which come from the mountains, and now are in full view on the bare spot the tenements had deserted. These rivulets have a sufficiently rapid course down the valley, to prove its not being a perfect level as was represented. I suppose the earthquake to have opened some depositions of rain-water in the clay-hills which surround the valley, which water, mixed with the loose soil, taking its course suddenly through the undermined surface, lifting it up with the large olive and mulberry trees, and a thatched cottage, floated the entire piece of ground, with all its vegetation, about a mile down the valley, where it now stands, with most of the trees erect. These two tenements may be about a mile long, and half a mile broad. I was shown several deep cracks in this neighbourhood, not one above a foot in breadth ; but which, I was credibly assured, had opened wide during the earthquake, and swallowed up an ox, and near a hundred goats, but no countrymen as was reported. In the valley abovementioned I saw the same sort of hollows in the form of inverted cones, out of which, I was assured, that hot water and sand had been emitted with violence during the earthquakes as at Rosarno ; but I could not find any one who could positively affirm that the water had been really hot, though the reports which government received affirm it. Some of the sand thrown out here with the water has a ferruginous appearance, and seems to have been acted on by fire.

From hence I went through the same delightful country to the town of Polistene. To pass through so rich a country, and not see a single house standing on it, is most melancholy indeed ; wherever a house stood, there you see a heap of ruins, and a poor barrack, with two or three miserable mourning figures sitting at the door, and here and there a maimed man, woman, or child, crawling on crutches. Instead of a town, you see a confused heap of ruins, and round about them numbers of poor huts or barracks, and a larger one to serve as a church, with the church bells hanging on a sort of low gibbet ; every inhabitant with a doleful countenance, and wearing some token of having lost a parent.

I travelled 4 days in the plain, in the midst of such misery as cannot be described. The force of the earthquake was so great there, that all the inhabi-



tants of the towns were buried either alive or dead under the ruins of their houses in an instant. The town of Polistene was large, but ill situated between two rivers, subject to overflow. 2100 out of about 6000 lost their lives here the fatal 5th of February. There was a hunnery at Polistene; being curious to see the nuns that had escaped, I asked the marquis, the baron of this country, to show me their barracks; but it seems only 1 out of 23 had been dug out of her cell alive, and she was fourscore years of age. What causes a confusion in all the accounts of the phenomena produced by this earthquake in the plain, is the not having sufficiently explained the nature of the soil and situation. They tell you, that a town has been thrown a mile from the place where it stood without mentioning a word of a ravine; that woods and corn-fields had been removed in the same manner, when in truth it is only on a large scale, what we see every day on a smaller, when pieces of the sides of hollow ways, having been undermined by rain waters, are detached into the bottom by their own weight. Here, from the great depth of the ravine, and the violent motion of the earth, two huge portions of the earth, on which a great part of the town stood, consisting of some hundreds of houses, were detached into the ravine, and nearly across it, about half a mile from the place where they stood; and what is most extraordinary, several of the inhabitants of those houses, who had taken this singular leap in them, were yet dug out alive, and some unhurt. I spoke to one myself who had taken this extraordinary journey in his house, with his wife and a maid-servant: neither he nor his maid-servant were hurt; but he told me, his wife had been a little hurt, but was now nearly recovered. I happened to ask him, what hurt his wife had received? His answer, though of a very serious nature, was ludicrous enough. He said, she had both her legs and one arm broken, and that she had a fracture on her skull so that the brain was visible. It appears to me, that the Calabresi have more firmness than the Neapolitans; and they really seem to bear their excessive present misfortune with a true philosophic patience. Of 1600 inhabitants at Terra Nuova, only 400 escaped alive. In other parts of the plain situated near the ravine, and near the town of Terra Nuova, I saw many acres of land with trees and corn fields that had been detached into the ravine, and often without having been overturned, so that the trees and crops were growing as well as if they had been planted there. Other such pieces were lying in the bottom, in an inclined situation; and others again that had been quite overturned. In one place, two of these immense pieces of land having been detached opposite to each other, had filled the valley, and stopped the course of the river, the waters of which were forming a great lake: and this is the true state of what the accounts mention of mountains that had walked, and joined together, stopping the course of the river, and forming a lake. At the moment of the earthquake the river disappeared, as at Rosarno, and returning soon after, overflowed the bottom of the ravine about three feet



in depth, so that the poor people that had been thrown with their houses into the ravine from the top of it, and had escaped with broken bones, were now in danger of being drowned. Having walked over the ruins of Oppido, I descended into the ravine, and examined carefully the whole of it. Here I saw indeed the wonderful force of the earthquake, which has produced exactly the same effects as in the ravine of Terra Nuova, but on a scale infinitely greater. The enormous masses of the plain, detached from each side of the ravine, lie sometimes in confused heaps, forming real mountains, and having stopped the course of two rivers, great lakes are already formed, and, if not assisted by nature or art, so as give the rivers their due course, must infallibly be the cause of a general infection in the neighbourhood. Sometimes I met with a detached piece of the surface of the plain, of many acres in extent, with the large oaks and olive-trees, with lupins or corn under them, growing as well, and in as good order at the bottom of the ravine, as their companions, from whom they were separated, do on their native soil in the plain, at least 500 feet higher, and at the distance of about three quarters of a mile. I met with whole vineyards in the same order in the bottom, that had likewise taken the same journey. In another part of the bottom of the ravine there is a mountain composed of the clay soil, and which was probably a piece of the plain detached by an earthquake at some former period; it is about 250 feet high, and about 400 feet diameter at its basis: this mountain, as is well attested, has travelled down the ravine near 4 miles, having been put in motion by the earthquake of the 5th of Feb. The abundance of rain which fell at that time, the great weight of the fresh detached pieces of the plain, which are heaped up at the back of it, the nature of the soil of which it is composed, and particularly its situation on a declivity, accounts well for this phenomenon. The Prince of Cariati showed me 2 girls, one of about 16 years of age, who had remained 11 days without food under the ruins of a house at Oppido: she had a child of 5 or 6 months old in her arms, which died the 4th day. The girl gave a clear account of her sufferings; having light through a small opening, she had kept an exact account of the number of days she had been buried. She did not seem to be in bad health, drank freely, but had yet a difficulty in swallowing any thing solid. The other girl was about 11 years of age; she remained under the ruins 6 days only; but in so very confined and distressful a posture, that one of her hands, pressing against her cheek, had nearly worn a hole through it.

Several fishermen assured me, that during the earthquake of the 5th of Feb. at night, the sand near the sea was hot, and that they saw fire issue from the earth in many parts. This circumstance has been often repeated to me in the plain; and my idea is, that the exhalations which issued during the violent commotions of the earth were full of electrical fire, just as the smoke of volcanos is constantly observed to be during violent eruptions; for I saw no mark; in any part



of my journey, of any volcanic matter having issued from the fissures of the earth; and I am convinced, that the whole damage has been done by exhalations and vapours only. I was assured here, where they have had such a long experience of earthquakes, that all animals and birds are in a greater or less degree much more sensible of an approaching shock of an earthquake than any human being; but that geese, above all, seem to be the soonest and most alarmed at the approach of a shock: if in the water, they quit it immediately, and there is no driving them into the water for some time after. The port of Messina and the town, in its half ruined state, by moon-light was strikingly picturesque. Certain it is, that the force of the earthquake, though very violent, was nothing at Messina and Reggio, to what it was in the plain. I visited the town of Messina the next morning, and found that all the beautiful front of what is called the Palazatta, which extended in very lofty uniform buildings, in the shape of a crescent, had been in some parts totally ruined, in others less; and that there were cracks in the earth of the quay, a part of which had sunk above a foot below the level of the sea. These cracks were probably occasioned by the horizontal motion of the earth in the same manner as the pieces of the plain were detached into the ravines at Oppido and Terra Nuova; for the sea at the edge of the quay is so very deep, that the largest ships can lie along-side; consequently the earth, in its violent commotion wanting support on the side next the sea, began to crack and separate, and as where there is one crack there are generally others less considerable in parallel lines to the first, I suppose the great damage done to the houses nearest the quay has been owing to such cracks under their foundations.

The mortality at Messina does not exceed 700 out of upwards of 30,000, the supposed population of this city at the time of the first earthquake. The generality of the inhabitants are in tents and barracks, which, having been placed in 3 or 4 different quarters, in fields and open spots near the town, but at a great distance from each other, must be very inconvenient for a mercantile town; and unless great care is taken to keep the streets of the barracks, and the barracks themselves, clean, I fear that the unfortunate Messina will be doomed to suffer a fresh calamity from epidemical disorders, during the heat of summer. Indeed, many parts of the plain of Calabria seem to be in the same alarming situation, particularly owing to the lakes, which are forming from the course of rivers having been stopped, some of which were already green, and tending to putrefaction. Out of the cracks on the quay, it is said, that during the earthquakes fire had been seen to issue; but there are no visible signs of it, and I am persuaded it was no more than, as in Calabria, a vapour charged with electrical fire, or a kind of inflammable air. A curious circumstance happened here also, to prove that animals can remain long alive without food. Two mules belonging to the duke of Belviso remained under a heap of ruins, one of them 22, and the



other 23 days : they would not eat for some days, but drank water plentifully, and were quite recovered. There are numberless instances of dogs remaining many days in the same situation ; and a hen, belonging to the British vice-consul at Messina, that had been closely shut up under the ruins of his house, was taken out the 22d day, and was recovered ; it did not eat for some days, but drank freely ; it was emaciated, and showed little signs of life at first. From these instances, and from those related before, of the girls at Oppido, and the hogs at Soriano, and from several others of the same kind, we may conclude, that long fasting is always attended with great thirst, and total loss of appetite. From every inquiry I found that the great shock of the 5th of February was from the bottom upwards, and not like the subsequent ones, which in general have been horizontal and vorticose. A circumstance worth remarking, and which was the same on the whole coast of the part of Calabria that had been most affected by the earthquake, is, that a small fish called cicirelli, resembling what we call in England white-bait, but of a greater size, and which usually lie at the bottom of the sea, buried in the sand, have been ever since the commencement of the earthquakes, and continue still to be, taken near the surface, and in such abundance, as to be the common food of the poorest sort of people ; whereas, before the earthquakes, this fish was rare, and reckoned among the greatest delicacies. All fish, in general, have been taken in greater abundance, and with much greater facility, in those parts since they have been afflicted by earthquakes than before. I constantly asked every fisherman I met with on the coast of Sicily and Calabria, if this circumstance was true ; and was as constantly answered in the affirmative ; but with such emphasis, that it must have been very extraordinary. I suppose, that either the sand at the bottom of the sea may have been heated by the volcanic fire under it ; or that the continual tremor of the earth has driven the fish out of their strong holds, just as an angler, when he wants a bait, obliges the worms to come out of the turf on a river side, by trampling on it with his feet, which motion never fails in its effect, as I have experienced very often myself. The officer who commanded in the citadel, and who was there during the earthquake, assured me, that on the fatal 5th of February, and the 3 following days, the sea, about a quarter of a mile from that fortress, rose and boiled in a most extraordinary manner, and with a most horrid and alarming noise, the water in the other parts of the Faro being perfectly calm. This seems to point out exhalations or eruptions from cracks at the bottom of the sea, which may very probably have happened during the violence of the earthquakes ; all of which, I am convinced have here a volcanic origin. I perfectly understood the nature of the formidable wave that was said to have been boiling hot, and had certainly proved fatal to the baron of the country, the prince of Scilla, who was swept off the shore into the sea by this wave, with



2473 of his unfortunate subjects. The following is the fact. The prince of Scilla having remarked, that during the first horrid shock, which happened about noon the 5th of February, part of a rock near Scilla had been detached into the sea, and fearing that the rock of Scilla, on which his castle and town is situated, might also be detached, thought it safer to prepare boats, and retire to a little port or beach surrounded by rocks at the foot of the rock. The 2d shock of the earthquake, after midnight, detached a whole mountain, much higher than that of Scilla, and partly calcareous, and partly cretaceous, situated between the Torre del Cavallo and the rock of Scilla. This having fallen with violence into the sea, at that time perfectly calm, raised the fatal wave, which broke on the neck of land, called the Punta del Faro, in the island of Sicily, with such fury, and returning with great noise and celerity directly on the beach, where the prince and the unfortunate inhabitants of Scilla had taken refuge, either dashed them with their boats and richest effects against the rocks, or whirled them into the sea; those who had escaped the first and greatest wave were carried off by a 2d and 3d, which were less considerable, and immediately followed the first.

To conclude: the idea I have of the present local earthquakes is, that they have been caused by the same kind of matter that gave birth to the Æolian or Lipari islands; that perhaps an opening may have been made at the bottom of the sea, and most probably between Stromboli and Calabria Ultra, for from that quarter all agree, that the subterraneous noises seem to have proceeded; and that the foundation of a new island or volcano may have been laid, though it may be ages, which to nature are but moments, before it is completed, and appears above the surface of the sea. Nature is ever active; but her actions are, in general, carried on so very slowly, as scarcely to be perceived by mortal eye, or recorded in the very short space of what we call history, let it be ever so ancient. Perhaps too, the whole destruction I have been describing may have proceeded simply from the exhalations of confined vapours, generated by the fermentation of such minerals as produce volcanoes, which have escaped where they meet with the least resistance, and must naturally in a greater degree have affected the plain, than the high and more solid grounds around it.

*XII. Account of the Earthquake in Calabria, March 28, 1783. In a Letter from Count Francesco Ippolito to Sir W. Hamilton. From the Italian. p. 209.*

Calabria has been at all times exposed to the terrible convulsions, of which we are at present the victims. The earthquakes in 1638 and 1659, by which the two provinces of Calabria were almost utterly destroyed, are fresh in every one's memory, as well as that of the year 174 $\frac{3}{4}$ , which afflicted us for a long time, but without loss of cities or of men. Reggio, and the countries near it,



are exposed to earthquakes almost every year, and if we look back to the highest antiquity, we shall find that all Italy, but particularly this country, and more particularly still the provinces we inhabit, have been subject to various catastrophes in consequence of volcanos and subterraneous fires. But among so many earthquakes to which we have been exposed, the least is not that under which we at present suffer, whether we consider the force of the concussions, or their duration, or the changes that have taken place in the surface of the earth, or the ruin of so many cities and villages, with the loss of 40,000 inhabitants.

From the 5th of February to this instant the shocks have been more frequent, and almost every day repeated. At times the earth shook as it usually does on these occasions; but at others the motion was undulatory, and at others vorticose, during which last state it resembled a ship tossed about in a high sea. The most considerable of these repeated earthquakes were those which took place Feb. 5, 7, and 28; and finally on the 28th of March. These 4 eruptions coming, as nearly as we can judge by the phenomena and effects, from the chain of mountains which extend from Reggio hitherwards, have produced 4 different explosions in 4 different parts of Calabria. These explosions have produced various great effects; ruined cities and villages, levelled mountains, formed immense breaks in the earth, new collections of waters, old rivulets sunk in the earth and dispersed, rivers stopped in their course, soils levelled, small mountains which existed not before formed, plants rooted up, and carried to considerable distances from their first site, large portions of earth rolling about through considerable districts, animals and men swallowed up by the earth.

But I will confine myself to a short narrative of the effects of the last explosion of the 28th of March, which doubtless must have arisen from an internal fire in the bowels of the earth in these parts, as it took place precisely in the mountains which cross the neck of our peninsula which is formed by the two rivers, the Lameto which runs into the gulph of St. Euphemia, and the Corace, which runs into the Ionian sea, and properly into the bay of Squillace. That the thing was so is evident from all the phenomena. This shock, like all the rest, came to us in the direction of the s. w. At first the earth began to undulate, then it shook, and finally it moved in a vorticose direction, so that many persons were not able to stand on their feet. This terrible concussion lasted about 10 seconds; it was succeeded by others which were less strong, of less duration, and only undulatory; so that, during the whole night, and for half the next day, the earth was continually shaken, at first every 5 minutes, afterwards every quarter of an hour.

A terrible groan from under ground preceded this convulsion, lasted as long as it did, and finally ended with a loud noise, like the thunder of a mine that takes effect. These mighty thunderings accompanied not only the shocks of



that night and of the succeeding day, but all the others which have taken place since that time. At the time of the earthquake, during the night, flames were seen to issue from the ground in the neighbourhood of this city towards the sea, where the explosion extended, so that many countrymen ran away for fear; these flames issued exactly from a place where some days before an extraordinary heat had been perceived. After the great concussion there appeared in the air, towards the east, a whitish flame, in a slanting direction; it had the appearance of electric fire, and was seen for the space of 2 hours.

In consequence of the terrible shock, many countries and cities, especially those situated in the neighbourhood and neck of our peninsula as you go from Tiriolo to the river Angitola, and which had suffered nothing before, were overturned. Curinga, Maida, Cortale, Girifalco, Borgia, St. Floro, Settingiano, Marcellinara, Tiriolo, and other countries of less importance, were almost entirely destroyed, but with the loss of very few people. Many hundreds however perished in Maida, Cortale, and Borgia. Many hills were divided or laid level; many apertures were made in the surface of the earth throughout the whole surface which lies between the two vallies occupied by the rivers Corace and Lameto, towards Angitola. Out of many of these apertures a great quantity of water, coming either from the subterraneous concentrations, or the rivers themselves in the neighbourhood of which the ground broke up, spouted during several hours. From one of these openings in the territory of Borgia, about a mile from the sea, there came out a large quantity of salt water which imitated the motions of the sea itself for several days. Warm water likewise issued from the apertures made in the plains of Maida. In all the sandy parts, where the explosion took place, there were observed, from distance to distance, apertures in the form of an inverted cone, out of which likewise came water. This seems to prove that from thence escaped a flake of electric fire. Fissures of this kind are particularly met with along the banks of the Lameto.

Amidst the various phenomena, which either preceded or followed the earthquake, the two following are remarkable. On the very day of the earthquake the water of a well in Maida, which heretofore people used to drink, was infected with so disgusting a sulphureous taste, that it was impossible even to smell to it. On the other hand, at Catanzaro the water of a well, which before could not be used because of a smell of calcination that it had, became so pure as to be drunk extremely well. In Maida itself many fountains were dried up by the earthquake of the 28th. This likewise happened at other places; but many also broke out in several spots where there had been none before, as did also several mineral springs, of which before there was not a vestige. Commonly, however, the fountains became more swelled and more copious, and emitted a larger volume of water than usual. The waters of some fountains were also observed to be



troubled, and to assume a whitish or yellowish colour, according to the countries through which they passed. For a long time before the earth shook, the sea appeared considerably agitated, so as to frighten the fishermen from venturing upon it, without any visible winds to make it so. Our volcanos too, as I am confidently assured, emitted no eruptions for a considerable time before; but there was an eruption of Etna in the first earthquake, and Stromboli showed some fire in the last.

*XIII. Of the Black Canker Caterpillar, which destroys the Turnips in Norfolk.*

*By W. Marshall, Esq.\* Dated Gunton, Norfolk, Aug. 22, 1782. p. 217.*

Among the numerous enemies to which turnips are liable, none have proved more fatal here than the black canker, a species of caterpillar, which in some years have been so numerous as to cut off the farmer's hopes in a few days. In other years however the damage has been little, and in others nothing. About 20 years ago the whole country was nearly stripped; and this year it has been subjected to a similar fate. Many thousands of acres, on which a fairer prospect for a crop of turnips has not been seen for many years, have been plowed up; and as, from the season being now far spent, little profit can be expected from a second sowing; the loss to the farmers individually will be very considerable, and to the country immense.

It was observed in the canker-year above-mentioned, that, prior to the appearance of the caterpillars, great numbers of yellow flies were seen busy among the turnip plants; and it was then suspected that the canker was the caterpillar state of the yellow fly; and since that time it has been remarked, that cankers have regularly followed the appearance of these flies. From their more frequently appearing on the sea coast, and from the vast quantities which have at different times been observed on the beach, washed up by the tide, it has been a received opinion among the farmers, that they are not natives of this country, but come across the ocean, and observations this year greatly corroborate the idea. Fishermen on the eastern coast declare, that they actually saw them arrive in cloud-like flights; and from the testimony of many, it seems to be an indisputable fact, that they first made their appearance on the eastern coast; and that, on their first being observed, they lay upon and near the cliffs so thick and so languid, that they might have been collected into heaps, lying, it is said, in some places 2 inches thick. Thence they proceeded into the country, and even at the distance of 3 or 4 miles from the coast they were seen in multitudes resembling swarms of bees. About 10 days after the appearance of the flies, the young caterpillars were first observed on the under sides of the leaves of the turnips,

\* The insect here described appears to be the larva of some species of the Linnean genus *Tentredo*.



and in 7 or 8 days more, the entire plants, except the stronger fibres, were eaten up. A border under the hedge was regularly spared till the body of the inclosure was finished; but this done, the border was soon stripped, and the gateway, and even the roads have been seen covered with caterpillars travelling in quest of a fresh supply of turnips; for the grasses, and indeed every plant, except the turnip and the charlock (*sinapis arvensis*) they entirely neglect, and even die at their roots, without attempting to feed on them. This destruction has not been confined within a few miles of the eastern coast, but has reached, more or less into the very centre of the county. The mischief however in the western parts of Norfolk, and even on the north coast, has been less general; but it is thought that one half of the turnips in the county have been cut off by this voracious animal.

The following is Mr. M.'s description of the insect in its different states. The wings of the fly are 4; the antennæ clubbed, and about  $\frac{1}{3}$  of the length of the body, each being composed of 9 joints, namely, 2 next the head, above which 2 there is a joint somewhat longer than the rest, and above this 6 more joints, similar to the 2 below. Near the point of the tail of the female there is a black speck, outwardly fringed with hair; but which, opening longitudinally, appears to be the end of a case, containing a delicate point or sting, about  $\frac{1}{20}$  of an inch in length, which on a cursory view appears to be a simple lanceolated instrument, with a strong line passing down the middle, and serrated at its edges; but, on a closer inspection, and by agitating it strongly with the point of a needle, it separates into 3 one-edged instruments, hanger-like as to their general form, with a spiral line or wrinkle winding from the point to the base, making 10 or 12 revolutions, which line, passing over their edges, gives them some appearance of being serrated.

By the help of these instruments probably the female deposits her eggs in the edges of the turnip-leaf, or sometimes perhaps in the nerves or ribs on the under surface of the leaf. Mr. M. having put some fresh turnip-leaves into a glass containing several of the male and female flies, he perceived, by means of a simple magnifier, that one of the females, after examining attentively the edge of the leaf, and finding a part which appeared to have been bitten, unsheathed her instruments, insinuated them into the edge of the leaf, and having forced them asunder so as to open a pipe or channel between them, placed her pubes (the situation of which from repeated and almost incessant copulations he had been able to ascertain precisely, and to the lower part of which these instruments seem to be fixed) to the orifice, and having remained a few seconds in that posture, deliberately drew out the instruments (which the transparency of the leaf held against a strong light afforded him an opportunity of seeing very plainly) and proceeded to search for another convenient place for her purpose.



The caterpillar has 20 feet (6 of its legs being of considerable length, the other 14 very short) and in its first stage is of a jetty black, smooth as to a privation of hair, but covered with innumerable wrinkles. Having acquired its full size, it fixes its hinder parts firmly to the leaf of a turnip, or any other substance, and breaking its outer coat or slough near the head, crawls out, leaving the skin fixed to the leaf, &c. The under coat, which it now appears in, is of a bluish or lead colour, and the caterpillar is evidently diminished in its size. In every respect it is the same animal as before, and continues to feed on the turnips for some days longer: it then entirely leaves off eating, and becomes covered with a dewy moisture, which seems to exude from it in great abundance, and appearing to be of a glutinous nature, retains any loose or pliant substance which happens to come in contact with it, and by this means alone seems to form its chrysalis coat. From the generic characters of the fly Mr. M. concludes it to be a *Tenthredo* of Hill.

*XIV. On the Shortening of Wire by Lightning. By Mr. Edw. Nairne, F. R. S. p. 223.*

In the Philos. Trans. for the year 1780, vol. 70, [Abridg. vol. 14, p. 688,] were printed some experiments of Mr. N.'s, showing the method of shortening of wire by electricity. He afterwards met with a similar circumstance produced by lightning; which was as follows. On the 18th of June, 1782, Mr. Parker's house at Stoke-Newington was struck by lightning, between 2 and 3 o'clock in the afternoon. The lightning passed down the leaden pipe without side the house, which pipe did not reach to the ground by about 10 feet. Here the lightning struck from one of the nails which fastened this leaden pipe to the wall to the end of a crank iron that was driven in the wall opposite to it, within side the room, and to which was fastened the wire of a night-bolt, rather thicker than usual. This wire was so very loose before the accident happened, that the bolt could not be raised by the handle at the bed-side, so that they were obliged every night to take hold of the bolt itself to lift it up to fasten the door; but on the night after the accident had happened, on going to bed, they went to raise the bolt up as usual, to secure their chamber-door, when, to their great surprise, they found the bolt drawn up; and on trying to pull it down, they could not with all their strength. Mr. N. went the next day, and not only found the bolt drawn up, but the wire, which before was very loose, and much bent, was drawn very straight, and so tight, that when struck it produced a musical tone. The wire was judged to be shortened several inches; for, had the wire before the accident been straight, it must have shortened it above 2 inches to have drawn the bolt up.

The whole length of the wire from the bed-side to the bolt was about 30



feet; but the part of the wire on which the lightning passed was about 15 feet. Near the crank iron that was directly over the bolt were two wires, which passed through the wainscot to a single one belonging to an alarm. The lightning passed these two wires, without damaging them; but the single one was partly dispersed into smoke, blackening all the wainscot near it; also a great deal was melted into globules, which were found by using a magnet. This was the first instance that Mr. N. had ever met with of wire being contracted or shortened by the effect of lightning, though he had not the least doubt, but that it is always the case; and that is the reason that we find them mostly broke where the lightning has passed, if it does not melt them.

To know whether the lightning had anywise altered the property of the iron by melting it into globules, Mr. Cavendish tried them with different acids, and found that they scarcely showed any signs of effervescence even when heated over the fire. He next tried some iron filings, which he put to some of the same acid; these not only caused an effervescence, but were entirely dissolved. He also tried the pieces of steel struck off by striking a light, which being separated by a magnet from the pieces of flint effervesced with the same acids, and dissolved almost entirely, only half a grain being left out of 18, and these consisted chiefly of those parts that were melted in globules.

*XV. An Account of Ambergris. - By Dr. Schwediawer. p. 226.*

Ambergris, or Grey Amber, is a solid, opaque, inflammable substance, of a white grey, sometimes of a blackish colour, which melted or inflamed yields a peculiar smell, agreeable to most persons, but disagreeable to others. It is a hard brittle substance, yet not so hard as to admit a polish; nor has it like succinum, a polished appearance or transparency. On scraping it with a knife into powder, part of it adheres to the cold steel like wax; as it does likewise to the teeth when masticated; it yields also the impression of the nail; it has no peculiar but rather an earthy taste when chewed. It has in its natural state a peculiar strong smell. The older it grows the more it seems to become agreeable. This smell is rendered more sensible by rubbing it with the fingers, or by burning or melting it.

It melts in a moderate degree of heat into a blackish thick oil, and then smokes, scums, and flies by degrees entirely off, without leaving any coal behind; as it does likewise when put upon any heated metal, leaving only a black spot on it: when the metal is red-hot, it melts and inflames instantaneously, smokes strongly, and flies speedily off, without leaving the least mark behind. When brought near a burning candle it catches fire immediately, and burns with a clear bright flame till it is consumed. A red-hot needle easily penetrates through its substance, a blackish oil then exudes, but no part of it seems to ad-



here to the needle ; the needle however feels afterwards as if it had been put into wax. It is so light, that it swims not only on the sea, but also on the surface of fresh water. Its colour is white grey, or yellowish, or blackish, the first of which is esteemed the best. All ambergris, when kept for a certain time, is covered with a kind of white grey dust like chocolate. When broken it appears to be of a granulated texture ; and in some pieces it seems to be laid on in strata. It feels rather rough when first touched, but when rubbed with the finger, it feels like hard soap, or rather like that kind of stone which the mineralogists call *Smectis*.

It is found swimming on the sea, or the sea-coast, or in the sand near the sea-coast ; especially in the Atlantic Ocean, on the sea-coast of Brazil, and that of Madagascar ; on the coast of Africa, of the East Indies, China, Japan, and the Molucca Islands ; but most of the ambergris which is brought to England comes from the Bahama Islands, from Providence, &c. where it is found on the coast. It is also sometimes found in the abdomen of whales by the whale-fishermen, always in lumps of various shapes and sizes, weighing from half an ounce to 100lb. and upwards. The piece, which the Dutch East India Company bought from the king of Tydor, weighed 182 lbs. An American fisherman from Antigua found some years ago, about 52 leagues south-east from the Windward Islands, a piece of ambergris in a whale, which weighed about 130 lbs. and sold it for 500 pounds sterling.

In all the pieces of any considerable size, whether found on the sea or in the whale, which Dr. S. frequently examined with much care, he constantly observed a considerable quantity of black spots, which appear to be the beaks of the *sepia octopodia*. These beaks seem to be the substances which have hitherto been always mistaken for claws or beaks of birds, or for shells. The presence of these beaks in ambergris proves evidently, that all ambergris containing them is in its origin, or must have been once, of a very soft or liquid nature.

That ambergris is found either on the sea and sea-coast, or in the bowels of whales, is a matter of fact, which seems to be universally credited. But it has never been examined into and determined, whether the ambergris found on the sea and sea-coast is the same as that found in the whale : whether that found on the sea or sea-coast has some properties, or constituent parts, which that found in the whale has not : and lastly, whether that found in the whale is superior or inferior in its qualities and value to the former ? By conversing with many persons having had experience in the facts, Dr. S. finds as follows : that ambergris is sometimes found in the belly of the whale, but in that particular species only which is called the *spermaceti* whale, and which from its description and delineation appears to be the *Physeter Macrocephalus Linnæi*.

The New England fishermen have long known that ambergris is to be found



in the spermaceti whale ; and they are so convinced of this fact, that whenever they hear of a place where ambergris is found, they always conclude that the seas in that part are frequented by this species of whale. It was for this reason that a gentleman at Boston, on hearing several years ago that ambergris was frequently found on the coast of Madagascar, immediately proposed a plan for a spermaceti-whale fishery in that part of the world. But the East India Company frustrated the project, by pretending, that as it was in their territory the right of fishery could belong only to them. The plan itself however they never adopted.

The persons who are employed in the spermaceti whale fishery confine their views to the *physeter macrocephalus*. They look for ambergris in all the spermaceti whales they catch, but it seldom happens that they find any. Whenever they hook a spermaceti whale, they observe, that it constantly not only vomits up whatever it has in its stomach, but also generally discharges its fæces at the same time ; and if this latter circumstance takes place, they are generally disappointed in finding ambergris in its belly. But whenever they discover a spermaceti whale, male or female, which seems torpid and sickly, they are always pretty sure to find ambergris, as the whale in this state seldom voids its fæces on being hooked. They likewise generally meet with it in the dead spermaceti whales which they sometimes find floating on the sea. It is observed also, that the whale, in which they find ambergris, often has a morbid protuberance, or, as they express it, a kind of gathering in the lower part of its belly, in which, if cut open, ambergris is found. It is observed, that all these whales, in whose bowels ambergris is found, seem not only torpid and sick, but are also constantly leaner than others ; so that, if we may judge from the constant union of these two circumstances, it would seem that a larger collection of ambergris in the belly of the whale is a source of disease, and probably sometimes the cause of its death. As soon as they hook a whale of this description, torpid, sickly, emaciated, or one that does not dung on being hooked, they immediately either cut up the abovementioned protuberance, if there be any, or they rip open its bowels from the orifice of the anus, and find the ambergris, sometimes in one sometimes in different lumps of generally from 3 to 12 and more inches in diameter, and from one pound to 20 or 30 pounds in weight, at the distance of 2, but most frequently of about 6 or 7 feet from the anus, and never higher up in the intestinal canal, which, according to their description, is, in all probability, the *intestinum cæcum*, hitherto mistaken for a peculiar bag made by nature for the secretion and collection of this singular substance. That the part they cut open to come at the ambergris is no other than the intestinal canal is certain, because they constantly begin their incision at the anus, and find the cavity every where filled with the fæces of the whale, which from their colour



and smell it is impossible for them to mistake. The ambergris found in the intestinal canal is not so hard as that which is found on the sea or sea-coast, but soon grows hard in the air: when first taken out it has nearly the same colour, and the same disagreeable smell, though not so strong, as the more liquid dung of the whale has; but, on exposing it to the air, it by degrees not only grows greyish, and its surface is covered with a greyish dust like old chocolate, but it also loses its disagreeable smell, and, when kept for a certain length of time, acquires the peculiar odour which is so agreeable to most people. Some gentlemen with whom Dr. S. conversed, confessed, that if they knew not from experience that ambergris thus found will in time acquire the abovementioned qualities, they would by no means be able to distinguish ambergris from hard indurated fæces. This is so true, that whenever a whale voids its fæces on being hooked, they look carefully to see if they cannot discover among the more liquid excrements, of which the whale discharges several barrels, some pieces floating on the sea, of a more compact substance than the rest; these they take up and wash, knowing them to be ambergris.

It will now be proper to examine the principal question, whether all ambergris is generated in the bowels of the whale, or whether it is simply an extraneous substance taken in with the food? In order to elucidate this matter, it will be necessary to resolve the following questions: 1st. Whether there is any material difference between ambergris found on the sea or sea-coast, and that found in the bowels or among the dung of the whale, either with regard to its qualities and chemical principles, or with respect to the heterogeneous substances that are mixed with it? And 2dly, if there is any such difference, in what does it consist.

From the most exact information I have been able to procure on this subject, says Dr. S., I find that what several authors have asserted, that all ambergris found in whales is of an inferior quality, and therefore much less in price, is destitute of truth. Ambergris is only valued for its purity, lightness, compactness, colour, and smell. There are pieces of ambergris found on different coasts, which are of a very inferior quality, whereas there are often found pieces of it in whales of the first value; nay, several pieces found in the same whale, according to the abovementioned qualities, are more or less valuable. All ambergris found in whales has at first when taken out of the intestines very near the same smell as the liquid excrements of that animal have; it has then also nearly the same blackish colour: they find it in the whale sometimes quite hard, sometimes rather softish, but never so liquid as the natural fæces of that animal. And it is a matter of fact, that, after being taken out and kept in the air, all ambergris grows not only harder and whiter, but also loses by degrees its smell, and assumes such an agreeable one, as that in general has which is found swimming on the sea; therefore the goodness of ambergris seems rather to depend



on its age. By being accumulated after a certain length of time in the intestinal canal, it seems even then to become of a whiter colour, and less ponderous, and acquire its agreeable smell. The only reason why ambergris found floating on the sea generally possesses the abovementioned qualities in a superior degree, is because it is commonly older, and has been longer exposed to the air. It is more frequently found in males than females; the pieces found in females are in general smaller, and those found in males seem constantly to be larger and of a better quality, and therefore the high price in proportion to the size is not merely imaginary for the rarity-sake, but in some respect well founded, because such large pieces appear to be of a greater age, and possess the abovementioned qualities in general in a higher degree of perfection than smaller pieces.

Having discovered beaks of the cuttle fish in all the pieces of ambergris I had an opportunity of examining, it now remained to be ascertained, how those beaks became so constantly mixed with ambergris? In prosecuting this inquiry, I had the satisfaction to learn from the same persons who gave me the information abovementioned, that the *sepia octopodia*, or cuttle fish, is the constant and natural food of the spermaceti whale, or *physeter macrocephalus*. Of this they are so well persuaded, that whenever they discover any recent relics of it swimming on the sea, they conclude that a whale of this kind is, or has been, in that part. Another circumstance which corroborates this fact is, that the spermaceti whale on being hooked generally vomits up some remains of the *sepia*.\* Hence we may easily account for the many beaks, or pieces of beaks, of the *sepia* found in all ambergris. The beak of the *sepia* is a black horny substance, and therefore passes undigested through the stomach into the intestinal canal, where it is mixed with the fæces; after which it is either evacuated with them, or if these latter be preternaturally retained, forms concretions with them, which render the animal sick and torpid, and produce an obstipation, which ends either in an abscess of the abdomen, as has been frequently observed, or becomes fatal to the animal; whence in both the cases, on the bursting of its belly, that hardened substance, known under the name of ambergris, is found swimming on the sea, or thrown on the coast. From the preceding account, and my having

\* It will not be improper here to remark, to what an enormous size this species of *sepia* grows in the ocean. One of the gentlemen who was so kind as to communicate to me his observations on this subject, about ten years ago hooked a spermaceti whale that had in its mouth a large substance with which he was unacquainted, but which proved to be a tentaculum of the *sepia octopodia*, nearly 27 feet long: this tentaculum however did not seem to be entire, one end of it appearing in some measure corroded by digestion, so that in its natural state it may have been a great deal longer. With regard to its being a tentaculum of the cuttle, the fishermen could not have been mistaken, as they themselves often feed on the smaller sort of the same *sepia*. When we consider the enormous bulk of the tentaculum of the *sepia* here spoken of, we shall cease to wonder at the common saying of the fishermen, that the cuttle-fish is the largest fish of the ocean.—Orig.



constantly found the abovementioned beaks of the sepia in all pieces of ambergris of any considerable size, I think we may venture to conclude, that all ambergris is generated in the bowels of the physeter macrocephalus, or spermæcetæ whale, and there mixed with the beaks of the sepia octopodia, which is the principal food of that whale; and we may therefore define ambergris to be the preternaturally hardened dung or fæces of the physeter macrocephalus, mixed with some indigestible relics of its food.

There now remains only one objection to be obviated on this subject, and this relates to the chemical analysis of ambergris.\* Neumann obtained from 1 drachm of ambergris 5 grains of an acid phlegm, 2 scruples and a half of empyreumatic oil, and 2 grains of a volatile acid salt in a crystalline form.

Now if all ambergris owes its origin to the animal kingdom in the manner we have stated, how are we to account for the acid obtained from it by distillation? Would not ambergris, if it was really of an animal nature, like all other fæces of animals feeding on animal food, yield a volatile alkali? I confess this seems to be a material objection; but I reply to it, first, that though my experiments made on adulterated ambergris confirm those made by Neumann, Grim, Browne, and Geoffroy; yet from that analysis, it by no means follows, that ambergris is not an animal product. Two eminent chemists, Mr. Scheele, and Mr. Bergman, professor of chemistry at Upsal, have lately discovered that human calculi of the bladder, though of an animal origin, are nothing else but a peculiar concrete acid, approaching in its qualities very nearly to the native vegetable acid: and Professor Crell has lately shown, in a paper presented to the R. S., that the presence of an acid, far from proving any thing against an animal substance, is to be found in the fat of all animals. This indeed proves as little as if I should conclude on the opposite side of the question, that because the cruciform plants yield first a volatile alkali in distillation, they are of an animal nature. This, however, I have by repeated experiments with cochlearia, nasturtium, &c. seen to be constantly the case. With regard to the nature of the acid which is obtained by distillation from ambergris, nobody has hitherto to my knowledge examined it; and the experiments I made on it are insufficient to say any thing positive about it.

The great price of ambergris, an ounce of it being now sold in London for 17. sterling, has been hitherto the cause of its being so often adulterated, and of its being so little examined by chemists. If however a chemical analysis of its

\* Chemistry shows that in all animal excrements an acid is present, though different from that found in ambergris. Besides, we do not know whether the marine acid of the sea-water in which these animals constantly live, has not a share in changing the nature of their fæces; nor whether the fæces of all cetaceous animals are perhaps by their chemical analysis not materially different from those of animals living on the continent. We have a chemical analysis of these latter, but none has been hitherto made of the former.—Orig.



acid should be made, we ought to be certain that the ambergris employed has not been previously adulterated, especially as it is but too common to find it adulterated with flower of rice, or with styrax or other resins, which might deceive us in forming a solid judgment about the real nature of its acid. The adulteration of ambergris with any of the heterogeneous substances may be discovered by its not having all the qualities mentioned above as requisite for the purest and best ambergris.

The use of ambergris in Europe is now nearly confined to perfumery, though it has formerly been recommended in physic by several eminent physicians; hence the *essentia ambræ Hoffmanni*, *tinctura regia cod. Parisini*, *trochisci de ambra*, Ph. Wurtemberg, &c. &c. If we wish to see any medicinal effects from this substance, we must certainly not expect them from 2 or 3 grains, but give rather as many scruples of it for a dose; though even then I should not expect much effect from it, as I have taken of pure unadulterated ambergris in powder 30 grs. at once, without observing the least sensible effect from it. A sailor however, who had the curiosity to try the effect of recent ambergris on himself, took half an ounce of it melted on the fire, and found it a good purgative; which proves, that it is not quite an inert substance. In Asia and part of Africa ambergris is not only used as a medicine and as a perfume, but a great use is also made of it in cookery, by adding it to several dishes as a spice; a great quantity of it is also constantly bought by the pilgrims who travel to Mecca, probably to offer it there, and make use of it in fumigations, in the same manner as frankincense is used in Catholic countries. The Turks make use of it as an aphrodisiac. Our perfumers add it to scented pillars, candles, balls or bottles, gloves, and hair-powder; and its essence is mixed with pomatums for the face and hands, either alone or mixed with musk, &c. though its smell is to some persons extremely offensive.

I mentioned above that it is only one kind of whale from which our fishermen obtain the spermaceti, which they call for this reason the spermaceti whale: in this same fish it is that they find ambergris. They never search after the *physeter catodon*, the *physeter microps*, *physeter tursio*, and others of the same genus; but they aim at taking both the male and female of the *physeter macrocephalus*, though the male contains not only a larger quantity, but also in their opinion a better quality of spermaceti. 1st. It is to be observed, that this species has but one spout (fistula). This spout is not, as has been generally hitherto asserted, in the neck (cervix) of the fish, but in its front, and on the very edge of the head, bending obliquely on the left side, so that whenever he spouts it is always on that side only. 2dly. It is also remarkable, that the female of this whale has a power of drawing back its breasts after it has suckled the calf, so that it hardly appears to have any prominence on the belly, whereas when it



suckles they hang out very long. 3dly. It is not true, though it has hitherto been asserted, that the substance which we so absurdly name spermaceti, and which perhaps might with much greater propriety be called sebum physeteris, is found in the ventricles of the brain, and in the cavity of the spinal marrow of the physeter macrocephalus. This fat substance, which is nothing but a kind of suet, undoubtedly formed for some particular purpose of that whale, is contained in a peculiar bony triangular cavity or trunk, which is lodged near the brain, and occupies nearly the whole upper part of the head. This trunk has no communication with the brain, but is entirely separated from it by its bony laminae. The brain, as in all other fishes, is very small in comparison with the size of the whale, and lies directly behind the eyes. In order to know whether the trunk in which the spermaceti is lodged had any connection with the brain of the whale, one of the above-mentioned gentlemen had the curiosity to lance that trunk, which in its upper part is only covered with the skin; he found the whale not in the least affected by this, but on the brain being lanced, the same whale died immediately.

*XVI. Extract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1782. By Thomas Barker, Esq. p. 242.*

		Barometer.			Thermometer.						Rain.		
		Highest.	Lowest.	Mean.	In the House.			Abroad.			Lyndon.	Selbourn Hamp.	S. Lamb. Surry.
					Hig.	Low	Mean	Hig.	Low	Mean			
Jan.	Morn.	30.00	28.45	29.27	49	37	42½	49	21½	37½	2.333	4.64	2.23
	Aftern.				50	38	43	52	30½	41½			
Feb.	Morn.	29.99	28.72	29.50	48	33	37½	48	23	32	0.636	1.98	0.56
	Aftern.				49	34	38½	53	30½	38			
Mar.	Morn.	29.78	28.57	29.21	47	37	41	47½	23½	34	1.923	6.54	2.49
	Aftern.				49	38	42	54	37	44			
Apr.	Morn.	29.69	28.09	29.20	49½	41	44	45	30½	38	6.125	4.57	2.14
	Aftern.				50	42	45	55	38½	46			
May	Morn.	29.62	28.54	29.24	58	42	49	58	33	44½	5.722	6.34	4.10
	Aftern.				60	43	51	73½	42	55			
June	Morn.	30.05	29.06	29.60	68½	50	60	67½	44	55	1.295	1.75	0.49
	Aftern.				71	52	61½	82	52	66			
July	Morn.	29.82	29.10	29.51	68	57½	61½	62	50½	56	2.697	7.09	6.88
	Aftern.				69	59	62½	75	55	66½			
Aug.	Morn.	29.64	28.60	29.21	62	55	59	57½	46	52½	3.114	8.28	7.80
	Aftern.				63	56½	60	70½	55	63½			
Sept.	Morn.	29.89	28.73	29.47	62½	52	58	58	41	51	5.151	3.72	7.80
	Aftern.				64	53½	59½	67½	50½	61½			
Oct.	Morn.	29.86	28.80	29.40	52½	44½	48½	52	30	41	1.502	1.93	1.24
	Aftern.				54	45	49½	59½	42½	50			
Nov.	Morn.	30.10	28.51	29.40	45	34½	39½	42½	23	32	1.074	2.51	1.24
	Aftern.				45	35	40	49	33	38½			
Dec.	Morn.	30.02	28.95	29.57	44½	34½	39	43½	25	34	0.517	0.91	0.72
	Aftern.				44½	34½	39	48½	30	38			
Mean of all . . . . .				29.39			49			47	32.089	50.26	28.65



*XVII. On the Proper Motion of the Sun and Solar System; with an Account of several Changes that have happened among the Fixed Stars since the Time of Mr. Flamsteed. By Wm. Herschel, Esq., F. R. S. p. 247.*

That several of the fixed stars have a proper motion, is now already so well confirmed, that it will admit of no further doubt. From the time this was first suspected by Dr. Halley we have had continued observations that show Arcturus, Sirius, Aldebaran, Procyon, Castor, Rigel, Altair, and many more, to be actually in motion; and considering the shortness of the time we have had observations accurate enough for the purpose, it may rather be wondered that we have already been able to find the motions of so many, than that we have not discovered the like alterations in all the rest. Besides, we are well prepared to find numbers of them apparently at rest, as, on account of their immense distance, a change of place cannot be expected to become visible to us till after many ages of careful attention and close observation, though every one of them should have a motion of the same importance with Arcturus. This consideration alone would lead us strongly to suspect, that there is not, in strictness of speaking, one fixed star in the heavens; but many other reasons will render this so obvious, that there can hardly remain a doubt of the general motion of all the starry systems, and consequently of the solar one among the rest.

We might begin with principles drawn from the theory of attraction, which evidently oppose every idea of absolute rest in any one of the stars, when once it is known that some of them are in motion: for the change that must arise by such motion, in the value of a power which acts inversely as the squares of the distances, must be felt in all the neighbouring stars; and if these be influenced by the motion of the former, they will again affect those that are next to them, and so on till all are in motion. Now as we know that several stars, in divers parts of the heavens, do actually change their place, it will follow, that the motion of our solar system is not a mere hypothesis; and what will give additional weight to this consideration is, that we have the greatest reason to suppose most of those very stars, which have been observed to move, to be such as are nearest to us; and therefore their influence on our situation would alone prove a powerful argument in favour of the proper motion of the sun, had it actually been originally at rest.

Mr. H. first gives a short but general account of the most striking changes he had found to have happened in the heavens since Flamsteed's time. He had made what he calls 3 reviews. The first was made with a Newtonian telescope, something less than 7 feet focal length, a power of 222, and an aperture of  $4\frac{1}{4}$  inches. It extended only to the stars of the 1st, 2d, 3d, and 4th magnitudes. Of his 2d review he gave some account, in the Philos. Trans.; vols. 70, 71, 72: it was made with an instrument much superior to the former, of 85.2 inches



focus, 6.2 inches aperture, and power 227. It extended to all the stars in Harris's maps, and the telescopic ones near them, as far as the 8th magnitude. The catalogue of double stars, which he communicated to the R. S., and the discovery of the Georgium Sidus, were the result of that review. His 3d review was with the same instrument and aperture, but with a very distinct power of 460, which he had experienced to be much superior to 227, in detecting excessively small stars, and such as are very close to large ones. At the same time he had ready at hand smaller powers to be used occasionally after any particularity had been observed with the higher powers, in order to see the different effects of the several degrees of magnifying such objects. He had also 18 higher magnifiers, which gave a gradual variety of powers from 460 to upwards of 6000, in order to pursue particular objects to the full extent of the telescope, whenever a favourable interval of remarkable fine weather presented a proper opportunity for making use of them. This review extended to all the stars in Flamsteed's catalogue, with every small star about them, as far as the 10th, 11th, or 12th magnitudes, and occasionally much farther, to the amount of a great many thousands of stars. To show the practicability of what is here advanced, Mr. H. mentions, that the convenient apparatus of his telescope is such, that he had many a night, in the course of 11 or 12 hours of observation, carefully and singly examined not less than 400 celestial objects, besides taking measures of angles and positions of some of them with proper micrometers, and sometimes viewing a particular star for half an hour together, with all the various powers of his telescope. The particularities he attended to in this last review were, 1, the existence of the star itself, such as it is given in the British catalogue. 2. To observe well whether it was double or single, well defined or hazy. 3. To view and mark down its particular colour, whenever the altitude and situation of the star would permit it to be done with certainty. 4. To examine all the small stars in the neighbourhood, as far at least as the 12 magnitude, and note the same particulars concerning them, except the colours, which would have taken up too much time in committing to paper, and be of no very material use. The result of these observations he collects under a few general heads in the following articles.

1. *Stars that are lost, or have undergone some capital change, since Flamsteed's time.*—In the British catalogue we find 2 remarkable stars of the 4th magnitude in the constellation of Hercules, viz. the 80th and 81st. But these are no more to be seen, though often and diligently sought for.—In the northern claw of Cancer Flamsteed has placed 3 stars of the 6th magnitude; they are the 53d, 55th, and 56th of his catalogue. The latter of them is vanished. We find a very small telescopic star near the place where the 56th should be: this may possibly be the remains of that vanishing star; but that may be ascertained



by those astronomical gentlemen, who, having fixed instruments, can determine the place of this small star, and compare it with the 56th of Cancer, when it will appear how far their places agree.

The 19th Persei, a star of the 6th magnitude, is either lost, or so considerably removed from its place in Flamsteed's time, that it is no longer to be known.—The 108th Piscium, a star of the 6th magnitude, near the head of Aries is lost.—Two stars of the 6th magnitude, the 73d and 74th Cancri, in the southern claw, are either both lost, or at least have undergone such a remarkable change of magnitude, and one of them of place, that it is hardly possible to know them any longer.—The 8th Hydræ is lost. There is a star just by, which may be the 31st Monocerotis. If this should be the 8th Hydræ, and a small star near the latter should agree with the place of the 31st Monocerotis, then the magnitudes will be quite contrary to what Flamsteed makes them. There must, at all events, have been a very remarkable change.—The 26th Cancri is lost.—The 62d Orionis is lost; and a star near the 54th and 51st is not taken notice of by Flamsteed. Perhaps the 62d has changed place; if this should be the case, it must have a very considerable motion.—The 71st Herculis, a star of the 5th magnitude, is lost. The 70th and 71st are so near each other by Flamsteed's catalogue, that it cannot be determined without fixed instruments, which is the star wanting. There is a small telescopic star, within about 30 minutes north following, in a direction towards  $\mu$  Lyræ; if that should be the 71st, it is wonderfully changed both in place and size.

The 34th Comæ Berenices is lost: Flamsteed has marked it as a star of the 5th magnitude.

The 19th of the same constellation is also lost, or moved and changed in magnitude.

The 40th and 41st Draconis have undergone so great an alteration of place that we cannot possibly mistake it; for in Flamsteed's time they were above 3 minutes asunder, whereas now their distance is much less than half a minute.—There seems to be an alteration in the place of the 65th, 64th, 54th, and 57th Orionis; but without fixed instruments it cannot be ascertained in which of the stars it is.

2. *Stars that have changed their magnitude since Flamsteed's time.*— $\alpha$  Draconis is so much less than  $\beta$ , which is set down as a smaller star in Flamsteed's catalogue, that the change of magnitude cannot be doubted.— $\beta$  Ceti, marked of the 3d, and  $\alpha$  Ceti of the 2d, are evidently the reverse,  $\beta$  being by much the larger star.

$\zeta$  Serpentis is not near so large as  $\eta$ , and yet we find Flamsteed has placed them in the same class.



$\eta$  Cygni is a brighter star than  $\chi$ , though marked by Flamsteed of a less magnitude.

The 2d Ursæ minoris is marked of the 6th magnitude, but is certainly intitled to the 5th.

$\eta$  Bootis is much larger than  $\zeta$ .— $\delta$  Delphini is much larger than  $\alpha$ .— $\beta$  Trianguli is much larger than  $\alpha$ .— $\gamma$  Aquilæ is much larger than  $\beta$ .— $\sigma$  Sagittarii is larger than  $\delta$ ,  $\gamma$ , and  $\epsilon$ , though marked of an inferior magnitude.— $\delta$  Canis majoris is larger than  $\beta$ , and yet is marked to be less.— $\eta$  Serpentis is so much larger than  $\zeta$ , that they certainly should not have been put in the same order of magnitude.

$\alpha$  Serpentarii is larger than  $\gamma$  and  $\epsilon$ , though marked to be of a less magnitude than either.

$\beta$  Equulei is so much less than  $\alpha$  that it could hardly deserve to be put in the same class.

$\delta$  Delphini is larger than  $\epsilon$ , though placed in an inferior order.

$\epsilon$  Bootis is so much larger than  $\zeta$  that it should not be put into the same order.

$\delta$  Sagittæ is larger than  $\alpha$  and  $\beta$ , though placed in a lower order of magnitude.

$\delta$  Ursæ majoris is less than either  $\epsilon$ ,  $\zeta$ , or  $\eta$ , though it is marked of a superior order of magnitude. Besides, it is evidently visible, that  $\delta$  cannot be intitled to more than the 4th magnitude, or at most to between the 4th and 3d; on the contrary,  $\epsilon$ ,  $\zeta$ , and  $\eta$ , should be of the 2d, or at least between the 2d and 3d; all which is very different from Flamsteed's account of those remarkable stars.

$\alpha$  Ursæ majoris is less than any star marked of the same magnitude, and cannot have the least pretension to be called a star of between the 1st and 2d, as Flamsteed has marked it.

The 1st and 2d Hydræ are noted by Flamsteed as being of the 4th magnitude, whereas they now are only of the 8th or 9th. It is remarkable, that the 30th Monocerotis, which is situated between them, has retained the order assigned to it by Flamsteed, and being of the 6th serves to point out the change of the other two in a very evident manner.

$\gamma$  Lyræ is much larger than  $\beta$ .—The change in the magnitudes of the 31st and 34th Draconis is very striking, these two stars being just the contrary of what they are marked in Flamsteed's catalogue. The 31st, from the 7th is increased to the 4th: and the 34th, from being a star between the 4th and 5th, is reduced to one of the 6th, if not 7th magnitude.

The 44th Cancræ is much too small for the 6th magnitude. As  $\epsilon$  and others are marked of the 6th, this, on being compared to them, can be intitled to no more than the 8th or 9th order.

The 96th Tauri is small enough to be of the 8th magnitude, though marked as one of the 6th.



The 62d Arietis is of the 5th magnitude, though only marked of the 6th.

The magnitudes of the 12th and 14th Lyncis are just the reverse in the heavens to what Flamsteed has marked them. This denotes a double change of a star from the 5th to the 7th, and from the 7th to the 5th magnitude.—The 38th Persei, marked of the 6th magnitude, is increased so as to be equal to  $\theta$  and  $\alpha$  of the 4th. Also,  $\theta$  is less than  $\tau$  contrary to Flamsteed.—The 8th Monocerotis is less than the 76th Orionis, though the former should be of the 4th, and the latter only of the 6th magnitude.—The 23d Geminorum, though marked of the 5th, is less than the 21st of between the 6th and 7th magnitude.

The 26th Orionis is much too small for the magnitude of which it is marked to be, or rather is lost; for we can hardly take any one of the remaining telescopic stars for it.

$\xi$  Leonis in Flamsteed's time was of the 4th; but is now less than a star of the 5th magnitude.

3. *Stars newly come to be visible.*—Near Lacerta's tail-end is a star of between the 4th and 5th magnitude, not mentioned in Flamsteed's catalogue, though the 1st Lacertæ, not far from that place, is recorded.

The star of the 5th magnitude following  $\tau$  Persei, supposed to be  $\nu$  removed, is most likely new, unless future observations were to favour the supposed motion of this star. It is among the double stars of my 4th class, so that it will be easy to detect its proper motion.

A very considerable star, not marked by Flamsteed, will be found near the head of Cepheus. Its right ascension in time, is about  $2^m 19^s$  preceding Flamsteed's 10th Cephei, and it is about  $2^\circ 20' 3''$  more south than the same star. A considerable star in a direction from the 68th Geminorum towards the 61st is not to be found in Flamsteed. A star of a considerable magnitude preceding the 1st Equulei is not contained in Flamsteed's catalogue. It is a double star of the first class, the 61st of Mr. H.'s 2d collection, where measures of it will be found.

A considerable star following the 1st Sextantis, and another following the 7th, are not inserted.

Between  $\beta$  Cancræ and  $\delta$  Hydræ is a very considerable star not marked by Flamsteed, though its situation is very remarkable. As the constellation of Cancer contains so rich a collection of very small stars, it is to be wondered how a star of such consequence could be omitted, if it had been visible in Flamsteed's time.—Nearly  $1\frac{1}{2}$  degree north following  $\delta$  Herculis, almost in the direction of  $\delta$  and  $\nu$ , is a star of the 5th, or between the 4th and 5th magnitude, very visible to the naked eye. We can hardly think Flamsteed could have overlooked it, had it been there in his time.—About  $3^\circ$  south preceding  $\gamma$  Bootis, a considerable star not in Flamsteed's catalogue of the 6th magnitude; and south preceding  $\lambda$ , another, almost as large.



To return to the principal subject of this paper, which is the proper motion of the sun and solar system : does it not seem very natural, that so many changes among the stars,—many increasing their magnitude, while numbers seem gradually to vanish ;—several of them strongly suspected to be new-comers, while we are sure that others are lost out of our sight ;—the distance of many actually changing, while many more are suspected to have a considerable motion :—does it not seem natural that these observations should cause a strong suspicion that most probably every star in the heaven is more or less in motion ? And though we have no reason to think that the disappearance of some stars, or new appearance of others, nor indeed the frequent changes in the magnitudes of so many of them, are owing to their change of distance from us by proper motions, which could not occasion these phenomena without being inconceivably quick ; yet we may well suppose that motion is some way or other concerned in producing these effects. A slow motion, for instance, in an orbit round some large opaque body, where the star, which is lost or diminished in magnitude, might undergo occasional occultations, would account for some of those changes, while others might perhaps be owing to the periodical return of large spots on that side of the surface which is alternately turned towards us by a rotatory motion of the star. The idea also of a body much flattened by a quick rotation, and having a motion similar to the moon's orbit by a change of the place of its nodes, by which more of the luminous surface would at one time be exposed to us than another, tends to the same end ; for we cannot help thinking with M. de la Lande (Mem. 1776), that the same force which gave such rotations, would probably also occasion motions of a different kind by a translation of the centre. Now, if the proper motion of the stars in general be once admitted, who can refuse to allow that our sun, with all its planets and comets, that is, the solar system, is no less liable to such a general agitation as we find to obtain among all the rest of the celestial bodies.

Admitting this for granted, the greatest difficulty will be, how to discern the proper motion of the sun among so many other, and variously compounded, motions of the stars. This is an arduous task indeed, which we must not hope to see accomplished in a little time ; but we are not to be discouraged from the attempt. Let us, at all events, says Mr. H. endeavour to lay a good foundation for those who are to come after us. I shall therefore now point out the method of detecting the direction and quantity of the supposed proper motion of the sun by a few geometrical deductions, and at the same time show by an application of them to some known facts, that we have already some reasons to guess which way the solar system is probably tending its course.

Suppose the sun to be at *s*, fig. 8, pl. 6 ; the fixed stars to be dispersed in all possible directions and distances around, at *s*, *s*, *s*, *s*, &c. Now, setting aside the



proper motion of the stars, let us first consider what will be the consequence of a proper motion in the sun; and let it move in a direction from *A* towards *B*, and suppose it now arrived at *c*. Here, by a mere inspection of the figure, it will be evident, that the stars *s, s, s*, which were before seen at *a, a, a*, will now, by the motion of the sun from *s* to *c*, appear to have gone in a contrary direction, and be seen at *b, b, b*; that is, every star will appear more or less to have receded from the point *B*, in the order of the letters *ab, ab, ab*. The converse of this proposition is equally true; for if the stars should all appear to have had a retrograde motion, with respect to the point *B*, it is plain, on a supposition of their being at rest, that the sun must have a direct motion towards the point *B*, to occasion all these appearances. From a due consideration of what has been said, we may draw the following inferences.

1. The greatest or total systematical parallax of the fixed stars, fig. 9, will fall on those that are in the line *DE*, at rectangles to the direction *AB* of the sun's motion.
2. The partial systematical parallax of every other star, *s, s, s*, not in the line *DE*, will be to the total parallax, as the sine of the angle *Bsa*, (being the stars distance from that point towards which the sun moves,) to radius.
3. The parallax of stars at different distances will be inversely as those distances; that is, one half at double the distance, one 3d at 3 times, and so on; for the subtense *sc* remaining the same, and the parallactic angle being very small, we may admit the angle *ssc*, to be inversely as the side *ss*, which is the star's distance.—
4. Every star at rest, to a system in motion, will appear to move in a direction contrary to that in which the system is moving.

Hence it follows, that if the solar system be carried towards any star situated in the ecliptic: every star, whose angular distance in antecedentia (reckoned on the ecliptic from the star towards which the system moves) is less than 180 degrees, will decrease in longitude. And that, on the contrary, every star, whose distance from the same star (reckoned upon the ecliptic but in consequentia) is less than 180 degrees, will increase in longitude, in both cases without alteration of latitude.

The immense regions of the fixed stars may be considered as an infinitely expanded globe, having the solar system for its centre. With this idea it will occur to us, that no method can be so proper for finding out the direction of the motion of the sun, as to divide our observations on the systematical parallax of the fixed stars into 3 principal zones. These, for the convenience of fixed instruments, may be assumed so as to let them pass around the equator and the equinoctial and solstitial colures, every one being at rectangles to the other two, according to the 3 dimensions of solids. And since no observations can be so conveniently made to ascertain small relative proper motions among the fixed stars as those on double stars, Mr. H. continued his researches in that line with great



application, and can now furnish out these 3 zones, with a very complete set of double stars for such observations. We have the greatest reason to hope for success in this attempt; for he thinks there will be found a secular systematical parallax of some considerable value; nay possibly so short a space of time as 10 years may suffice to bring us acquainted with many hitherto unknown celestial motions. Mr. H. here gives a long list of the double stars proper for this purpose in each of the 3 zones; viz. in the equatorial zone, extending 10 degrees on each side of the equator, about 150 stars; the zone of the equinoctial colure, extending 10 degrees of a great circle on each side, will contain, as far as it is visible in our hemisphere, about 70 double stars; and the zone of the solstitial colure, of the same extent, will include about 120 double stars. And he adds a zone of the ecliptic, which contains among others, a great many double stars that may undergo occultations by the moon or planets. This is of the same extent, and includes about 120 double stars.

It remains now only to make an application of this theory to some of the facts we are already acquainted with, relating to the proper motion of the stars. Astronomers have already observed what they call a proper motion in several of the fixed stars, and the same may be supposed of them all. We ought therefore to resolve that which is common to all the stars, which are found to have what has been called a proper motion, into a single real motion of the solar system, as far as that will answer the known facts; and only to attribute to the proper motion of each particular star, the deviations from the general law the stars seem to follow in those movements. By Dr. Maskelyne's account of the proper motion of some principal stars, we find that Sirius, Castor, Procyon, Pollux, Regulus, Arcturus, and  $\alpha$  Aquilæ, appear to have respectively the following proper motions in right ascension.  $-0''.63$ ;  $-0''.28$ ;  $-0''.80$ ;  $-0''.93$ ;  $-0''.41$ ;  $-1''.40$ ;  $+0''.57$ ; and two of them, Sirius and Arcturus, in declination, viz.  $1''.20$  and  $2''.01$ , both southward. Let fig. 10, represent an equatorial zone, with the above mentioned stars referred to it, according to their respective right ascensions, having the solar system in its centre. Assume the direction AB from a point somewhere not far from the 77th degree of right ascension to its opposite 257th degree, and suppose the sun to move in that direction from s towards B; then will that one motion answer that of all the stars together: for if the supposition be true, Arcturus, Regulus, Pollux, Procyon, Castor, and Sirius, should appear to decrease in right ascension, while  $\alpha$  Aquilæ, on the contrary, should appear to increase. Again, suppose the sun to ascend at the same time in the same direction towards some point in the northern hemisphere, for instance, towards the constellation of Hercules; then will also the observed change of declination of Sirius and Arcturus be resolved into the single motion of the solar system. But lest Mr. H. should be censured for admitting so new and capital a motion



on too slight a foundation, he observes, that the concurrence of those 7 principal stars cannot but give some value to an hypothesis that will simplify the celestial motions in general. We know that the sun, at the distance of a fixed star, would appear like one of them; and from analogy we conclude the stars to be suns. Now, since the apparent motions of these 7 stars may be accounted for, either by supposing them to move just in the manner they appear to do, or else by supposing the sun alone to have a motion in a direction, somehow not far from that above assigned to it, we are no more authorized to suppose the sun at rest, than we should be to deny the diurnal motion of the earth, except in this respect, that the proofs of the latter are very numerous, whereas the former rests only on a few though capital testimonies. But to proceed: I have only mentioned the motions of those 7 principal stars, says Mr. H. as being the most noticed and best ascertained of all; I will now adduce a further confirmation of the same from other stars.

M. de la Lande gives the annexed table of the proper motion of 12 stars, both in right ascension and declination, in 50 years, in his *Astron.* tom. 4, p. 685. Fig. 11 represents them projected on the plane of the equator. They are all in the northern hemisphere, except Sirius, which must be supposed to be viewed in the concave part of the opposite half of the globe, while the rest are drawn on the convex surface. Regulus being added to that number, and Castor being double, we have 14 stars. Every star's

Etoiles.	Chang. d'arc. droite.	Chang. de declinaison.
Arcturus ..	— 1' 11"	.. — 1' 55"
Sirius ..	— 37	.. — 52
$\beta$ Cygni ..	— 3	.. + 49
Procyon ..	— 33	.. — 47
$\epsilon$ Cygni ..	+ 20	.. + 34
$\gamma$ Arietis ..	— 14	.. — 29
$\gamma$ Gemin. ..	— 8	.. — 24
Aldébaran +	3	.. — 18
$\beta$ Gemin. ...	— 48	.. — 16
$\gamma$ Piscium. ..	+ 53	.. + 7
$\alpha$ Aquilæ ..	+ 32	.. — 4
$\alpha$ Gemin. ...	— 24	.. — 1

motion, except Regulus, is assigned in declination as well as in right ascension, so that we have no less than 27 motions given to account for. Now, by assuming a point somewhere near  $\lambda$  Herculis, and supposing the sun to have a proper motion towards that part of the heaven, we shall satisfy 22 of these motions. For  $\beta$  Cygni,  $\alpha$  Aquilæ,  $\epsilon$  Cygni,  $\gamma$  Piscium,  $\gamma$  Arietis, and Aldebaran, ought on the supposed motion of the sun, to have an apparent progression, according to the hour circle 18, 19, 20, &c. or to increase in right ascension, while Arcturus, Regulus, the two stars of  $\alpha$  Geminorum, Pollux, Procyon, Sirius, and  $\gamma$  Geminorum, should apparently go back in the order 16, 15, 14, &c. of the hour circle, so as to decrease in right ascension; but according to M. de la Lande's table, excepting  $\beta$  Cygni and  $\gamma$  Arietis, all these motions really take place. With regard to the change of declination, we see that every star in the table should go towards the south; and here we find but 3 exceptions, in  $\beta$  and  $\epsilon$  Cygni, and  $\gamma$  Piscium; so that on the whole we have only 5 deviations out of 27 known mo-



tions, which this hypothesis will not account for. And these exceptions must be resolved into the real proper motion of the stars.

There are also some very striking circumstances in the quantities of these motions that deserve our notice. First, Arcturus and Sirius being the largest of the stars, and therefore probably the nearest, ought to have the most apparent motion, both in right ascension and declination; which is agreeable to observation, as we find by the table. Next, in regard to the right ascension only, Arcturus being better situated to show its motion, by theorem 2, ought to have it much larger, which we find it has. Aldebaran, both badly situated and considerably smaller than the two former, by the same theorem ought to show but little motion. Procyon, better situated than Sirius, though not quite so large, should have almost as much motion; for by the 3d theorem, on supposing it farther off because it appears smaller, the effect of the sun's motion will be lessened on it; whereas, on the other hand, by the 2d theorem, its better situation will partly compensate for its greater distance. This again is conformable to the table.  $\epsilon$  Cygni very favourably situated, though but a small star, should show it considerably, as well as  $\alpha$  Aquilæ; whereas  $\beta$  Cygni should have but little motion; and  $\gamma$  Piscium, best situated of all, should have a great increase of right ascension; and these deductions also agree with the table.

In the last place, a very striking agreement with the hypothesis is displayed in Castor and Pollux. They are both pretty well situated; and we accordingly find that Pollux, for the size of the star, shows as much motion in right ascension as we could expect; but it is remarkable, and seemingly contrary to our hypothesis, that Castor, equally well placed, shows by the table no more than half the motion of Pollux. Now, if we recollect that the former is a double star, consisting of 2 stars not much different in size, we can allow but about half the light to each of them, which affords a strong presumption of their being at a greater distance, and therefore their partial systematical parallax, by the 3d theorem, ought to be so much less than that of Pollux; which agrees wonderfully with observation. Not to mention the great difficulty in which we should be involved, were we to suppose the motion of Castor to be really in the star; for how extraordinary must appear the concurrence, that two stars, namely, those that make up this apparently single star, should both have a proper motion so exactly alike, that in all our observations hitherto they have not been found to disagree a single second, either in right ascension or declination, for 50 years together! Does not this seem strongly to point out the common cause, the motion of the solar system? With respect to the change of declination, I would observe, that the point of  $\lambda$  Herculis, which in fig. 11 is assumed as the apex of the solar motion, is not perhaps the best selected. A somewhat more northern situation may agree



better with the changes of declination of Arcturus and Sirius, which capital stars may perhaps be the most proper to lead us in this hypothesis.

It may be expected I should also mention something concerning the quantity of the solar motion; but here I can only offer a few distant hints. From the annual parallax of the fixed stars, which, from my own observations, I find much less than it has hitherto been counted to be, we may certainly admit that the diameter of the earth's orbit, at the distance of Sirius or Arcturus, would not nearly subtend an angle of 1 second; but the apparent motion of Arcturus, if owing to a translation of the solar system, amounts to no less than  $2''.7$  a year, as will appear if we compound the two motions, of  $1' 11''$  in right ascension, and  $1' 55''$  in declination, into one single motion, and reduce it to an annual quantity. Hence we may in a general way estimate, that the solar motion can certainly not be less than that which the earth has in her annual orbit.

P. S. In my paper I used a table of the proper motion of some fixed stars, which M. de la Lande has given us as an extract from Tob. Mayer's Opera inedita. But I am now furnished with the scarce edition of the original. This work contains a catalogue of the place of 80 stars, observed by Mr. Mayer in 1756, and compared with the same stars as given by Roemer in 1706. From the goodness of the instrument with which the observations, to which Mr. Mayer has compared his own, were made, he gives it as his opinion, that where the disagreement in the place of a star is but small, it may be attributed to the imperfection of the instrument; but that when it amounts to 10 or  $15''$ , it is a very probable indication of a proper motion of such a star. He adds, that when the disagreement is so much as in some stars which he names, he has not the least doubt of a proper motion. By this extensive table I thought it highly necessary immediately to examine the hypothesis of the motion of the solar system, that it might receive an early check from observations, if they should be unfavourable; or that, on the other hand, it might be supported by the additional evidence of more stars, if their apparent proper motions should coincide with the idea I have pointed out in my paper on this subject.

I have followed Mr. Mayer's judgement of his own and Roemer's observations, and left out of the list all the stars that do not show a disagreement amounting to  $10''$  in the places which are given for them in 1706 and 1756. I have also left out those 13, or rather 14 stars, which have already been examined in my paper, and have been shown to support the hypothesis I have advanced: the rest are here drawn up in 2 tables. The first contains the stars that agree with my assigned motion of the solar system; or rather which are thereby revolved into apparent, or partly apparent, and partly proper motions. The 2d table contains those stars whose motions cannot be accounted for by my hypothesis, and must



therefore be ascribed to a real motion in the stars themselves, or to some still more hidden cause of a still remoter parallax.

TABLE I.

Names of stars.	Mot. in right as.	Motion in decl.	Names of stars.	Mot. in right as.	Motion in decl.	Names of stars.	Mot. in right as.	Motion in decl.
$\beta$ Ceti.....	+ 32 ..		$\alpha$ Orionis .....	insens.	- 11	$\gamma$ Aquilæ ....		- 20
$\alpha$ Arietis....	+ 10 ..		$\mu$ Geminorum ..	- 16		$\gamma$ Capricorni ..	+ 19..	
$\delta$ Ceti.....	+ 15 ..		$\epsilon$ Navis .....	- 13	- 11	$\epsilon$ Pegasi.....		- 28
$\alpha$ Ceti.....	+ 16 ..		$\beta$ Cancrî .....		- 14	$\delta$ Capricorni ..	+ 24..	- 17
$\alpha$ Persei ....	+ 16 ..		$\epsilon$ Ursæ majoris..	- 54		$\alpha$ Aquarii ....	+ 13..	
$\eta$ Pleiadam..		- 16	$\zeta$ Hydræ .....	- 23		$\zeta$ Pegasi.....		- 13
$\gamma$ Eridani ..	+ 14 ..		$\gamma$ Leporis .....		- 10	Fomahand ..	+ 21..	
$\epsilon$ Tauri ....		- 11	$\epsilon$ Ursæ majoris..	- 33	+ 10	$\beta$ Pegasi ....	+ 12..	
$\alpha$ Aurigæ ..	+ 11 ..	- 11	$\alpha$ Serpentarii ..	insens.		$\alpha$ Andromedæ		- 21
$\beta$ Orionis ..	insens.	insens.	$\gamma$ Draconis ....	+ 12		$\beta$ Cassiopeæ ..	+ 34..	
$\beta$ Tauri ....	- 11 ..	- 13	$\alpha$ Lyræ.....	insens.	+ 14			

TABLE II.

Polaris ..	.. + 13	$\mu$ Geminorum ..	+ 15	$\beta$ Herculis ...	+ 14
$\gamma$ Ceti ....	- 14 ..	$\epsilon$ Canis majoris..	+ 10	$\gamma$ Cygni.....	+ 13
$\beta$ Persei....	- 13	$\zeta$ Hydræ .....	+ 24	$\epsilon$ Pegasi.....	- 14
$\alpha$ Leporis ..	.. + 11	$\alpha$ Hydræ .....	+ 13	$\zeta$ Pegasi.....	- 20

From the first table we gather, that the principal stars, Lucida Lyræ, Capella,  $\alpha$  Orionis, Rigel, Fomahand,  $\alpha$  Serpentarii,  $\alpha$  Aquarii,  $\alpha$  Arietis,  $\alpha$  Persei,  $\alpha$  Andromedæ,  $\beta$  Tauri,  $\beta$  Ceti, and 20 more of the most distinguished of the 2d and 3d rank of stars, agree with our proposed solar motion; when, on the contrary, the 2d table contains but a few stars, and not a single one of the first magnitude among them to oppose it. It is also remarkable, that many stars of the first table agree both in right ascension and declination with the supposition of a solar motion; whereas there is not one among those of the 2d table which opposes it in both directions. This seems to indicate that the solar motion, in some of them at least, has counteracted, and thereby destroyed the effect of their own proper motion in one direction, so as to render it insensible; otherwise it would appear improbable, that 8 stars out of 12, contained in the latter table, should only have a motion at rectangles, or in opposition to any one given direction. The same may also be said of 19 stars among those of the former table, that only agree with the solar motion one way, and are as to sense at rest in the other direction; but these singularities will not be near so remarkable when we have the motion of the sun to compound with their own proper motions.

It will be found, that I have placed the want of sensible motion of  $\alpha$  Lyræ and  $\alpha$  Orionis in right ascension, and of Rigel both in right ascension and declination, to the account of those stars that are in favour. These stars are so bright, that we may reasonable suppose them to be among those that are nearest



to us ; and if they had any considerable motion, it would most likely have been discovered, since the variations of Sirius, Arcturus, Procyon, Castor, Pollux, &c. have not escaped our notice. Now, from the same principle of the motion of the solar system, by which we have accounted for the apparent motion of the latter stars, we may account for the apparent rest of the former. Those two bright stars,  $\alpha$  Lyræ and  $\alpha$  Orionis, are placed so near the direction of the assigned solar motion, that from the application of my 2d theorem, their motion ought to be insensible in right ascension, and not very considerable in declination ; all which we find is confirmed by observation. With respect to Rigel and  $\alpha$  Serpentarii, admitting them both as stars large enough to have shown a proper motion, were their situation otherwise than it is, we find that they also should be apparently at rest in right ascension ; and Rigel having southern declination, and being a less considerable star than  $\alpha$  Orionis, which shows but  $11''$  motion towards the south in 50 years, its apparent motion in declination may, on that account, be also too small to become visible.

*XVIII. Some Experiments on the Ochra friabilis nigro fusca of Da Costa, Hist. Foss. p. 102 ; and called by the Miners of Derbyshire, Black Wadd. By Josiah Wedgwood, F. R. S. p. 284.*

The extraordinary circumstance of this substance taking fire on being slightly mixed with linseed oil, first discovered by accident in the year 1752, at Mr. Bassano's, a painter in Derby, has rendered it a subject of curiosity ; but, as it is now employed in considerable quantities, and very advantageously, as an oil-colour in ship and house-painting, it has a better claim to our attention.

Many years before, Mr. W. first collected some of this earth, which basseted out in a hollow way, near Winster, in Derbyshire, and he tried some experiments on it ; but as they were not very interesting to him at that time, and being occupied with other matters, he made no further use of it till Dec. 1782, when a series of experiments being made at the President's house on its inflammable property when mixed with oil, at which John Walsh, Esq. and several other gentlemen were present, Mr. Walsh sent him a specimen, and expressed his desire that Mr. W. would analyse, and make some further experiments on this extraordinary substance. Mr. Woodward, as well as Mr. Da Costa, has described this earth so minutely, that it cannot easily be mistaken ; but from the following experiments it will appear, that it should not be classed among the ochres not acted on by acids ; and that it may, with as great propriety, be called manganese as ochre.

*Exper. 1.* Mixed with porcelain biscuit body, it gives darker or lighter shades of black and brown, as the quantity is greater or less in proportion to the body.  
—*Exper. 2.* Mixed with linseed oil, in the quantity of a few penny-weights



only, into a paste, it dried very slowly, without producing any perceptible smoke or heat. The quantity perhaps was too small for ignition, and it was probably over-dosed with oil.—*Exper. 3.* When the mineral was previously calcined with a slight red heat about half an hour, the mixture of it with the oil dried much sooner and harder; a circumstance which, if not already known, may render it still more valuable to the painter. In other respects no difference could be observed.

*Exper. 4.* In the above low heat it suffers no alteration of colour or texture. In a heat of  $30^{\circ}$ , by the thermometer for measuring high degrees of heat, it loses its property of staining the hands, diminishes very considerably in bulk, acquires a little hardness, though it still proves friable between the fingers, and has its colour changed from a brownish to a blueish black. In a heat of  $80^{\circ}$  it begins to melt; and at  $95^{\circ}$  runs into a black scoria.—*Exper. 5.* With black flux, in a heat of  $90^{\circ}$ , by the above-mentioned thermometer, it yielded a button of lead, amounting, in one experiment to 21, and in another to 22 grains, from an ounce, or nearly  $\frac{1}{2}$ .

*Exper. 6.* Water extracts nothing from it. The mineral acids, with the assistance of heat, dissolve about 11 parts out of 12; but a large quantity of acid is necessary for this solution. The residuum is greyish white, full of bright micaceous particles, with a few fine filaments like those of asbestos, which suffer no change in a moderate red heat. In a heat of  $144^{\circ}$ , which is  $14^{\circ}$  beyond the fusion of cast iron, it ran into a perfect glass; but whether this was a vitrification of the pure earth itself, or of a combination of it with the argillaceous matter it was in contact with, the smallness of the quantity did not admit of ascertaining. On the Hessian crucible it formed a black glass; what adhered to the thermometer-piece was brown.

*Exper. 7.* On boiling with oil of vitriol to dryness, the bottom and sides of the mass became red like colcothar, the middle white, the intermediate parts yellow or reddish yellow, and some greenish. These appearances were at first attributed to a vitriol of iron in different degrees of calcination; but, on separating some of the purer white and red parts, the former were found to produce in vitrification the same colour as manganese does, the latter the same as colcothar; the other seemed to be a mixture of the two.

*Exper. 8.* A solution of the mineral in nitrous acid was precipitated, instead of common alkali, with Prussian lixivium, which has the property of throwing down from acids, iron, manganese, and all metallic bodies, but no one of the earthy class. When the addition of this lixivium ceased to make any further precipitation, common alkali, added afterwards, had also no effect; a proof that this mineral contains no earth soluble in acids, for that would have remained in



the liquor after the precipitation of the other matters by the Prussian, lixivium, and been precipitated by the alkali added at last.

*Exper. 9.* On precipitating a like solution by gradual additions of alkaline lixivium, and separating the precipitates as often as a fresh addition of the alkali occasioned any different appearance from what the preceding had done; the first precipitate was white; the next of a rusty red colour, like precipitate of iron; the last very white, while diffused through the liquor, and when settled, but in drying turned a little brown. The first, which was in a very small quantity, as nearly as could be judged by weighing the filters, about  $\frac{1}{10}$  part of the other 2, was found to be lead; the 2d was iron; and the 3d manganese, nearly in equal quantities, all pure, or very nearly so, from each other.

It appears from these experiments, that 22 parts of this mineral contain nearly two of indissoluble earth, chiefly micaceous, 1 of lead, about  $9\frac{1}{2}$  of iron, and the same quantity of manganese.

*Specimens of the colours produced by vitrification.*—o. The mineral itself.  
1. 2. 3. The first, second, and third precipitates.

*XIX. On the Method of preparing, with the least possible loss, pure and white fusible Salt of Urine and perfectly transparent Phosphoric Acid. By the Duke de Chaulnes, F. R. S. p. 288. From the French.*

Until of late years, phosphoric acid was prepared from the so called fusible salt of urine, a triple salt consisting of soda and ammonia united to the phosphoric acid.\* This fusible salt was obtained by evaporating urine, dissolving the saline residuum in distilled water, filtrating the solution, and then evaporating and crystallizing; and purifying it, by re-dissolving and re-crystallizing; during which operations much of the salt was commonly lost. In this memoir, the duke de Chaulnes shows in what manner, with the least possible loss, a pure and white fusible salt of urine, and a perfectly transparent phosphoric acid, may be obtained; but as these processes, which require much time and trouble, have been wholly set aside by Scheele's easier and more certain method of procuring phosphoric acid from bones, it is unnecessary to take up the reader's attention with a further account of this paper.

*\*XX. Experiments for ascertaining the Point of Mercurial Congelation. By Mr. Thos. Hutchins, Governor of Albany Fort, in Hudson's Bay. p. \*303.*

The following experiments, to determine the freezing point of quicksilver, were made by the direction of the R. S., at Albany Fort in Hudson's Bay, situated in the lat. of  $52^{\circ} 14'$  north and  $82^{\circ}$  west long. from Greenwich. The in-

\* Phosphate of soda and ammonia.



struments used were simply thermometers, except the apparatus F and G, furnished by Mr. Cavendish.

The first 5 experiments were made according to the directions sent by the Society, to obtain the point of congelation; as are the 2 next, to endeavour to ascertain the greatest degree of contraction mercury is capable of; then follow 2 experiments made in a different manner by Mr. H.'s own suggestion; and lastly, an account of mercury frozen in the open air without the aid of any artificial cold, which will be found to corroborate the preceding experiments, and determine the exact point of congelation to be at  $40^{\circ}$  below the cypher. Mr. H. here inserts a letter from the ingenious Dr. Black, of Edinburgh, who favoured him with some remarks on the experiments he made in 1775 to freeze quicksilver, and first suggested this method of ascertaining the point of congelation.

*Dr. Black's Letter referred to above. Dated Edinburgh, 5th Oct. 1779.*

I have read with pleasure the experiments made at Hudson's Bay, on the congelation of mercury, and observe that the author has succeeded perfectly in effecting it; but could not determine with precision what degree of cold was necessary to produce it. This, however, does not surprize me, as I have always thought it evident, from Professor Braun's experiments, that this degree of cold cannot be discovered conveniently by congealing the mercury of the thermometer itself. I shall not here give my reasons for this opinion; they would lengthen out this letter too much. I shall only propose what appears to me the proper manner of making the experiment, which is as follows: provide a few wide and short tubes of thin glass, sealed at one end and open at the other; the wideness of these tubes may be from half to three-quarters of an inch, and the length of them about three inches. Put an inch or an inch and half depth of mercury into one of these tubes, and plunging the bulb of the thermometer into the mercury, set the tube with the mercury and the thermometer in it into a freezing mixture, which should be made for this purpose in a common tumbler or water-glass; and, N. B. in making a freezing mixture with snow and spirit of nitre, the quantity of the acid should never be so great as to dissolve the whole of the snow, but only enough to reduce it to the consistence of panada. When the mercury in the wide tube is thus set in the freezing mixture, it (the mercury) must be stirred gently and frequently with the bulb of the thermometer; and if the cold be sufficiently strong, it will begin to congeal by becoming thick and broasy like an amalgam. As soon as this is observed, the thermometer should be examined without lifting it out of the congealing mercury; and I have no doubt, that in every experiment, thus made, with the same mercury, the instrument will always point to the same degree, provided it has been made and graduated with accuracy.



*Thermometers described.*—A represents a mercurial thermometer, with an air-bulb at the top, graduated 628 degrees below the cypher, and marked at every second degree. Makers, Nairne and Blount; the scale box-wood.

B, another mercurial thermometer graduated to 526° below the cypher, each line representing 2°, made by Nairne and Blount; the scale box-wood.

C, is a fine mercurial thermometer, with an air-bulb at the top graduated 2300° below the cypher, each division containing 5°; the scale made of box, by Troughton.

D, a small spirit thermometer on a box scale, made by Troughton, and divided to every single degree down to 160° below the cypher.

E, Another spirit thermometer, by the same maker (Troughton) graduated 90° below the cypher; the scale box.

F, a small mercurial thermometer, on an ivory scale, divided at every 5° between 220° above and 250° below the cypher; made by Nairne and Blount.

G, another mercurial thermometer, every way like the last-mentioned, except only reaching from 215° above to 250° below the cypher; by Nairne and Blount.

H, a spirit thermometer, made by Nairne and Blount, with which Mr. H. made meteorological observations from the year 1774.

Mr. H. next gives a register of the height of all these thermometers, as observed several times every day, in the natural cold, from Nov. 23, 1781, till March 9, 1782; during which trial the thermometer A was uniformly the lowest, and the greatest degree of cold indicated by it was  $-82^{\circ}$ , or 82 below 0, which was on Feb. 22, at 7 in the morning. He then records an experiment, made Dec. 15, 1781, with an artificial mixture, in which the lowest state of the thermometer was  $-448\frac{1}{2}$ , or  $448\frac{1}{2}$  below 0. And on which experiment he remarks as follows: viz. That, finding the thermometer on the evening of Dec. 14, was  $18^{\circ}$  below the cypher, he concluded the morning would afford an opportunity to make an attempt to fix the point at which quicksilver begins to freeze; he therefore put a bottle of spirit. nitri fortis on the top of the house in open air, that it might be of the same temperature when it was to be used. At 7 in the morning of the 15th, the thermometers were about  $23^{\circ}$  below 0; he therefore made preparations for the experiments, getting the quicksilver out into the air, providing glass tumblers for mixing the nitrous acid with the snow, &c. He put as much quicksilver into a glass cylinder as, when the thermometer F was introduced, just filled the bulbous part of the cylinder; the scale of the thermometer did not reach the length of the tube by about 3 inches. The experiment was made in the open air, on the top of the Fort, with only a few deer-skins sewed together, placed to windward for a shelter: there was plenty of snow, 18 inches deep, on the works, and the thermometers were close at hand. In thrusting the thermometer F into the quicksilver, the instrument rose to the cypher, but soon began to descend again; but being unwilling to lose time, Mr. H. stuck the apparatus into the snow, the sooner to bring it to the temperature of the air. He was in hopes, by shifting the instruments into 3 fresh mixtures, he should have been able to have produced a greater degree of cold than by one



only; yet it did not. He added more spirit of nitre, but without effect. At  $10^h\ 3^m\ 35^s$  he took out the apparatus, and raised the bulbous end to make the quicksilver run, but found it was frozen, so that it did not alter its figure in the least. He then placed it in the mixture, where it continued till  $10^h\ 1^m$ , when he made another trial as before, but without perceiving any alteration: however, to be more certain of its being frozen, he proposed to take out the thermometer; but all the strength in his fingers could not move it in the least, so that he and the officers, who stood by, were convinced it was frozen fast.

The thermometers used on this occasion were those marked A and F.

A 2d experiment was made the next day, Dec. 16, with the same instruments and artificial mixtures. Mr. H. was rather unfortunate in making too small a quantity of the freezing mixture at the beginning, which obliged him to make repeated additions to it: by this means the operation was not only retarded, but sometimes it even undid what had been done before; for in pouring in the nitrous acid part of it unavoidably came in contact with the bulbs of the instruments before it was mixed with snow. In this case it never failed making the thermometers rise suddenly much higher than where they stood before the spirit was added; and at length it only descended to  $206^\circ$ , which is not half so low as on the preceding day, though the temperature of the air was  $10^\circ$  colder, viz.  $34^\circ$ : yet it is remarkable, that though the thermometer was so much higher, the apparatus was sunk more than twice as low as the day before; for after having been long stationary at  $40^\circ$ , it sunk to  $95^\circ$ . He then made a fresh mixture, but it had no effect any way during three quarters of an hour he attended to it afterwards. During this idle interval he made the 3d experiment. Finding no alteration, he went down to breakfast, and on returning, was surprized to find the quicksilver in the apparatus thermometer had subsided into the bulb, and the standard thermometer had been very low, and was rising briskly. The spirit thermometer also showed the mixture had a less degree of cold than before. To be certain that the quicksilver in the apparatus thermometer was in the bulb, he took the apparatus out of the mixture, and examined it minutely for half a minute, till he was quite certain of it; and also that the quicksilver in the cylinder was frozen, and it is remarkable it did not liquify in all that time.

The 3d experiment was made during the continuance of that which immediately precedes it, and was the effect of chance; for the first freezing mixture, which had been used in the 2d experiment, standing in the glass close by him, he took down the thermometer G, and charged its cylinder with quicksilver, as in the other examples, and suspended it in the old mixture, with the mercurial thermometer B, and a spirit thermometer; the mixture seemed to have lost much of its coldness, as appeared by the thermometers. It seemed very extraordinary, that the apparatus, after having been so long stationary at  $43^\circ$ , should



yet contain fluid quicksilver; but he thought it was thicker than ordinary, as it did not run freely, but seemingly in pieces, not globules: however he put it back again into the mixture, and set it by as of no further use; but returning after breakfast, he found it was firmly frozen, so as to give no appearance of fluidity, though the included thermometer was only at  $40^{\circ}$ , which he considers the exact freezing point of quicksilver; and that the congelation was in fact begun before, and effected by only a longer continuance in the same degree of cold.

Mr. H. made a 4th experiment Jan. 7, 1782. This was with the mercurial thermometer A, and the apparatus F, as in the 1st and 2d experiments. The day was clear, with little wind at w. by s. or w. s. w. which is generally the case in this country in the coldest weather. The thermometers at 8 o'clock were as follows, according to the rotation of the letters from A to G,  $39^{\circ}\frac{1}{4}$ ,  $36^{\circ}\frac{1}{4}$ ,  $35^{\circ}$ ,  $25^{\circ}$ ,  $25^{\circ}$ ,  $34^{\circ}\frac{1}{4}$ ,  $34^{\circ}$  below the cypher. The apparatus thermometer F, after standing at  $42^{\circ}$  and  $41^{\circ}\frac{1}{4}$  for a considerable time, sunk at once to  $77^{\circ}$ , not gradually, but suddenly as a weight falls. The great descent of the quicksilver in the index thermometer A to  $440^{\circ}$  in the first freezing mixture he imputed to the coldness of the weather, but was surprized to find it did not sink more than  $10^{\circ}$  lower in the 2d mixture. It is remarkable, that after pouring in the first mixture on the 2d, the apparatus, which had risen a little before, sunk suddenly into the bulb. At  $11^h 21^m$  Mr. H. took the apparatus out to examine it, and, by shaking it in his hand, all of a sudden some of the quicksilver in the cylinder liquified; the concussion perhaps dissolved its solidity, for it was not above a minute out of the mixture. Wondering much at this unexpected phenomenon, as the quicksilver in the thermometer did not rise, he put it into the mixture again immediately; but finding the inclosed thermometer showed no alteration, his curiosity determined him to examine it again; therefore, about 4 minutes after, he took it out a 2d time, and found the surface of the quicksilver in the cylinder was liquified about  $\frac{1}{8}$  of the whole quantity; the rest formed a solid ball, including the bulb of the thermometer, which easily accounts for the quicksilver in that instrument remaining stationary.

A 5th experiment was made Feb. 22, 1781. The weather was clear and serene, the wind about s. s. w., and the several thermometers stood as follows, A  $82^{\circ}$ , B  $66^{\circ}$ , D  $34^{\circ}$ , E  $34^{\circ}$ , F  $42^{\circ}$ , G  $42^{\circ}$ , H  $46^{\circ}$ , at 7 in the morning, and at 8 o'clock they were A  $78^{\circ}$ , B  $114^{\circ}$ , D  $29^{\circ}\frac{1}{4}$ , E  $29^{\circ}\frac{1}{4}$ , F  $29^{\circ}\frac{1}{4}$ , G  $40^{\circ}$ , H  $43^{\circ}$ ; yet it is remarkable, that quicksilver which was constantly exposed to the air in a saucer was not frozen. Mr. H. imputes the small descent of the quicksilver in the thermometers to the great degree of the cold in the atmosphere as in the 6th experiment, for there the effect was similar. The most remarkable circumstance in this day's operation was the sudden descent of the quicksilver in the apparatus



thermometer, and the length of time it continued at  $79^{\circ}$  before the quicksilver in the cylinder became solid.

Experiment 6th was made Jan. 11, 1782. After a cold night, the quicksilver in the thermometer was at  $44^{\circ}$  below 0 at 7 in the morning: thinking this great degree of cold was the most favourable opportunity of observing how low it was possible to make the quicksilver descend in the tube of the thermometers, Mr. H. resolved to embrace it, and at the same time to observe the concurrent degrees with a spirit thermometer. The thermometers are marked from A to H, and the observations are regularly in that order. At the beginning they stood as follows, below 0, in the open air.

	A	B	C	D	E	F	G	H
At 7 <sup>h</sup> 45 <sup>m</sup>	44 $\frac{1}{2}$	45	41	28	29	40	40	46
7 50	46	64	124	30	32	42	41	46
7 55	—	—	60	—	—	—	—	—

It is observable, that neither the quicksilver which was in the cylinders affixed to F and G, nor the other quicksilver kept in the same place, some in a saucer, some in a gallipot, and some in a phial, showed the least appearance of congelation. Being engaged in preparing for the ensuing experiment, Mr. H. did not remark either the great descent or ascent of the quicksilver in c, which must have been very sudden, as his remarks are only 5 minutes asunder. The small descent of the quicksilver in c, and the little effect produced by moving it into a second mixture, made him at first apprehend the instrument was damaged; he did not however take it out, but took another thermometer A, and put it also in the mixture; but he found it was stationary at a higher degree than c: he therefore exchanged A for the mercurial thermometer B, which to his great surprise was stationary at  $86^{\circ}$ , nor could it be got lower till the cold of the mixture diminishing, it fell at once to  $434^{\circ}$ , and a few minutes afterwards c fell to  $360^{\circ}$ . Imagining that a new mixture would now bring it very low, he made another, but in the mean time the instruments had risen greatly, and after standing in the fresh mixture, c sunk to  $374^{\circ}$ , and B to  $438^{\circ}$ . These mixtures were double in quantity to those used in the former experiments; instead of glass tumblers, they were made in pint basons.

Mr. H. observed also, that the mixtures seemed to grow thin sooner than common; for he always made them of the consistence of pap. He added snow at times, to thicken it, but found it had very little effect, but rather decreased the cold. While the instruments were stationary in the foregoing experiment, he put the apparatus F and G severally into the mixture with the others; the consequence was, that in 2 minutes the quicksilver in the cylinder was frozen solid. At 9<sup>h</sup> 48<sup>m</sup> put in apparatus F, when it stood in the air at  $40^{\circ}$  or  $41^{\circ}$  below 0; and at 9<sup>h</sup> 50<sup>m</sup> took it out frozen solid, and the inclosed thermometer point-



ing still at  $40^{\circ}$  or  $41^{\circ}$ . He then hung it up in the open air, and looked at it only now and then. At  $10^{\text{h}} 47^{\text{m}}$ , after being exposed to the air near an hour, only a small quantity of the surface of the quicksilver was fluid, the rest was a frozen globe resembling a ball of polished silver; the thermometer inclosed was still at  $40^{\circ}$ . At  $11^{\text{h}} 4^{\text{m}}$  he observed a segment of a globe of solid quicksilver; in the inside was a concavity, made probably by the bulb of the thermometer. The thermometer was still at  $40^{\circ}$ , which undoubtedly is the freezing point of quicksilver, as in this instance part of it was frozen, and part solid. He withdrew the thermometer, poured out the fluid quicksilver, and returned the thermometer into the cylinder, shortly after which it was at  $37^{\circ}$ , and the frozen segment was then fluid.

The apparatus G was hanging in the open air at  $40^{\circ}$ , and put into the same freezing mixture at  $9^{\text{h}} 51^{\text{m}}$ , on which it sunk instantly to  $210^{\circ}$ , at which degree it was stationary at  $9^{\text{h}} 53^{\text{m}}$ , when it was taken out of the mixture perfectly solid. At  $10^{\text{h}} 6^{\text{m}}$  it had subsided into the bulb. Finding the quicksilver in the enclosed thermometer sink instantaneously as soon as the apparatus was put into the freezing mixture, it was taken out immediately to view it, and replaced in a few seconds of time. The quicksilver was not yet solid, but was in frozen pieces of irregular shapes, resembling ice that had been broken to pieces by concussion in a pail of water, but with this remarkable difference, that as ice swims on the water, the frozen quicksilver subsided in fluid quicksilver, and the segment of ice, found in the thermometer F was also at the bottom of the cylinder, and remained there after decanting the liquid quicksilver from it. Hence we may conclude, that cold increases the gravity of quicksilver, as indeed must be the case, since it is certain it occupies less space in a solid than in a fluid state.

Experiment 7th was made Jan. 22, 1782. From the 6th experiment Mr. H. was induced to think, that the nearer the temperature of the atmosphere approached to the freezing point of quicksilver, so that a great degree of cold might be communicated to the bulb of a thermometer and yet the quicksilver in the tube remain fluid, would be the properest time for ascertaining in this manner to what degree quicksilver will contract by the application of cold. With this view this 7th experiment was made: the several thermometers from A to H were as follow, before beginning; A 38, B 36, C 33, D 24, E  $24\frac{1}{2}$ , F 33, G 33, H 37. Those used in the experiment were C, D and H. The first was to show the descent of the quicksilver; and the other two, which were spirit thermometers, were employed to show the corresponding contractions of the two substances, quicksilver and alcohol. After above an hour's attendance on them, the quicksilver fell to  $1367^{\circ}$  below the cypher; and he supposed, by changing the mixture for a fresh one, he should get it still much lower. He made another accordingly, and removed the instruments into it. The quicksilver rose, as was



common on changing the mixtures ; but after waiting a considerable time, without its descending again, he recollected Professor Braun mentioning that his thermometers were always broken when below  $600^{\circ}$ . This made Mr. H. examine his, when he found the bulb was broken and fallen off ; but on a diligent search in the mixture, he could not find either quicksilver or the pieces of glass ; he therefore concluded it had dropped off into the other mixture, which unluckily he had thrown away the moment before, having occasion to use the bason in decanting the present mixture : he had no doubt but it broke at the time the quicksilver fell so rapidly. During the course of this experiment Mr. H. put the apparatus G into the freezing mixture ; in a minute's time the quicksilver in the inclosed thermometer had subsided into the bulb, and remained so during the time it continued immersed in the freezing mixture, which was about  $\frac{3}{4}$  of an hour ; but though the thermometer, which made part of the apparatus, showed so great a degree of cold, yet the quicksilver in the cylinder was never frozen ; and indeed the spirit thermometers, suspended in the mixture, seemed to indicate, that there was not sufficient cold to freeze quicksilver, except at the beginning ; for it is not effected at  $40^{\circ}$ , without continuing some time at that degree, as appears very clearly from the 3d experiment.

The 8th experiment was made Dec. 21, 1781, with a view to try whether quicksilver would freeze while in contact with the freezing mixture. For this purpose Mr. H. did not use the apparatus employed in the other examples, but substituted another, by taking a gallipot made of flint stone (being thinner than the common sort) of about an ounce measure, and filled it half full of quicksilver, into which he inserted the mercurial thermometer B, and employed the other mercurial thermometer A as an index, as before. Mr. H. hoped by this means to determine exactly when the quicksilver was congealed, as he had free access to it at all times, which was not the case when inclosed in the cylindrical glass, the worsted wound round the tube of the ivory thermometer to exclude the air, equally excluding any instrument from being introduced to touch the quicksilver. He made a kind of skewer, with a flat point, of dried cedar wood for lightness, which he found would remain in the gelatinous freezing mixture at any depth ; but when inserted into the quicksilver contained in the gallipot, the great disproportion of gravity made it rebound upwards, and by the touch he could easily perceive, by the resistance it met with, whether it proceeded from quicksilver in a fluid or congealed state. The event did not answer his wishes, for he could not find that the quicksilver was frozen in the least during the trial. Indeed the temperature of the air was not favourable, being under  $20^{\circ}$  below the cypher. The large quantity too of the quicksilver in the gallipot, as well as the thickness of that vessel, might both of them contribute to render the operation unsuccessful ; yet, as the apparatus thermometer showed the same degree



(— 40) as when quicksilver froze in the glass cylinder, Mr. H. was of opinion it would congeal by this simple method in very cold weather, and a long continued application of a proper degree of cold by the mixtures.

Exper. 9 was made Feb. 22, 1782. While Mr. H. was attending on the 5th experiment, and had removed the instruments into a 2d mixture, the former one by this means being unemployed, he put into it a gallipot (the same as used in the 8th experiment) with about  $\frac{3}{4}$  of a pound of quicksilver, and let it remain immersed in the mixture about half an hour, and finding, by touching with a quill, that part of it was congealed, he drew the gallipot out, it being previously slung with a string, and decanted off the super-incumbent mixture and fluid quicksilver; the remainder, about  $\frac{2}{3}$  of the whole quantity, remained solid in the gallipot; the internal surface remained every where very rough and white, shining like an old silver spoon long in use and having lost its polish. Part of it became fluid in a few minutes; and imagining it afforded a fine opportunity of confirming what had before appeared to be the freezing point of quicksilver, he put a mercurial thermometer F, which then stood at  $34^{\circ}$ , into the part of the quicksilver in the gallipot, which was just thawed, and it subsided directly to  $-40^{\circ}$ , and became stationary. He repeated the same with another instrument, and the result was the same. He then tried the spirit thermometer D, which became stationary at  $28^{\circ}\frac{1}{2}$ ; and another spirit thermometer E, which he took out of the freezing mixture, where it was at  $35^{\circ}$ , and it rose to  $30^{\circ}$ ; and by comparing the spirit thermometers with mercurial ones, and also with another spirit thermometer H, it appears, that  $29^{\circ}$  on the former is about equal to  $40^{\circ}$  on the scale of both the latter. By the time these observations were taken, the frozen lump was loosened in the gallipot: he turned it out, and beat it with a hammer; it yielded a dead sound and flattened, but its cohesion was very weak; for, instead of expanding into a thin plate, as in other instances when frozen in the bulb of a thermometer, it crumbled to pieces, and had not that polish which he had before constantly observed. Mr. H. attributed these circumstances to the effect of the spirit of nitre on the quicksilver. It thawed very soon after its parts were disjoined by the stroke of the hammer.

Exper. 10 was made Jan. 26, 1782, when the quicksilver was frozen by the natural cold in Hudson's Bay. The subject of this curious phenomenon was quicksilver put into a common two-ounce phial, and corked. The phial was about a third part full, and had been constantly standing by the thermometer for a month past. At 8 o'clock this morning Mr. H. observed it was frozen rather more than a quarter of an inch thick round the sides and bottom of the phial, the middle part continuing fluid. As this was a certain method to find the point of congelation, he introduced the mercurial thermometer F, and the spirit thermometer D, into the fluid part, after breaking off the top of the phial, and they



rose directly and became stationary; the former at  $40^\circ$  or  $40^\circ\frac{1}{2}$ , the latter at  $29^\circ\frac{3}{4}$ , both below the cypher. Having taken these out, he put in two others, G and E; the former became stationary at  $40^\circ$ , the latter at  $30^\circ$ . He then decanted the fluid quicksilver, to examine the internal surface of the frozen part, which proved very uneven, with many radii going across; some of these resembled pins with heads. Urgent business called Mr. H. away an hour. On his return he found a small portion only had liquified in his absence. He then broke the phial entirely, and with a hammer repeatedly struck the quicksilver. It beat out flat, yielded a deadish sound, and became fluid in less than a minute afterwards. By the comparative observations of the several thermometers it appears, that  $30^\circ$  on the scale of the spirit thermometers D and E, is about equal to  $40^\circ$  or  $41^\circ$  on the standard spirit thermometer H. The following was the state of the instruments that morning,

	A.	B.	D.	E.	F.	G.	H.
At 8 <sup>h</sup> .....	- 103 ....	- 80.....	$33\frac{1}{2}$ .....	33.....	$42\frac{1}{2}$ .....	42 ..	.. 46
At 9 .....	- 323 ....	- 444....	- 29 .....	- $29\frac{1}{2}$ .....	40 .....	- 40 ..	- 44.
At 12 .....	34 .....	32.....	21 .....	$21\frac{1}{2}$ .....	30 .....	$29\frac{1}{2}$ ..	.. 34

*XX. Observations on Mr. Hutchins's Experiments for determining the Degree of Cold at which Quicksilver freezes. By H. Cavendish, Esq., F.R.S. p. 303.*

The design of the following paper is to explain some particulars in the apparatus sent by Mr. C. to Mr. Hutchins, the intention of which does not readily appear; and also to endeavour to show the cause of some phenomena which occurred in his experiments; and point out the consequences to be drawn from them.

This apparatus was intended to determine the precise degree of cold at which quicksilver freezes: it consisted of a small mercurial thermometer, the bulb of which reached about  $2\frac{1}{2}$  inches below the scale, and was inclosed in a glass cylinder swelled at bottom into a ball, which, when used, was filled with quicksilver, so that the bulb of the thermometer was entirely surrounded with it. If this cylinder is immersed in a freezing mixture till great part of the quicksilver in it is frozen, it is evident that the degree shown at that time by the inclosed thermometer, is the precise point at which mercury freezes; for as in this case the ball of the thermometer must be surrounded for some time with quicksilver, part of which is actually frozen, it seems impossible that the thermometer should be sensibly above that point; and while any of the quicksilver in the cylinder remains fluid, it is impossible that it should sink sensibly below it. The ball of the thermometer was kept constantly in the middle of the swelled part of the cylinder, without danger of ever touching the sides, by means of some worsted wound round the tube. This worsted also served to prevent the access of the air to the quicksilver in the cylinder, which, if not prevented, would have made



it more difficult to have communicated a sufficient degree of cold. The diameter of the bulb of the thermometer was rather less than  $\frac{1}{4}$  of an inch; that of the swelled part of the cylinder was  $\frac{2}{3}$ ; so that there was no where a much less thickness of quicksilver between the ball and cylinder than  $\frac{1}{6}$  of an inch. The bulb of the thermometer was purposely made as small as it conveniently could, in order to leave a sufficient space between it and the cylinder, without making the swelled part of this larger than necessary; which would have caused more difficulty in freezing the quicksilver in it. Two of these instruments were sent, for fear of accidents.

One of the most striking circumstances in the experiments which have been made for freezing mercury, is the excessively low degree to which the thermometers sunk, and which, if it had proceeded, as was commonly supposed, from the freezing mixture having actually produced such a degree of cold, would have been really astonishing. The experiments however made at Petersburg afforded the utmost reason to suppose, and Mr. Hutchins's last experiments have put beyond a possibility of doubt, that quicksilver contracts in the act of freezing, or in other words, that it takes up less room in a solid than in a fluid state; and that the very low degree to which the thermometers sunk was owing to this contraction, and not to the intensity of the cold produced: for example, in one of Mr. Hutchins's experiments, a mercurial thermometer, placed in the freezing mixture, sunk to  $450^{\circ}$  below nothing, though the cold of the mixture was never more than  $-46$ ; so that the quicksilver was contracted not less than  $404^{\circ}$  by the action of freezing.

If a glass of water, with a thermometer in it, be exposed to the cold, the thermometer will remain perfectly stationary from the time the water begins to freeze, till it is entirely congealed, and will then begin to sink again. In like manner, when a thermometer is dipped into melted tin or lead, it remains perfectly stationary, from the time the metal begins to harden round the edges of the pot till it is all become solid, when it again begins to descend; and there was no reason to doubt that the same thing would obtain in quicksilver.

From what has been just said it was concluded, that if this apparatus was put into a freezing mixture of a sufficient coldness, the thermometer would immediately sink till the quicksilver in the cylinder began to freeze, and would then continue stationary, supposing the mixture still to keep cold enough, till it was entirely congealed. This stationary height of the thermometer is the point at which mercury freezes; though, in order to make the experiment convincing, it was necessary to continue the process till so much of the quicksilver in the cylinder was frozen as to put the fact out of doubt.

If the experiment had been tried with no further precautions, it is apprehended that considerable difficulties would have occurred, from want of knowing whe-



ther the cold of the mixture was sufficiently great, and when a sufficient quantity of the quicksilver was frozen; for, in the first place, there would be no judging when a sufficient quantity was frozen without taking out the apparatus now and then to examine it, which could not be done without a loss of cold; and, what is still worse, if before the experiment was completed the cold of the mixture was so much abated as to become less than that of congealing mercury, the frozen quicksilver would begin to melt, and the operator would have no way of detecting it, but by finding that great part of his labour was undone. For this reason two other mercurial thermometers were sent, called A and B by Mr. Hutchins, the scales of which were of wood, for which reason I shall call them, for shortness, the wooden thermometers, as I shall call the two others the ivory ones, their scales being of that material; they were graduated to about  $600^{\circ}$  below nothing, and their balls were nearly equal in diameter to the swelled part of the cylinders, in order that the quicksilver in both should cool equally fast; and it was recommended to Mr. Hutchins to put one of these into the freezing mixture along with the apparatus: for then, if the cold of the mixture was sufficient, both thermometers would sink fast till the quicksilver in the cylinder began to freeze, when the ivory thermometer would become stationary, but the wooden one would still continue to sink, on account of the contraction of the quicksilver in its ball by freezing; but if this last thermometer, after having continued to sink for some time after the ivory one had become stationary, ceased at last to descend, it would show that the mixture was no longer cold enough to freeze mercury; for as long as that was the case, the wooden thermometer would continue to descend by the freezing of fresh portions of quicksilver in its ball, but would cease to do so as soon as the cold was at all less than that. As I was afraid, however, that the quicksilver might possibly freeze and stick tight in the tube of this thermometer, and prevent its sinking, which would make the cold of the mixture appear too small when in reality it was not, one of these thermometers, instead of having a vacuum above the quicksilver as usual, was made with a bulb at top filled with air, that the pressure might serve to force down the quicksilver. If the degree of cold at which mercury freezes had been known, a spirit thermometer would have answered better; but that was the point to be determined.

As it appeared, from Mr. Hutchins's table of comparison, that these thermometers did not agree well together, they were all examined after they came back, except the ivory thermometer F, which was broken before it arrived. This loss, however, is of little consequence, as it appeared from his experiments that F and G agreed well together. The boiling and freezing points were first examined, when the divisions on the scale answering to them were found to be as follows:



	Boiling point.	Freezing point.
A.....	220.3.....	29.9
B.....	218.8.....	30.9
G.....	215.3.....	32.

From what has been said it appears, that  $183^{\circ}.3$  on the scale of G, are equal to only  $180^{\circ}$  on a thermometer adjusted as recommended by the committee, and therefore  $72^{\circ}$  are equal to  $70^{\circ}\frac{2}{3}$ ; so that the point of  $-40^{\circ}$  answers really to  $-38^{\circ}\frac{2}{3}$ ; that is, the cold shown by this thermometer at the temperature of about  $-40^{\circ}$ , is  $1^{\circ}\frac{1}{3}$  too great. In like manner it appears that the cold shown at that temperature by B is  $4^{\circ}\frac{1}{3}$ , and by A  $6^{\circ}\frac{1}{3}$ , too great.

Before entering on the examination of Mr. Hutchins's experiments, it will be proper to take notice of a phenomenon which occurs in the freezing of water; and is now found to take place in that of quicksilver, and which occasioned many remarkable appearances in these experiments. It is well known, that if a vessel of water, with a thermometer in it, be exposed to the cold, the thermometer will sink several degrees below the freezing point, especially if the water is covered up so as to be defended from the wind, and care is taken not to agitate it; and then, on dropping in a bit of ice, or on mere agitation, spiculæ of ice shoot suddenly through the water, and the inclosed thermometer rises quickly to the freezing point where it remains stationary.

This shows that water is capable of being cooled considerably below the freezing point, without any congelation taking place; and that as soon as by any means a small part of it is made to freeze, the ice spreads rapidly through the remainder of the water. The cause of the rise of the thermometer, when the water begins to freeze, is the circumstance now pretty well known to philosophers, that all, or almost all bodies, by changing from a fluid to a solid state, or from the state of an elastic to that of an unelastic fluid, generate heat; and that cold is produced by the contrary process. This explains all the circumstances of the phenomenon perfectly well; for as soon as any part of the water freezes, heat will be generated in consequence of the abovementioned law, so that the new formed ice and remaining water will be warmed, and must continue to receive heat by the freezing of fresh portions of water, till it is heated exactly to the freezing point, unless the water would become quite solid before a sufficient quantity of heat was generated to raise it to that point, which is not the case; and it is evident that it cannot be heated above the freezing point, for as soon as it comes to it, no more water will freeze, and consequently no more heat will be generated. The reason why the ice spreads all over the water, instead of forming a solid lump in one part, is, that as soon as any small portion of ice is formed, the water in contact with it will be so much warmed as to be prevented freezing; but the water at a little distance from it will still be below the freezing point, and will consequently begin to freeze.



If it was not for this generation of heat by the act of freezing, whenever a vessel of water, exposed to the cold, was arrived at the freezing point, and began to freeze, the whole would instantly be turned into solid ice; for as the new formed ice is not sensibly colder than water beginning to freeze, it follows, that as soon as all the water in the vessel was cooled to that point, the least addition of cold would convert the whole into ice; whereas it is well known, that though the whole vessel of water is cooled to, or even below the freezing point, there is a long interval of time between its beginning to freeze and being entirely frozen, during all which time it does not grow at all colder.

In like manner, it is the cold generated by the melting of ice which is the cause of the long time required to thaw ice or snow. It is this also which is the cause of the cold produced by freezing mixtures; for no cold is produced by mixing snow with any substance, unless part of the snow is dissolved. Mr. C. formerly found, by adding snow to warm water, and stirring it about till all was melted, that the water was as much cooled as it would have been by the addition of the same quantity of water, rather more than  $150^{\circ}$  colder than the snow; or, in other words, somewhat more than  $150^{\circ}$  of cold are generated by the thawing of snow; and there is great reason to think, that just as much heat is produced by the freezing of water. The cold generated was exactly the same whether he used ice or snow.\* He formerly kept a thermometer in melted tin and lead till they became solid; the thermometer remained perfectly stationary from the time the metal began to harden round the sides of the pot, till it was entirely solid; but he could not perceive it to sink at all below that point, and rise up to it when the metal began to harden. It is not unlikely however, that the great difference of heat between the air and melted metal might prevent this effect from taking place; so that though he did not perceive it in those experiments, it is not unlikely that those metals as well as water and quicksilver, may bear being cooled a little below the freezing or hardening point (for the hardening of melted metals and freezing of water seems exactly the same process) without beginning to lose their fluidity.

Mr. Hutchins's first 5 experiments were made with the apparatus, and in the

\* I am informed, says Mr. C., that Dr. Black explains the abovementioned phenomena in the same manner; only, instead of using the expression, heat is generated or produced, he says, latent heat is evolved or set free; but as this expression relates to an hypothesis depending on the supposition, that the heat of bodies is owing to their containing more or less of a substance called the matter of heat; and as I think Sir Isaac Newton's opinion, that heat consists in the internal motion of the particles of bodies, much the most probable, I chose to use the expression, heat is generated. Mr. Wilke also, in the Transactions of the Stockholm Academy of Sciences, explains the phenomena in the same way, and makes use of an hypothesis nearly similar to that of Dr. Black. Dr. Black, as I have been informed, makes the cold produced by the thawing of snow  $140^{\circ}$ ; Mr. Wilke,  $130^{\circ}$ .—Orig.



manner above described. In the first experiment the ivory thermometer, inclosed in the cylinder, sunk to  $-40^{\circ}$ , where it remained stationary for about half an hour, though the wooden thermometer, placed in the same mixture, kept sinking almost all the while. At the end of that time the apparatus was taken out of the mixture to be examined, and the quicksilver in the cylinder was found frozen. It seems evident therefore, that the true point at which mercury freezes is  $40^{\circ}$  below nothing on the thermometer R, which was that made use of in the experiment. It cannot be lower than that, for if it was, the thermometer could not have remained so long stationary at that point, while surrounded with freezing quicksilver; and it cannot be higher, as the thermometer could not sink below the freezing point, while much of the quicksilver, with which it was surrounded, remained unfrozen.

In the 2d exper., tried with the same apparatus, the ivory thermometer quickly sunk to  $-43^{\circ}$ ; but, in about half a minute, rose to  $-40^{\circ}$ , where it remained stationary for upwards of  $17^m$ . It appears therefore, that in this experiment the quicksilver was cooled  $3^{\circ}$  below the freezing point, without losing its fluidity; it then began to freeze, and the inclosed thermometer immediately rose to  $-40^{\circ}$ : so that this experiment, besides confirming the former, shows that quicksilver is capable of being cooled a little below the freezing point without freezing; and that it suddenly rises up to it as soon as it begins to lose its fluidity. In this experiment the cold was carried far enough to freeze the quicksilver in the ivory thermometer, which was not the case in the former: for after it had remained  $17^m$  stationary at  $-40^{\circ}$ , it began to sink again, and in about a minute sank to  $-44^{\circ}\frac{1}{2}$ ; it then sank instantaneously to  $-92^{\circ}$ , and soon after remained fixed for an hour and a quarter at  $95^{\circ}$ ; being then left without examination for three-quarters of an hour, the mercury was found to have sunk into the ball, the spirit thermometer showing at that time that the mixture was rather above the point of freezing, whereas before it had been below it. It appears therefore, that the quicksilver in the thermometer, after having descended to  $-44^{\circ}\frac{1}{2}$ , froze in the tube, and stuck there; but, being by some means loosened, sunk instantly to  $-92^{\circ}$ , and again stuck tight at  $95^{\circ}$ , till at last the mixture rising above the freezing point, the quicksilver in the tube melted, and sank into the ball, to supply the vacuum formed there by the frozen quicksilver. A similar accident of the quicksilver freezing in the tube of the thermometer, and sticking there, and then melting and sinking into the ball as the weather got warmer, has been found by Dr. Blagden to have happened to several gentlemen whose thermometers froze by the natural cold of the atmosphere, and with reason caused much perplexity to some of them. In this experiment the apparatus was not taken out to be examined till the ivory thermometer had sunk to  $-95^{\circ}$ ; it was then found to be frozen solid.



The 3d experiment was tried while the former was carrying on, and was made by putting the other apparatus, namely, that with the thermometers G and B, into the first mixture made for the former experiment, and which may consequently be supposed to have lost great part of its cold. The ivory thermometer quickly sank to  $-43^{\circ}$ , where it remained stationary for near  $12^m$ . The apparatus being then taken out to be examined, the quicksilver in the cylinder was found fluid, but thick and in grains, like crumbs of bread. The apparatus was then put back into the mixture; and, on observing the thermometer, it was found to have risen to  $-40^{\circ}$ , where it remained stationary about  $40^m$ ; being then examined, the quicksilver was found solid.

It appears therefore, that the cold of the mixture was sufficient to cool the quicksilver in the cylinder about  $3^{\circ}$  below the point of freezing, but did not make it freeze till, on taking out the apparatus, the agitation suddenly set it a freezing, and produced the appearance described by Mr. Hutchins. This immediately made the inclosed thermometer rise; so that when it was re-placed in the mixture and observed, it stood exactly at the freezing point. It appeared by the spirit thermometer, that the cold of the mixture, at the time the apparatus was first taken out to be examined, was only  $2^{\circ}$  below the point of freezing, which agrees very well with this explanation. This experiment therefore affords a fresh confirmation that the point of mercurial congelation is  $-40^{\circ}$  on these thermometers, and that quicksilver will bear being cooled a little below that point without freezing.

In the 4th, 5th, 6th, and 7th experiments a new phenomenon occurred, namely, the ivory thermometer sank a great deal below the freezing point without ever becoming stationary at  $-40^{\circ}$ . In the 5th experiment, tried with the apparatus G, it quickly sank to  $-42^{\circ}$ , and then, without remaining stationary at any point, sank in half a minute to  $-72^{\circ}$ , and soon after remained fixed at  $-79^{\circ}$ . While it was at  $-79^{\circ}$ , the apparatus was twice examined, and the quicksilver found fluid; but being again examined after having been removed into a fresh mixture, it was found solid. It seems likely from hence, that the quicksilver in the cylinder was quickly cooled so much below the freezing point as to make that in the inclosed thermometer freeze, though it did not freeze itself. If so, it accounts for the appearances perfectly well; nor does there seem any thing improbable in the explanation, except that it is contrary to what happened in the first 3 experiments; but the degree to which fluids will bear being cooled below the freezing point without freezing, seems to depend on such minute circumstances, that Mr. C. thinks this forms no objection. It must be observed, that the cold of the mixture appeared by the spirit thermometer to be 5 or 6 degrees below the freezing point; so that if the quicksilver in the cylinder was as cold as the mixture, it is not at all extraordinary that the thermometer



should have frozen; the only thing extraordinary is, that the quicksilver in the cylinder should have borne that cold without freezing. The same phenomenon occurred in the 6th and 7th experiments, on putting the same apparatus into the freezing mixture.

In the 4th experiment the ivory thermometer sank quickly to  $-42^{\circ}$ ; but soon after rose half a degree, probably from the cold of the mixture diminishing; it then, after having remained 6 or 7 minutes at those two points, sank very quick to  $-77^{\circ}$ . It does not appear at what time the quicksilver in the cylinder began to freeze, as it was not examined till long after the thermometer had sunk to  $-77^{\circ}$ , when it was found solid; but from the resemblance of this to the three former experiments, it is most likely, that it did not begin to freeze till after the thermometer had sunk to  $-77^{\circ}$ . In the 5th experiment the wooden thermometer was partly frozen before it was put into the freezing mixture, and the ivory one was at  $-40^{\circ}$ . On putting them into the mixture, they both rose; the latter, half a degree; the former, many degrees; which shows that the part of the mixture in which they were placed was rather warmer than the freezing point, though that in which the spirit thermometer was placed was colder.

Though in the 6th experiment the thermometer in the apparatus G froze without the quicksilver with which it was surrounded freezing, yet in trying the apparatus F in the same mixture, this did not happen; but, on the contrary, it afforded as striking a proof that the point of freezing quicksilver answers to about  $-40^{\circ}$  on this thermometer as any of Mr. Hutchins's experiments; for, on taking out the apparatus after it had been 2 minutes in the mixture, the quicksilver in the cylinder was found frozen solid, the inclosed thermometer standing at  $40^{\circ}$  or  $41^{\circ}$  below 0. After having been exposed for near an hour to the air, which was then very little above the point of freezing quicksilver, only a small quantity of the surface was become fluid; the rest formed a frozen globe round the ball of the thermometer, resembling polished silver, and in 17<sup>m</sup> after this only a segment of a globe of frozen quicksilver, with a concavity on the inside, formed by the ball of the thermometer, was observed, the thermometer all this while continuing the same as before, namely, at  $40^{\circ}$  or  $41^{\circ}$  below 0; so that in this experiment the ball of the thermometer was surrounded for more than an hour with quicksilver, which was visibly frozen and slowly melting, and during all which time it continued stationary at  $40^{\circ}$  or  $41^{\circ}$  below 0.

Though the foregoing experiments leave no reasonable room to doubt that this is the true point at which quicksilver freezes, yet Mr. Hutchins has, if possible, made this still more evident by his last 2 experiments; as, in the first of them, he froze some quicksilver in a gallipot immersed in a freezing mixture, so that the quicksilver was in contact with, and covered by, the snow and spirit of nitre; and in the latter in the open air, by the natural cold of the weather, and



then dipping the ball of the thermometer into the unfrozen part, observed what degree it stood at. These experiments agree with the former in showing the freezing point to be  $-40^{\circ}$  on the two mercurial thermometers; and also show what degree on the spirit thermometers answers to it, namely,  $29^{\circ}\frac{3}{4}$  or  $28^{\circ}\frac{1}{2}$  on D, and  $30^{\circ}$  on E; for in these two experiments the spirit thermometers also were dipped into the frozen quicksilver.

In all the experiments therefore, tried with the thermometer G, the freezing point came out  $-40^{\circ}$ . In those tried with F, it came out either  $-40^{\circ}$ , or about  $-40^{\circ}\frac{1}{2}$ ; so that as it appears from Mr. Hutchins's table of comparison, that F stood at a medium a quarter of a degree lower than G, the experiments made with that thermometer also show the freezing point to be  $-40^{\circ}$  on G; and as it appeared from the examination of this thermometer after it came home, that  $-40^{\circ}$  on it answers to  $-38^{\circ}\frac{2}{3}$ , on a thermometer adjusted in the manner recommended by the committee of the R. S., it follows, that all the experiments agree in showing that the true point at which quicksilver freezes is  $38^{\circ}\frac{2}{3}$ , or in whole numbers  $39^{\circ}$  below O.

It appears then, that the point at which quicksilver freezes has been determined by Mr. Hutchins in different ways, all perfectly satisfactory, and all agreeing in the same result. In the first 3 experiments the thermometer was surrounded by quicksilver, which continued freezing till it became solid. In the 6th experiment the quicksilver with which it was surrounded continued slowly melting till the whole was dissolved; and in both cases the thermometer remained stationary all the while at what we have just said to be the freezing point. In the 9th and 10th experiments, the ball of the thermometer was dipped into quicksilver, previously frozen and beginning to melt, as usually practised in settling the freezing point on thermometers, and agreed in the same result, the quicksilver in the last experiment being frozen by the natural cold of the atmosphere; and in the former, by being immersed in, and in contact with, a freezing mixture; so that this point appears to be determined in as satisfactory a manner as can be desired; and the more so, as it seems impossible that experiments should be made with more care and attention, or more faithfully and circumstantially related, than these have been. The 2d and 3d experiments also show, that quicksilver, as well as water, can bear being cooled a little below the freezing point without freezing, and is suddenly heated to that point as soon as it begins to congeal.

*On the contraction of quicksilver in freezing.*—All these experiments prove, that quicksilver contracts or diminishes in bulk by freezing; and that the very low degrees to which the thermometers have been made to sink, is owing to this contraction, and not to the cold having been in any degree equal to that shown by the thermometer. In the 4th experiment the thermometer A sunk to  $-45^{\circ}$ , though it appeared by the spirit thermometers that the cold of the



mixture was not more than  $5^{\circ}$  or  $6^{\circ}$  below the point of freezing quicksilver. In the first experiment also, it sunk to  $-448^{\circ}$ , at a time when the cold of the mixture was only  $2^{\circ}\frac{1}{4}$  below that point; so that it appears, that the contraction of quicksilver, by freezing, must be at least equal to its expansion by  $404^{\circ}$  of heat. This, however, is not the whole contraction which it suffers; for it appears, by an extract from Mr. Hutchins's meteorological journal, kept by him at Albany Fort, that his thermometer once sunk to  $490^{\circ}$  below 0, though it appeared, by a spirit thermometer, that the cold scarcely exceeded the point of freezing quicksilver. There are two experiments also of Professor Braun, in which the thermometer sunk to  $544^{\circ}$  and  $556^{\circ}$  below 0, which is the greatest descent he ever observed without the ball being cracked. It is not indeed known how cold his mixtures were; but from Mr. Hutchins's there is great reason to think that they could not be many degrees below  $-40^{\circ}$ . If so, the contraction which quicksilver suffers in freezing is sometimes not much less than its expansion by  $500^{\circ}$  or  $510^{\circ}$  of heat, that is almost  $\frac{1}{3}$  of its whole bulk, and in all probability is never much more than that, being probably no very determinate quantity.

*On the cold of the freezing mixtures.*—The cold produced by mixing spirit of nitre with snow is owing, as was before said, to the melting of the snow. Now, in all probability, there is a certain degree of cold in which the spirit of nitre, so far from dissolving snow, will yield out part of its own water, and suffer that to freeze, as is the case with solutions of common salt; so that if the cold of the materials before mixing be equal to this, no additional cold can be produced. If the cold of the materials be less, some increase of cold will be produced; but the total cold will be less than in the former case, since the additional cold cannot be generated without some of the snow being dissolved, and thus weakening the acid, make it less able to dissolve more snow; but yet the less the cold of the materials is, the greater will be the additional cold produced. This is conformable to Mr. Hutchins's experiments; for in the 5th experiment, in which the cold of the materials was  $-40^{\circ}$ , the additional cold produced was only  $5^{\circ}$ . In the first experiment, in which the cold of the materials was only  $-25^{\circ}$ , an addition of at least  $19^{\circ}$  of cold was obtained; and by mixing some of the same spirit of nitre with snow in this climate, when the heat of the materials was  $+26^{\circ}$ , I have sunk the thermometer to  $-29^{\circ}$ ; so that an addition of  $55^{\circ}$  of cold was produced.

However extraordinary it may at first appear, there is the utmost reason to think, that a rather greater degree of cold would have been obtained if the spirit of nitre had been weaker; for Mr. C. found, by adding snow gradually to some of this acid, that the addition of a small quantity produced heat instead of cold; and it was not until so much was added as to increase the heat from  $28^{\circ}$  to  $51^{\circ}$ ,



that the addition of more snow began to produce cold; the quantity of snow required for this purpose being pretty exactly  $\frac{1}{4}$  of the weight of the spirit of nitre, and the heat of the snow and air of the room, as well as of the acid, being  $28^{\circ}$ . The reason of this is, that a great deal of heat is produced by mixing water with spirit of nitre, and the stronger the spirit is, the greater is the heat produced. Now it appears from this experiment, that before the acid was diluted, the heat produced by its union with the water formed from the melted snow was greater than the cold produced by the melting of the snow; and it was not till it was diluted by the addition of  $\frac{1}{4}$  of its weight of that substance, that the cold generated by the latter cause began to exceed the heat generated by the former. From what has been said it is evident, that the cold of a freezing mixture, made with the undiluted acid, cannot be quite so great as that of one made with the same acid, diluted with a quarter of its weight of water, supposing the acid and snow to be both at  $28^{\circ}$  of heat, and there is no reason to think, that the event will be different if they are colder; for the undiluted acid will not begin to generate cold until so much snow is dissolved as to increase its heat from  $28^{\circ}$  to  $51^{\circ}$ , so that no greater cold will be produced than would be obtained by mixing the diluted acid heated to  $51^{\circ}$  with snow of the heat of  $28^{\circ}$ . This method of adding snow gradually to an acid is much the best way of finding what strength it ought to be of, in order to produce the greatest effect possible.

By means of this acid, diluted in the above-mentioned proportion, Mr. C. froze the quicksilver in the thermometer called G by Mr. Hutchins, on the 26th of February. He did not, indeed, break the thermometer to examine the state of the quicksilver in it; for as it sunk to  $-110^{\circ}$  it must certainly have been in part frozen; but immediately took it out, and put the spirit thermometer in its room, in order to find the cold of the mixture. It sank only to  $-30^{\circ}$ ; but, by making allowance for the spirit in the tube being not so cold as that in the ball, it appears, that if it had not been for this cause it would have sunk to  $-35^{\circ}$ , which is  $5^{\circ}$  below the point of freezing, and is as great a degree of cold, within  $1^{\circ}$ , as was produced in any of Mr. Hutchins's experiments.

In this experiment the thermometer G sank very rapidly, and, as far as Mr. C. could perceive, without stopping at any intermediate point, till it came to the above-mentioned degree of  $-110^{\circ}$ , where it stuck. The materials used in making the mixture were previously cooled, by means of salt and snow, to near 0; the temper of the air was between  $20^{\circ}$  and  $25^{\circ}$ ; the quantity of acid used was  $4\frac{1}{4}$  oz.; and the glass in which the mixture was made was surrounded with wool, and placed in a wooden box, to prevent its losing its cold so fast as it would otherwise have done. Some weeks before this, he made a freezing mixture with some spirit of nitre, much stronger than that used in the foregoing experiment, though not quite so strong as the undiluted acid, in which the cold



was less intense by  $4^{\circ}\frac{1}{2}$ , as the thermometer G sank to  $-40^{\circ}\frac{1}{2}$ . It is true, that the temper of the air was much less cold, namely,  $35^{\circ}$ ; but the spirit of nitre was at least as cold, and the snow not much less so. The experiment was tried in the same vessel and with the same precautions as the former. The cold produced by mixing oil of vitriol, properly diluted with snow, is not so great as that procured by spirit of nitre, though it seems not to differ from it by so much as  $8^{\circ}$ ; for a freezing mixture, prepared with diluted oil of vitriol, whose specific gravity, at  $60^{\circ}$  of heat, was 1.5642, sunk the thermometer G to  $-37^{\circ}$ , the experiment being tried at the same time, and with the same precautions, as the foregoing. It was previously found, by adding snow gradually to some of this acid, as was done by the spirit of nitre, that it was a little, but not much stronger than it ought to be, in order to produce the greatest effect.

*XXI. History of the Congelation of Quicksilver. By Chas. Blagden, M.D.,  
F. R. S. p. 329.*

The late experiments at Hudson's Bay have determined a point, on which philosophers not only were much divided in their opinion, but also entertained, in general, very erroneous sentiments. Though many obvious circumstances rendered it improbable, that the term of mercurial congelation should be 5. or 600 degrees below 0 of Fahrenheit's scale, as had been at first supposed; yet scarcely any one ventured to imagine that it was short of  $100^{\circ}$ . Mr. Hutchins, however, has clearly proved, that even this number is far beyond the truth; and that quicksilver freezes in a degree of cold not exceeding that which sometimes occurs in the northern parts of Europe, and frequently in the more rigorous climates of Asia and America.

It was undoubtedly M. Braun, professor of philosophy in the Imperial Academy at Petersburg, who first, on decisive evidence, established the fact, that quicksilver can be made solid by a diminution of its heat. M. Braun undertook the experiments at the suggestion of Dr. Zeiher, professor of mechanics in the same academy, who having repeated Fahrenheit's experiments with frigorific mixtures in Germany, before he came to settle at Petersburg, wished to try whether they might not be prosecuted further in the great natural cold which sometimes prevails in that city. Illness prevented Dr. Zeiher from carrying his ideas into execution; he therefore communicated them to Professor Braun, who was already much conversant in thermometrical experiments, and engaged him to take up the subject of artificial cold whenever the weather should be favourable for this purpose. A proper opportunity occurred on the 14th of December, 1759, o. s. the thermometer sinking in the open air so low as  $-34^{\circ}$  of Fahrenheit's scale, which we now know to be within a few degrees of the point at which mercury freezes. M. Braun accordingly prepared a frigorific mixture with



aqua fortis and pounded ice, by means of which his thermometer was reduced to  $-69^{\circ}$ , lower, by almost 30 degrees, than it had fallen in any preceding experiments of this nature.

Animated by the hope that a still greater degree of cold might be produced, he entered on the experiment anew; and all his pounded ice being expended, he was fortunately obliged to substitute snow in its place. With this fresh mixture he had the satisfaction of seeing the mercury in his thermometer sink to  $-100^{\circ}$ , and in successive experiments to  $-244^{\circ}$  and  $-352^{\circ}$ . Surprized at so unexpected an event, he drew the instrument out of the mixture, and carefully examined its bulb, to see if it had received any injury; but he found it perfectly entire, and perceived a much more unexpected phenomenon, that the quicksilver was fixed, and remained immoveable above 12 minutes. On repeating the same experiment with another thermometer, graduated no lower than  $-220^{\circ}$ , all the mercury sunk into the ball, and became solid as before, not beginning to re-ascend till after a still longer interval of time.

From these appearances the professor very justly concluded, that the quicksilver in both instruments had been fixed or frozen by the cold; but as the evidence was not yet complete, he only ventured to propose the congelation of mercury as a probable truth, at the next meeting of the Academy held 3 days afterwards; and in the mean time was making preparations to acquire more palpable proofs of the fact. The thermometers ordered with this view were not ready till the 25th of December O. S. when, in company with the celebrated *Æpinus*, professor of physics, he performed the experiment with similar materials, and as soon as he found the quicksilver immoveable, broke the bulb of his thermometer. Now all his doubts were removed; he obtained a solid shining metallic mass, which extended under the strokes of a pestle, in hardness rather inferior to lead, and yielding a dull dead sound like that metal. Professor *Æpinus* was occupied at the same time in similar experiments, employing both thermometers and simple tubes of a large bore; with which last he remarked, that the quicksilver in them fell sensibly on freezing, and assumed a concave surface; also, that the congealed pieces would sink in fluid mercury; all evident proofs of its great contraction. These observations were frequently repeated during the winter, with some variety in the circumstances and phenomena, by Prof. Braun, and many other persons.

When the season for experiments requiring cold was past, Prof. Braun employed himself in drawing up a general account of such as he had then made, which he communicated to the Petersburg Academy, Sept. 6, 1760, O. S. and printed soon afterwards as a separate dissertation. Five years afterwards, Prof. Braun again addressed the public on the same subject, under the title of "Supplements" to his former dissertation. Here he declares, that since the first dis-



covery he has suffered no winter to elapse without making similar experiments, and never failed of success in freezing the quicksilver, whenever there was a proper degree of natural cold, which he states at  $-10^{\circ}$ , in order for the experiment to be complete, though some commencement of congelation might be perceived when the temperature of the air is as high as  $+2^{\circ}$ . He confirms all his former observations, and adds many others to illustrate them; among which two are very important, as coming nearer than any yet known to ascertain the real contraction that quicksilver suffers in becoming solid. At the same time it must be confessed, he has not rectified any of his former mistakes: he retains the same groundless opinions relative to the freezing point of the quicksilver, the prodigious cold generated by his mixtures, and the explanation of various phenomena, which depend on very different principles, from those to which he assigns them.

The general state of M. Braun's experiments is, that with the above-mentioned frigorific mixtures, and once, when the natural cold was at  $-28^{\circ}$ , with rectified spirits and snow, he congealed the quicksilver, and discovered most of its properties in a solid state, especially that it is a real metal, which melts with a very small degree of heat. But not perceiving the necessary consequence of its great contraction in freezing, though aware of the fact, he perpetually confounded the diminution of its volume from this cause with that which is simply the effect of cold. Hence he considered, as the commencement of congelation, what was, in reality, its extreme term, or the utmost contraction which the whole would suffer in becoming solid. To this, indeed, he scarcely ever attained, owing to the various impediments that occurred from adhesion of the quicksilver in the thermometrical tube, hollows left in the bulb as it froze, portions of the mercury remaining uncongealed, and many other causes. In his supplementary treatise, the professor engages to continue his researches, and to lay the result of them before the academy, if they should lead to any thing new. But he did not live to accomplish his design, as he died the year following.

It was not till the year 1774, that Mr. Braun's assertions received any sort of confirmation out of Russia, and then by a mode of experiment which did not seem to promise much success. M. J. F. Blumenbach, then a student of physic, and afterwards professor of medicine, at Gottenburg, observing the intense cold that prevailed there in January that year, took the opportunity of exposing some quicksilver to its action. On the 11th. of Jan. M. Blumenbach, "at half after 5 in the evening, put 3 drams of quicksilver in a small sugar-glass, and covered it with a mixture of equal parts of snow and Egyptian sal ammoniac. This mixture was put loose into the glass, so that the quicksilver lay perfectly free, being only covered by it as with pieces of ice; the whole, together with the glass, weighed somewhat above an ounce. He hung it out at a window. 3



stories high, on a small roof facing the west, so that the glass was freely exposed to the north-west; and he mixed with the snow on which it stood 2 drams more of sal ammoniac. The snow and sal ammoniac in the glass soon froze in the open air to a mass like ice: no sensible change however appeared in the quicksilver that evening; but at 1 in the morning it was found frozen solid. It had divided into 2 large and 4 small pieces; of the former, one was hemispherical and the other cylindrical, each seemingly rather above a dram in weight; the 4 small bits might amount to half a scruple. The spirit of wine, in an excellent thermometer made by Brander, stood at this time  $10^{\circ}$  under 0 of Fahrenheit's scale, which was the cold of Upsal in 1740. Next morning, the 12th, about 7 o'clock, the larger hemisphere began to melt, perhaps because it was most exposed to the air, and not so near as the others to the sal ammoniac mixture which lay beneath. In this state it resembled an amalgam, sinking to that side on which the glass was inclined, but without quitting the surface of the glass, to which it was still firmly congealed; the 5 other pieces had not yet undergone any alteration, but remained frozen hard, as in the night. Towards 8 o'clock the cylindrical piece began to soften in the same manner as the former, and the other 4 soon followed. About 8 they fell from the surface of the glass, and divided into many fluid shining globules, which were soon lost in the interstices of the frozen mixture, and re-united in part at the bottom, being now exactly like common quicksilver. This whole experiment remains involved in such obscurity, that some persons have supposed the quicksilver itself was not frozen, but only covered over with ice; to which opinion, however, there are great objections. It is worthy of remark, that Gottingen, though situated in the same latitude as London, and enjoying a temperate climate in general, becomes subject at times to a great severity of cold. This of the 11th of January, 1774, is one instance. There are others when the thermometer sunk there to  $-12^{\circ}$ ,  $-16^{\circ}$ , or  $-19^{\circ}$ ; and at Cattlenburg, a small town about two German miles distant, to  $-30^{\circ}$ . By watching such extraordinary occasions, experiments on the freezing of quicksilver might easily be performed in many places where the possibility of them is at present little suspected. The cold observed at Glasgow in 1780 would have been fully sufficient for that purpose, viz.  $-23^{\circ}$  on the snow,  $-14^{\circ}$  in the air.

Dr. Blumenbach's description of the solid quicksilver differs so much from Prof. Braun's, with respect to its colour and general appearance, as to require a particular explanation. Their disagreement, Dr. B. imagines, was occasioned by a diversity in the circumstances of their experiments. Quicksilver crystallizes in becoming solid. In this property it resembles other metallic substances, as appears from many facts, and is elegantly exemplified in those curious cups formed by exposing proper masses of melted metal to the cold air till the outer part be



sufficiently hardened to constitute a solid coat, and then letting out the internal fluid part, so as to leave a hollow in the middle. This cavity is found every where beset with metallic crystals, scarcely yielding in beauty and regularity to the finest configurations of salts. In like manner, with regard to quicksilver, Professor Braun himself observed, that whenever it had congealed but imperfectly, and the fluid part was poured off, the solid surface which came in view was extremely rough, as if composed of many small globules. One of Mr. Hutchins's late observations exceedingly illustrates this matter; for he remarks, that when the fluid mercury was decanted off, in his 10th experiment. "the internal surface of the frozen quicksilver showed very uneven, with many radii going across, some of which had heads resembling pins." Now in Professor Blumenbach's experiments, the quicksilver lying loose, except the flat side that touched the glass, could crystallize without impediment, and hence assumed a rough, and consequently a dead white surface; whereas in those made by Mr. Braun, with tubes and thermometers, the metal being so much confined by the smooth glass, its surface was rendered of a high polish, not distinguishable in point of splendour from that of fluid mercury. Perhaps also, M. Blumenbach's quicksilver might have been made to look duller by some dirt or moisture collected on it from the sal ammoniac and snow.

In Jan. and Feb. 1775, Mr. Hutchins twice froze quicksilver at Albany Fort, Hudson's Bay; and in the first of these experiments, having broken his thermometer, he found that the metal flattened by a fall of about 6 inches, bore to be hammered, gave a dull sound like lead, and was finely polished on the surface. The account of these experiments was published in the Philos. Trans. vol. 66. As Mr. Hutchins adopted exactly the method of Professor Braun, he observed the same phenomena, encountered the same difficulties from the sticking of the quicksilver in the tube, and cracking of his thermometer, and was equally at a loss with regard to the point of congelation. Still, however, this was the fullest confirmation that M. Braun's Dissertations had ever yet received; and it may be considered as a prelude, by which Mr. Hutchins acquired the experience that enabled him to succeed so perfectly in his last most decisive and satisfactory experiments, viz. at the same place in the year 1781, as published in the Philos. Trans. of the year 1783, and in the present volume of these *Abridgments*. The preceding experiments had done little more than prove that quicksilver might be rendered solid by cold, and show what sort of substance it was in that state. Nothing satisfactory had been ascertained with regard to its freezing point, or the degree of a thermometer at which it ceases to be a melted and becomes a solid metal. It must not be supposed however, that the gentlemen who were engaged in these researches neglected such a principal object of inquiry; on the contrary,



Professor Braun himself took great pains to investigate it, but, for want of perceiving the consequences of the metal's great contraction in becoming solid, went very wide of the truth. This source of error did not escape the penetration of other philosophers, several of whom declared their opinion that the degree of cold necessary for the congelation of quicksilver could hardly be determined by freezing a thermometer filled with that fluid. But Mr. Cavendish and Dr. Black were the gentlemen who suggested an adequate method of obviating the difficulty, so as to ascertain the point in question with certainty and precision. Reasoning on the well-known fact, that a quantity of water continues at the same temperature from the moment it begins to freeze till the whole is become solid, they very justly concluded that the same would hold good with regard to quicksilver; and Mr. Cavendish confirmed this inference by experiments with metals of easy fusion, in which he found a thermometer keep at the same degree all the time they were passing from a fluid to a solid state. Hence it was proposed, that a small thermometer should be placed in some quicksilver to be frozen; which sinking pretty regularly till the congelation began, and remaining stationary till it should be complete, would thus show the degree of cold at which this effect takes place.

Though the methods proposed by Mr. Cavendish and Dr. Black were essentially the same, yet there was some difference in the apparatus they recommended; and as the former gentleman got his executed in London and sent out to Hudson's Bay, it was that which Mr. Hutchins employed in performing most of his experiments. These have not only confirmed the preceding observations relative to the solid state into which quicksilver can be brought by cold, its metalline splendour and polish when smooth, its roughness and crystallization where the surface was unconfined, its malleability, softness, and dull sound when struck; but have also clearly demonstrated, that its point of congelation is no lower than  $-40^{\circ}$ , or rather  $-39^{\circ}$ , of Fahrenheit's scale; that it will bear however to be cooled a few degrees below that point, to which it jumps up again on beginning to congeal; and that its rapid descent in a thermometer through many hundreds of degrees, when it has once past the above-mentioned limits, proceeds merely from its great contraction in the act of freezing. These and the other consequences reducible from Mr. Hutchins's experiments have been so exactly pointed out in the present volume by Mr. Cavendish, the real author and first mover of the whole business, that nothing remains but to add a few supplementary remarks.

Though in two of Mr. Hutchins's thermometers the quicksilver sunk exceedingly low, to  $-450^{\circ}$  or near  $-500^{\circ}$ , there is reason to believe he did not in any instance obtain the extreme term of contraction, since Professor Braun, in some of his last experiments, brought the mercury in one thermometer to  $-544^{\circ}$ , and



in another to  $-556^{\circ}$ . Hence it would seem, that Mr. Hutchins had always some part of the quicksilver left unfrozen, or some vacuity remaining, either in the stem or the ball of his instruments; and as no objection appears against those experiments of M. Braun's, we must conclude, that quicksilver, in becoming solid, contracts about a 23d of its whole bulk. Among the numerous improvements in natural knowledge which have been made within a short period of years, perhaps none tends to illustrate more phenomena of nature than the late discovery, that a considerable quantity of heat disappears when bodies pass into a state of fluidity or elastic vapour, and re-appears when they are converted back again to their original condition. This remarkable effect of such changes, it seems, was first observed at Glasgow, about 20 years ago, by Dr. Black and Mr. Irwin, who endeavoured to determine its most material circumstances by various experiments. Since that time Dr. Black has constantly taught it in his chemical lectures; and considering the heat which disappears as still remaining in the fluid or vapour, but deprived for the time of its property of being communicated to other bodies, and thereby becoming sensible, he calls it latent heat, a term sufficiently expressive of his manner of conceiving the fact.

In the year 1772, Professor Wilcke inserted, in the Transactions of the Royal Acad. of Sciences at Stockholm, a paper professedly on the subject of the cold produced by snow in melting. He seemed not at all acquainted with what Dr. Black had done, but speaks of it as his own discovery, originating in an accidental attempt to melt away a quantity of snow by the affusion of hot water; when he found the process go on so slowly, and so little effect produced, that he determined to investigate the cause of so unexpected an event. After a series of experiments with this view, he came to the following conclusion; that snow, in melting, constantly absorbs a certain and equal quantity of heat, which is employed entirely in giving it fluidity. Two principal methods have been adopted to prove this loss of heat; one, by adding ice at the freezing point to a certain proportion of water at a known degree of heat, and observing how much the temperature of the mixture comes out below that which should have resulted according to the common laws of the distribution of heat among bodies; the other, by observing how much faster water near the freezing point acquires sensible heat, than an equal quantity of ice melting under similar circumstances. It is obvious, that both these methods tend not only to prove the fact, but likewise to discover the quantity of heat so absorbed; and that the latter also, if the operation be reversed, will show the quantity of heat evolved, when a fluid congeals or becomes solid. In this way Dr. Black estimates the heat in question to be equal to 140 degrees on Fahrenheit's scale; M. Wilcke, by a great variety of experiments with different proportions of snow and water, brought it out pretty uniformly



about 130; and Mr. Cavendish finds it amount to 150, and chooses to call the process a generation of heat or production of cold.

The account of Mr. Hutchins's first success at Hudson's Bay was read before the Royal Society at the commencement of the severest winter that had been known for many years in Europe. Two gentlemen of different countries embraced this opportunity to attempt the congelation of quicksilver. The first was Dr. Lambert Bicker, secretary to the Batavian Society at Rotterdam, who, on Jan. 28, 1776, at 8 in the morning, made an experiment to try how low he could reduce the thermometer by artificial cold, the temperature of the air being then  $+2^{\circ}$ . He could not bring the mercury lower than  $-94^{\circ}$ , at which point it stood immoveable; and on breaking the bulb he saw with certainty that the outer part of the quicksilver had lost its fluidity, and was thickened to the consistence of an amalgam: it fell out of the bulb in little bits, which bore to be flattened by pressure, without running into globules like the inner fluid part. Next day, when the thermometer stood at  $+8^{\circ}$ , he repeated the experiment with all possible exactness, after M. Braun's manner; but could not obtain a greater descent of the mercury than to  $80^{\circ}$  under 0, and did not again break his thermometer.

The other gentleman who tried the effect of this severe cold in 1776 on mercury was Dr. Ant. Fothergill, at Northampton, and the account of his experiment may be seen in the Philos. Trans. for that year. His frigorific mixture appears to have been made with the vitriolic acid; and the natural cold of the air at Northampton that day, the 30th of January, was  $+9^{\circ}$ . It is scarcely possible to determine how far he succeeded. The quicksilver of his thermometer sunk into the bulb, and it, as well as some in a phial, contracted what Dr. Fothergill calls a film on the top; but unless the scale of his instrument went below  $-40^{\circ}$ , or some solid crystals were formed, such as M. Braun and others observed at the commencement of congelation, nothing can be collected with certainty from this experiment.

Dr. B. next notices some attempts made at Petersburg to freeze pure quicksilver by Dr. Mat. Guthrie, F.R.S. physician to the Cadet Corps of nobles there. From his experiments Dr. G. infers that that metal, when quite pure, never has been nor can be congealed: but the errors he seemed to have fallen into are here refuted by Dr. Blagden. He further adds also, possibly the point of congelation may not be exactly the same in all quicksilver under all circumstances. Foreign admixtures may occasion a difference in this respect; and it does not follow, that the effect of such, in certain proportions, must necessarily be to make the mercury congeal sooner, since, in the case of the fusible metal, the melting point of tin is brought lower by the addition of 2 metallic substances, both of which separately require a stronger heat than it for their fusion. But as quicksilver bears to



be cooled some degrees below its freezing point, before it begins to form solid crystals, the phenomenon in question may depend on that circumstance: for if, from whatever cause, the mercury in the thermometer should begin to congeal as soon as it was cooled down to  $-39^{\circ}$  or  $-40^{\circ}$ , while that which surrounded it would sustain a cold of  $-43^{\circ}$  or  $-44^{\circ}$  without becoming solid; it is evident that the whole of the former might be congealed, and yet no part of the latter, though the real freezing point of both were the same, that is, though the surrounding quicksilver as soon as it came to shoot its crystals would rise immediately to  $-39^{\circ}$ , the point at which that in the thermometer froze.

As this is undoubtedly the most obscure part of our knowledge relative to the congelation of quicksilver, Dr. B. endeavoured to illustrate it by some experiments on the freezing of water. The purest water he could obtain bore to be cooled to  $+21^{\circ}$ , no less than  $11^{\circ}$  below the temperature to which it instantly rose as soon as the crystals of ice shot through it. This was distilled water very recently boiled: it is a mistake, therefore, that boiling necessarily renders water not so capable of being cooled below the freezing point. In proportion as the water was less pure, it seemed to congeal the sooner; and the kind of impurity which had the most effect appeared rather to be extraneous matter diffused through the water, so as to trouble its transparency, than such as was chemically dissolved in it.\* The smallest particle of ice also, whenever the water was below the freezing point, either added from without, or by any means formed in it, would instantly cause a crystallization, by which the whole came immediately up to  $+32^{\circ}$ . Likewise a crack in the bottom of the containing glass vessel effectually prevented the water from being cooled below the freezing point, as ice constantly formed on the bottom, perhaps in consequence of the early generation of some minute portions of it in the crack. But independently of these circumstances, neither stirring, agitation, a current of fresh air on the surface, nor the contact of any extraneous body not colder, would cause the water to shoot into ice, even after it was cooled many degrees below the freezing point, notwithstanding the repeated assertions of authors to the contrary.

This account of mercurial congelation by artificial means would remain incomplete, Dr. B. says, were he not to mention that at Hampstead, on the 26th of February last (1783), the temperature of the air being then above  $+20^{\circ}$ , Mr. Cavendish, by an ingenious artifice of diluting the nitrous acid to a proper degree, sunk the quicksilver in his thermometer to  $110^{\circ}$ , and consequently froze it in part. He then interrupted the experiment to try the cold of his frigorific mixture by a

\* This appears to be the reason that boiling has been thought to render water incapable of being cooled below the freezing point. In most kinds of water, the application of heat occasions the precipitation of earthy substances which were before held in solution: hence the water comes to be in the state of having extraneous matter diffused through it, and therefore readily congeals.—Orig.



spirit thermometer, and found it nearly as great as Mr. Hutchins had ever produced at Hudson's Bay, that is, about equal to  $-45^{\circ}$  of a standard mercurial thermometer.

The subsequent part of this narrative demonstrates, that quicksilver has very frequently become solid by natural cold; that in a few instances the effect was so palpable and obvious as to strike with immediate conviction; but that in most it has never been even suspected till the present time, the strange appearances which often occurred being imputed by the observers to any other rather than the real cause, though they are now found to carry with them a force of internal evidence which establishes the truth beyond all doubt. In enumerating these facts, Dr. B. continues to pursue a chronological order. They are in general of such a kind as could scarcely become an object of attention, till thermometers had acquired some degree of accuracy. This did not happen till near the year 1730, and the first observations which prove the freezing of quicksilver were made within 4 or 5 years of that period: so intimately are improvements in philosophy connected with the perfection of instruments!

When the Empress Anna Iwanovna had ascended the throne of Russia, she resolved to carry into execution one of the favourite ideas of her uncle, Peter the Great, by sending out proper persons to explore the different parts of her vast dominions, and inquire into the communication between Asia and America. Three professors of the Imperial Academy were chosen for this expedition; Dr. John George Gmelin, in the department of Natural History and Chemistry; M. Gerard Frederic Muller, as general Historiographer; and M. Louis de L'Isle de la Croyere, for the department of Astronomy; draughtsmen and other proper assistants were appointed to attend them. In the summer of the year 1733 they departed from Petersburg; and though a principal object of their commission was unavoidably neglected, from the difficulty of transporting the necessary supplies of provisions to Kamchatka, yet it was the 10th year of their travels before the survivors returned to Europe. The thermometrical observations made in the course of this memorable survey of the Russian empire were communicated to the world by Professor Gmelin; and they prove in the most remarkable manner the excessive rigour of the Siberian climate. It was at Yeniseisk, lat.  $58\frac{1}{2}^{\circ}$  N. and long.  $92^{\circ}$  E. of Greenwich, that M. Gmelin first observed such a descent of his thermometer as we now know indicated the mercury to have been frozen. This happened in the winter of 1734 and 1735. Here, says the professor, we first experienced the truth of what various travellers have related, with respect to the extreme cold of Siberia; for, about the middle of December, such severe weather set in, as, we are certain, had never been known in our time at Petersburg. The air seemed as if it were frozen, with the appearance of a fog, which did not suffer the smoke to ascend as it issued from the chimnies. Birds fell



down out of the air as if dead, and froze immediately, unless they were brought into a warm room. Whenever the door was opened, a fog suddenly formed round it. During the day, short as it was, parhelia and haloes round the sun were frequently seen, and in the night mock moons and haloes about the moon. Finally, our thermometer, not subject to the same deception as the senses, left us no doubt of the excessive cold; for the quicksilver in it was reduced [on the 5th of Jan. O. S.] to  $-120^{\circ}$  of Fahrenheit's scale; lower than it had ever hitherto been observed in nature. Little did Mr. G. conceive that, though his thermometer was not subject to the same deception as the senses, it was yet subject to another source of error which defeated all his conclusions: for as soon as the cold became sufficiently great to produce any congelation of the quicksilver, it ceased to be a measure of the temperature; instead therefore of  $120^{\circ}$  below O, the cold most probably did not exceed the point of mercurial congelation, or  $-39^{\circ}$ , but by a very few degrees, the great descent of the quicksilver, as it depended on its contraction in the act of freezing, only affording a proof that it had really suffered this change.

The next instance of mercurial congelation to be found in Gmelin's journal exhibits a very striking example of the force of prejudice. It happened at Yakutsk, lat.  $62^{\circ}$  N. and long.  $130^{\circ}$  E. in the winter of 1736 and 1737, and is thus related by the professor. "This winter was unusually mild here, yet we suffered at times very severe cold, being frost-bitten in a sledge within the space of 6 minutes, notwithstanding all our precautions. One day also, a certain person, who has some reputation in the learned world on account of his observations in natural philosophy, informed me by a note, that the quicksilver in his barometer was frozen. I hastened immediately to his house, to see this hitherto incredible wonder of nature. Not feeling by the way the same effects of cold as I had experienced at other times in less distances, I began, before my arrival, to entertain suspicions about the congelation of his quicksilver. In fact, I saw that it did not continue in one column, but was divided in different places as into little cylinders which appeared frozen, and in some of these divisions between the quicksilver I perceived an appearance like frozen moisture. It immediately occurred to me, that the mercury might have been cleaned with vinegar and salt, and not sufficiently dried. The person acknowledged it had been purified in that manner. This same quicksilver, taken out of the barometer and well-dried, would not freeze again, though exposed to a much greater degree of cold, as shown by the thermometer. We were assured by the inhabitants, that the severest cold of this winter did not approach what they had suffered in other years; and yet the thermometer fell several times to  $72^{\circ}$  below O of Fahrenheit's scale, which would be thought, in Germany at least, a very intense frost." The gentleman to whose observation Dr. Gmelin here shows so little respect, says



Dr. B., seems to have been no other than one of his associates in the commission, M. de L'Isle de la Croyere, probably the first person on earth who saw quicksilver reduced to a solid form by cold, and ventured to credit the testimony of his senses. As to the objection, that the same mercury did not freeze with a greater degree of cold, it is of no avail; for M. Gmelin had not any other means of estimating this but by the descent of his thermometer, which could not be depended on farther than to the point of mercurial congelation. The absurd idea, that quicksilver appears to congeal in consequence of water it contains, was derived, it seems, originally from a whim of Raymond Lully's. It has been the usual refuge of those gentlemen who thought proper to deny that mercury could be made solid by cold; but it is too destitute of support to merit confutation.

Another set of observations, in the course of which the mercury frequently congealed, were made by Dr. Gmelin at Kirenga fort, lat.  $57\frac{1}{2}$  N. long. 108 E. in the winter of 1737 and 1738. His thermometer on different days stood at  $-108^{\circ}$ ,  $-86^{\circ}$ ,  $-100^{\circ}$ ,  $-113^{\circ}$ , and several intermediate degrees. Some extraordinary appearances, which very much perplexed him in these observations, not only admit of a ready solution from Mr. Hutchins's determination of the freezing point of quicksilver, but also confirm it with wonderful precision. Another instance occurred at Kirenga fort a few days afterwards, explicable in the same manner. On Dec. 29, o. s., Dr. Gmelin found his thermometer, which had been standing at  $-40^{\circ}$  early in the morning, sunk down to  $-100^{\circ}$  at 4 in the afternoon. He subjoins the following remark. "I observed some air in the thermometer, separating the quicksilver for the space of about 6 degrees. Yesterday evening I took notice of a similar appearance, except that the air was not then collected into one place, but lay scattered in several. I considered it as an accidental fault in the instrument, and attempted to expel it by means of a steel wire, but could not bear the cold. In the barometer also some very small air-bubbles were perceived. Next morning only a very few minute air-bubbles remained in the quicksilver of the thermometer, which had then risen to  $-44^{\circ}$ , and not the least vestige of them was to be seen in the barometer." It cannot be doubted, but these appearances proceeded from a congelation of the mercury in Gmelin's instruments. On several other occasions Dr. G. observed that the quicksilver in his thermometer looked as if air was interspersed in it. Whenever this happened, it always subsided many degrees below what we now understand to be the point of mercurial congelation. The professor, totally at a loss to explain such a phenomenon, imputes it sometimes to a fundamental fault in his instrument, but which he could never discover, and at other times to an imaginary effect of the intense cold, in expelling or extricating air from the pores of the quicksilver, to be absorbed as the cold abated. On the 9th of January 1738, o. s., the mer-



cury sunk at once to  $-114^{\circ}$ , after having been stationary two whole days at  $-45^{\circ}$ . The last observations of M. Gmelin's, in which quicksilver froze, were made on his return homeward in a part of Siberia, much nearer the confines of Europe. During the month of December 1742, as he was passing over that branch of the Ural or Riphæan mountains which runs between Verchoturie and Solikamsk, about the 59th degree of N. lat. and scarcely 60 degrees E. of Greenwich, his thermometer sunk to  $-41^{\circ}$ ,  $-70^{\circ}$ , and at length into the bulb, though it was graduated to  $96^{\circ}$  below 0. The same appearance of air-bubbles which he had so frequently remarked in such great descents of the thermometer, puts it beyond doubt that the quicksilver was frozen. This event furnished a very striking proof of the force of habit in reconciling men to hardships, which in their common course of life are thought intolerable. Professor Gmelin, who had now been 9 years in Siberia, not only bore to travel in this excessive cold, but also, in order to ascertain the height of the mountains he traversed, employed himself in observing a barometer, while the quicksilver was freezing in his instruments!

These are the principal of Dr. Gmelin's thermometrical observations. He collected many more, part of which were destroyed by fire or other accidents, and the remainder seem to contain no further information. They were considered by him as demonstrating the cold of Siberia to exceed that even of the most northern parts of Europe near  $100^{\circ}$ , an opinion which has since been almost universally adopted; whereas we have in fact no proof that the difference of climate amounts to so much as the variation between one winter and another. At Yeniseisk, where the cold was so intense in 1735, it does not seem to have ever been sufficient to freeze a thermometer in the winter that M. Gmelin spent there 4 years afterwards; and it will soon be shown that quicksilver has congealed more than once in Europe. All that we are authorized to conclude therefore, with respect to the Siberian climate, is, that the cold there not unfrequently exceeds the degree indicated by  $-39^{\circ}$  of a standard mercurial thermometer.

About the time when the quicksilver was exhibited frozen to Prof. Gmelin, near the extremity of Asia, without overcoming his prepossession, M. Maupertuis and his associates saw the liquor congeal in their spirit thermometer at Tornea in Lapland. Their mercurial thermometer sunk at the same time to  $-37^{\circ}$  of M. de Reaumur's scale; which, if the instrument was exactly graduated according to that philosopher's original idea, would undoubtedly show that the quicksilver froze, as it corresponds with  $-51^{\circ}$  of Fahrenheit. But the inaccuracies in constructing M. de Reaumur's thermometers have been so great, that I think no dependance can be placed on this observation, especially as it does not appear to have been attended with any extraordinary phenomenon. The same objection holds good with regard to the observations made by M. Gautier at



Quebec, from the year 1743 to 1749, an extract from which is inserted in the *Memoirs of the French Academy of Sciences*. The account given of his thermometer is too indefinite to allow any certain inference to be drawn; but as the quicksilver several times contracted so much as to leave a visible vacuity in the top of the bulb, and the scale seems to have reached near to its point of congelation, it is rather probable that it actually froze. If so, Quebec, situated in lat.  $47^{\circ}$ , is the most southern place in which such a great degree of natural cold has hitherto been observed.

We come now to an instance of what, however often it may have happened, has hitherto never been suspected, the congelation of quicksilver in Europe by natural cold. The observations which prove this fact are recorded in the *Transactions of the Royal Academy of Sciences at Stockholm*, whence Dr. B. has extracted the following account. In January 1760, the weather was remarkably cold in Lapland. On the 5th of that month different thermometers sunk to  $-76^{\circ}$ ,  $-128^{\circ}$ , or lower. Again, on the 23d and following days, they fell to  $-58^{\circ}$ ,  $-79^{\circ}$ ,  $-92^{\circ}$ , and below  $-238^{\circ}$  into the ball. This great descent of the mercury was observed in 4 places, Tornea, Sombio, Iukasierf, and Utsioki, all situated between the 65th and 70th degrees of N. lat. and the 21st and 28th of eastern longitude, by M. Andrew Hellant, economical inspector of Lapland, whose remarks on the phenomenon afford of themselves sufficient evidence, that the quicksilver was frozen.

Several reflections present themselves on the perusal of his observations. The phenomena fairly show, that there was a sufficient degree of cold to congeal the quicksilver in Mr. Hellant's thermometers, which sometimes sunk regularly into the bulb, but commonly stuck fast in the tube till it was heated by the sun, the fire, or a warm room, and thus made to subside. The continuance of this cold was very remarkable; it lasted no less than 3 days, with sufficient intensity to freeze mercury; a circumstance almost unparalleled any where, and the more extraordinary, because M. Hellant, during 23 years that he had made observations in Lapland, never before saw the thermometer so low as to indicate a congelation of the mercury. But it was not in Lapland alone that the season was uncommonly severe. At this same time the frost was nearly, if not quite, intense enough at Petersburg to freeze quicksilver, as appears from the remarks of M. Braun, who was then engaged in his experiments. And it is a curious coincidence of events, that on the very day when the congelation of mercury by artificial means was first clearly established in Russia, Nature should be performing the same operation before the eyes of an attentive and philosophical observer in a neighbouring kingdom, who yet had not sufficient sagacity to divine her secret.

Early in the spring of 1761, the Abbé Chappe D'Auteroche, in his journey to Tobolsk for observing the transit of Venus, passed through Solikamsk, a



town of Siberia, situated in  $59\frac{1}{2}^{\circ}$  N. lat. and  $57^{\circ}$  E. of Greenwich. On this occasion he takes notice, that the thermometer had sunk there the preceding winter to  $-124^{\circ}$ ; which, if the general stile of the Abbé's remarks will allow sufficient dependance to be placed on it, would necessarily show that the quicksilver was then frozen.

M. Erich Laxmann, late professor of mineralogy and chemistry at Petersburg, was resident in 1765 at Barnaul in Siberia, lat.  $53^{\circ}$  N. and long.  $81^{\circ}$  E. as minister to the German congregation of the Kolyvan province. On the first day of that year, he saw the thermometer down so low as  $-58^{\circ}$ ; whence it is probable, that some part at least of the quicksilver was congealed.

The benefits accruing from the travels of learned men could not escape the penetration of the wise Empress Catharine. Soon after her establishment on the throne, she ordered an expedition of the same nature as that in which Gmelin had been engaged above 30 years before. Among the gentlemen who undertook this 2d philosophical survey of the Russian empire, was Dr. Peter Simon Pallas. The journal of his travels is published by himself in the German language, and comprehends a rich store of curious and useful information. In general his winters were not spent in the coldest parts of Asia; twice however he resided at Krasnoyarsk lat.  $56\frac{1}{2}^{\circ}$  N. long.  $93^{\circ}$  E. and the last time, in 1772, had an opportunity of witnessing the most remarkable instance of the congelation of mercury by natural cold that is yet known to the world. "The winter," says M. Pallas, "set in early this year, and was felt in December with uncommon severity. On the 6th and 7th of that month happened the greatest cold I have ever experienced in Siberia; the air was calm at the time, and seemingly thickened, so that, though the sky was in other respects clear, the sun appeared as through a fog. I had only one small thermometer left, on which the scale went no lower than  $-70^{\circ}$ ; and on the 6th in the morning I remarked that the quicksilver in it sunk into the ball, except some small columns which became solid and stuck fast in the tube. By the temperature of a room not much warmed, into which I brought the thermometer from the gallery of my house, these congealed columns immediately fell down; but it was more than half a minute before the mercury came into motion out of the ball. I repeated this experiment frequently, and always with similar success, sometimes one and sometimes more threads of frozen quicksilver remaining behind in the tube. When the ball of the thermometer, as it hung in the open air, was warmed by being touched with the fingers, the quicksilver rose; and it could plainly be seen, that the solid frozen columns stuck and resisted a good while, and were at length pushed up with a sort of violence. In the mean time I placed on the gallery on the north side of my house about a quarter of a pound of clean and dry quicksilver in an open bowl; within an hour I found the edges and surface of it frozen solid, and some



minutes afterwards the whole was condensed, by the natural cold, into a soft mass very much like tin. While the inner part was still fluid, the frozen surface exhibited a great variety of branched wrinkles; but in general it remained pretty smooth in freezing, as did also a larger quantity of quicksilver which I afterwards exposed to the cold. The congealed mercury was more flexible than lead; but on being bent short it was found more brittle than tin, and when hammered out thin it seemed somewhat granulated. When the hammer had not been perfectly cooled, the quicksilver melted away under it in drops; and the same thing happened when the metal was touched with the finger, by which also the finger was immediately benumbed. In our warm room it thawed on its surface gradually, by drops, like wax on the fire, and did not melt all at once. When the frozen mass was broken to pieces in the cold, the fragments adhered to each other, and to the bowl in which they lay. Though the frost seemed to abate a little toward night, yet the congealed quicksilver remained unaltered, and the experiment with the thermometer could still be repeated. On the 7th of December I had an opportunity of making the same observations all day; but some hours after sunset a north-west wind sprung up, which raised the thermometer to  $-46^{\circ}$ , when the mass of quicksilver began to melt."

Before this observation of Dr. Pallas's, no person had seen or handled quicksilver frozen by natural cold, so as to submit the fact to the public with competent evidence; but the circumstances here related are so pointed and consistent, that even those who had doubted of M. Braun's experiments were now staggered, and began to believe. Indeed, it was scarcely possible to suppose any mistake, when Dr. Pallas had 2 whole days to repeat and vary the experiments at his leisure. But besides removing all doubts on the congelation of quicksilver, these observations tended to show, within certain limits, the degree of cold necessary for that effect. It was evident that the freezing point must be somewhere above  $-70^{\circ}$ , because the thermometer's graduation reached only so low, and yet some part of the mercury always congealed in the tube; and as the solid masses did not begin to melt till the thermometer rose to  $-46^{\circ}$ , that seemed to be nearly the point at which it passes from a solid to a fluid state, and very possibly was so on this instrument, a difference of several degrees being often found in thermometers so low down on the scale as  $-40^{\circ}$ , from inaccuracies in their construction.

The crystallization of quicksilver also became manifest on this occasion. Hence, when hammered out thin, it showed a granulated texture. The branched wrinkles too, which formed on its surface while it was congealing, could scarcely have proceeded from any other cause, and suggest a general idea of the manner in which it shoots. That quicksilver should crystallize so much more visibly than most other metals, will not appear surprizing, if we consider how little the



cold is below its freezing point. Such substances as require, in order to melt, a degree of heat much above that of our atmosphere, experience so great a change of temperature on being taken off the fire, that they become solid hastily, and as it were in confusion; whereas quicksilver, having never probably been exposed to a degree of cold much exceeding that of its melting point, its particles have had full leisure to arrange themselves regularly, in exact conformity to the laws of their mutual attractions. As in Prof. Blumenbach's and Mr. Hutchins's experiments, so here probably some slight roughness of the surface was occasioned by this crystallization; in consequence of which M. Pallas compared his frozen quicksilver to tin, rather than to bright silver, the appearance it always assumes when congealed in smooth glass.

Another property of quicksilver, very important to be known, was observed perhaps no where so distinctly as on this occasion at Krasnoyarsk: viz. its tendency to adhesion in freezing. Thus, Dr. Pallas says the fragments of the congealed mass stuck to each other, and to the bowl in which they lay. So likewise Mr. Hutchins found the frozen quicksilver adhering to his cylinders and gallipot; Professor Blumenbach to his glass vessel; and similar facts occurred to other observers. Hence the deceptions, so often mentioned, from the sticking of the mercury in the stems of thermometers. And this cause of error can scarcely ever fail to take place; for if quicksilver congealing in wide open vessels adheres to them wherever it touches, how can it be expected to remain loose when frozen in a narrow tube? Now, since quicksilver, under these circumstances, retains the same appearance as while fluid, from the polish given to its surface by the smooth glass, it is no wonder that such frequent mistakes have been made relative to the height of the thermometer, both in experiments with artificial cold, and in meteorological observations. At the same time it must be confessed, that such a tendency to adhere, in a metal which contracts so much in becoming solid, is not a little difficult to explain, unless we may suppose it to be the immediate effect of the crystallization. Quicksilver, with all its other qualities of a perfect metal, seems from Dr. Pallas's, and indeed most of the experiments, not to be completely malleable, but rather apt to break under the hammer. Perhaps it has never been sufficiently cooled to possess its metallic properties in perfection; for with respect to its melting point it may be considered as having always been hot, that is, heated near to fusion, a state in which other metals undergo a very sensible change in their properties. But when mercury congeals in vessels which confine its surface, it seems to become more malleable than under a loose crystallization.

Nearly 500 miles south-eastward of Krasnoyarsk is the town of Irkutsk, the capital of a Siberian province on the vast Baikal lake, and situated in lat. 52° N. and about the 104th degree of E. longitude. At the former of these places, the



cold recorded by Dr. Pallas began to abate on the 7th of December in the evening; but more than a day after, that is, on the 9th in the morning, it became so intense at Irkutsk as to freeze quicksilver. An account of this phenomenon, sent by Lieutenant-general Von Brill, the governor, was published by Dr. Pallas in his journal, and M. Georgi, one of the associates in this expedition, afterwards collected some further particulars. It appears, that about 4 in the morning, the quicksilver was found frozen in the barometer and thermometer, its upper surface being irregularly broken. In the former of these instruments the mercury stood at 28 inches 7 lines, and the broken appearance extended through a space of about 5 lines from the top downward: when it came to melt a few hours afterwards, it rose to 29 inches 7 lines, which difference of height was probably in part at least an effect of the contraction it undergoes in freezing, its greater specific gravity in the congealed state making it stand proportionably lower. In the thermometer, part of the mercury had stuck at  $-44^{\circ}$ ; and immediately under  $-59^{\circ}$  an empty space was left, equal to 11 degrees of the scale. This observation therefore determines almost precisely the freezing point; for none of the quicksilver could have adhered in the tube so high as  $-44^{\circ}$ , unless it had congealed before it sunk below this point, and consequently before the cold exceeded this degree. And that the mercury was really frozen became evident afterwards; for about 11 in the forenoon, as the air got warmer, it was found to have all subsided into the bulb, the small threads in the tube melting down into the vacuity left there, out of which it did not rise again till near 2 hours had elapsed.

As the cold of America is well known to exceed that which prevails under the same latitudes in Europe, we must expect to find quicksilver freezing spontaneously in parts of that continent which do not lie very far to the northward. Accordingly, besides the instance of Quebec formerly mentioned, this effect takes place frequently in Hudson's Bay, even at Albany fort, where the latitude is not one degree greater than in London. Mr. Hutchins, in his different situations at Hudson's Bay, has been constantly attentive to meteorological observations. During his former residence at York fort, situated near the middle of the Bay in lat.  $58^{\circ}$  N. he was not provided with any thermometer graduated more than 70 or 90 degrees below the cypher: but he remarked, that "the quicksilver frequently sunk into the bulb," especially after having been stationary at  $-55^{\circ}$  or  $-57^{\circ}$ , and that it afterwards used to ascend to "about  $-30^{\circ}$ , indicating a greater degree of heat than before it fell." These phenomena were clearly owing to its congelation, adhesion in the tube, and subsequent liquefaction as the air got warmer. When Mr. Hutchins went afterwards to Albany fort, and had procured instruments with more extensive scales, he observed the same appearances still more distinctly. His thermometers froze twice in the winter of 1774



and 1775, and 3 times in that of 1777 and 1778; and in every instance, except one, the mercury sunk hundreds of degrees just as the cold began to abate. The last of these observations is rendered remarkable by the descent of the quicksilver to  $-490$ , the greatest ever known by natural cold, and probably very near its extreme term of contraction by freezing. In 1782, also, Mr. Hutchins's thermometers, together with some quicksilver in a phial, again congealed in the open air, and exhibited similar phenomena, as appears from the account of his experiments.

Fortunately in these instances of intense cold at Albany fort, attention was paid not only to the mercurial thermometers, but likewise to one made of spirits, whose relative movement has been ascertained by comparison. This instrument, while the others were 3, 4, or almost 500 degrees below 0, never sunk farther than to a point which corresponds with  $-46^{\circ}$  of a standard mercurial thermometer. Hence it would have been easy to infer, both that the quicksilver actually congealed on these occasions, and that the degree of cold necessary for such an effect does not exceed  $-46^{\circ}$ . That the most intense cold of Hudson's Bay, during a series of several years, went so little below the point of mercurial congelation, well deserves to be noticed; and as it seems to be seldom greater in Siberia, at least in the parts visited by Gmelin or Pallas, the effects being not more violent, perhaps we are authorized to conclude, that the extreme of artificial cold, produced by snow and nitrous acid, corresponds pretty exactly with the extreme of natural cold in the most rigorous climates which can well be inhabited.

Again, to return to Europe. In the beginning of the year 1780, M. Von Elterlein, of Vytegra, froze quicksilver by natural cold, and sent an account of his experiment in a letter to the late Prof. Guldenstadt, then at Petersburg, to the following purport. "On the 4th of January, 1780, the cold having increased to  $-34^{\circ}$  that evening at Vytegra, I exposed to the open air 3 oz. of very pure quicksilver, in a China tea-cup, covered with paper pierced full of holes. Next day, at 8 in the morning, I found it solid, and looking like a piece of cast lead, with a considerable depression in the middle. On attempting to loosen it in the cup, my knife raised shavings from it as if it had been lead, which remained sticking up; and at length the whole separated from the bottom of the cup in one mass. I then took it in my hand to try if it would bend; it was like stiff glue, and broke into 2 pieces; but my fingers immediately lost all feeling, and could scarcely be restored in an hour and a half by rubbing with snow. At 8 o'clock a thermometer, made by M. Laxmann of the Academy, stood at  $-57^{\circ}$ ; by half after 9 it was risen to  $-40^{\circ}$ ; and then the 2 pieces of mercury, which lay in the cup, had lost so much of their hardness that they could no longer be broken or cut into shavings, but resembled a thick amalgam, which,



though it became fluid when pressed by the fingers, immediately afterwards resumed the consistence of pap. With the thermometer at  $-39^{\circ}$ , the quicksilver became fluid. The cold was never less on the 5th than  $-28^{\circ}$ , and by 9 in the evening it had increased again to  $-33^{\circ}$ . In the morning the wind was N. N. E. and afterwards N. W."

This experiment of M. Von Elterlein's deserves attention in many respects. It ascertains the freezing point of mercury with such wonderful exactness, from the melting of the solid pieces when the thermometer came up to  $-39^{\circ}$ , as to furnish a valuable corroboration of Mr. Hutchins's experiments, and at the same time very much to enhance our opinion of M. Laxmann's skill in the construction of instruments. When the thermometer was thought, early in the morning, to be standing at  $-57^{\circ}$  probably that part of the quicksilver being frozen adhered in the tube. The ductility of the solid metal must have been considerable, from its yielding to the knife in the form of shavings; yet, as in most other instances, it showed some degree of brittleness when force was applied to it in the mass. Crystallizing without impediment, it assumed an appearance which M. Von Elterlein rather compares to that of lead than of silver. Its tendency to adhesion became evident from the necessity of employing an instrument to separate it from the tea-cup; and its contraction in freezing was demonstrated by the depression observed in the middle of the solid mass. This single experiment therefore exemplifies, in a very beautiful manner, most of the properties hitherto discovered in quicksilver, when it passes from a fluid to a solid form.

Vytegra, or Witegorsk, is situated in lat.  $61^{\circ}$  N. and long.  $36^{\circ}$  E. on a river of the same name. It has acquired some celebrity from one of the many useful projects which occupied the active mind of Czar Peter the Great. He proposed to cut a canal from the river Vytegra which discharges itself into the lake Onega, to the river Kovsha which joins the Belosero, or White Lake, in order to form a communication between those two great bodies of water; but the undertaking was unfortunately interrupted by his death.

The last instance to be found of the congelation of quicksilver by natural cold, occurred the beginning of the year 1782, in Iemtland, one of the northern provinces of Sweden. M. John Törnsten, engineer-extraordinary, is the gentleman to whom we are indebted for this observation. His letter on the subject, dated from Brunflo in Iemtland, lat.  $63^{\circ}\frac{1}{2}$  N. and long.  $15^{\circ}$  E. is inserted in the Swedish Transactions for 1782, together with some remarks upon it by Professor Wilcke. "During 12 years," says M. Törnsten, "that I have resided here in Iemtland, the cold had never but once brought the thermometer so low as  $-36^{\circ}$ , till the last day of December, 1781, when it fell in the evening to  $-54^{\circ}$ . The following new-year's day it was sunk to  $-56^{\circ}$  at 8 in the morning, and by 10 to  $-62^{\circ}$ . Here it continued stationary several hours, but at half past 4 in



the afternoon it was observed at  $-116^{\circ}$ , and by 8 the same evening it had risen to  $-31^{\circ}$ . Though the quicksilver," continues M. Törnsten, "thus fell to  $-116^{\circ}$  on the first of January in the afternoon, I am of opinion that its descent ought not to be ascribed to a proportionable increase of cold, but on the contrary proceeded from the sudden change to milder weather, which came on that afternoon. For the preceding evening, when the thermometer was standing at  $-54^{\circ}$ , I remarked, that on bringing it into a warm room, the quicksilver fell on a sudden entirely into the ball, which was about 130 degrees below 0. This experiment I repeated several times with success, but observed the following difference, that if I had not kept the thermometer in the heat long enough for the quicksilver to begin to rise again after it had sunk into the ball, it never ascended above the 130th degree by continuing in the cold, but on being carried back into the warm room it contracted still more in the ball by a quantity which, however visible, could not be measured. On the other hand, if the instrument had been kept in the room till the mercury had risen above  $-54^{\circ}$ , it became stationary at that degree in the open air. Now, though I did not, on the 1st of January, bring the thermometer within doors before it had sunk of itself to  $-116^{\circ}$ , yet this fall likewise seems to have been occasioned by the change to milder weather which was then taking place. For at 8 in the evening, when the external cold was at  $-31^{\circ}$ , I found that hoar-frost formed on the ball and stem of the thermometer as before, on its being brought into a warm room; but the mercury did not sink, on the contrary it began immediately to rise. Some quadrupeds perished by the intense cold, and a great number of small birds were found dead."

M. Törnsten certainly judged right when he concluded, that the fall of the thermometer to  $-116^{\circ}$  rather indicated a diminution than an increase of the cold. Though he knew nothing of the cause, yet his observation led him to a just inference, in which he displayed more sagacity than M. Hellant on a similar occasion. All the phenomena which so much perplexed these gentlemen are explicable in the following manner. When the air becomes sufficiently cold to freeze quicksilver, that metal must be standing about  $-39^{\circ}$ , or in the common way of marking the boiling point, somewhere between  $-40^{\circ}$  and  $-50^{\circ}$ , in the tube of a thermometer exposed to it. As the small thread of mercury in the tube must be more easily affected by the cold, it will probably congeal before any other part, and stick fast about the above-mentioned degrees. The remainder of the mercury will then go on to freeze, and as it suffers such a great contraction in becoming solid, must leave a considerable vacuity in the bulb of any common thermometer. Consequently, when the cold, from whatever cause, comes to be less than is required for keeping the metal in a solid state, the small thread that was frozen in the tube immediately melts, and sinks down into the vacuity



of the bulb, where the whole mass remains, till by its gradual liquefaction it expands again into the tube, and becomes a just measure of the temperature. This agrees exactly with what M. Törnsten observed. In the evening of the 31st the quicksilver congealed in his thermometer, and part of it stuck in the tube at  $-54^{\circ}$ , but subsided into the vacuity left in the bulb, as soon as it was exposed to heat. When the instrument had been kept in the warm room till the quicksilver re-ascended into the tube, it froze and adhered again in the open air, and the same phenomena were repeated. If M. Törnsten be exact in saying it always became fast at  $-54^{\circ}$ , the circumstance is curious, and may have depended on some particular state of the tube in that part, or on the first shooting of the mercury after it had been cooled to a certain degree below its freezing point. But when the thermometer was carried back into the open air before any of the quicksilver had risen out of the bulb, the effect of the cold could not be to force it up into the tube, and therefore no such appearances were observed as in the former case. With regard to M. Törnsten's remark, that when the whole mass of quicksilver remained in the ball it still contracted on the application of heat, the fact is so improbable, and would be perceived with such difficulty, that I have no doubt but he was misled by some prepossession. In like manner on the 1st of January, when the thermometer, having been stationary some hours at  $-62^{\circ}$ , sunk in the afternoon to  $-116^{\circ}$ , it happened unquestionably from the melting and subsiding of a thread of frozen mercury, which had adhered in the tube of the instrument as high as the former degree. None of these effects could be produced when the thermometer had risen to  $-31^{\circ}$ , because the cold was not then sufficient to congeal the quicksilver. In this easy and simple manner, does our knowledge of the freezing point of mercury enable us to account for phenomena, which were thought so anomalous as to elude every kind of explanation. Even so lately as last year, one of the most eminent philosophers in Europe, Professor Wilcke of Stockholm, made a vain attempt to solve the difficulties by a strained application of his doctrine relative to the various specific quantities of heat in bodies, and their different attractions for the matter of heat. It would now be superfluous to add, that the real cold at Brunflo was by no means what the thermometer seemed to indicate, but probably very little exceeded  $-39^{\circ}$ , or the degree of mercurial congelation, had not M. Törnsten's observations been lately represented, even in this country, as exhibiting an instance of cold actually carried to such a disproportionate and enormous excess.

Thus is the history of the congelation of quicksilver, both by natural and artificial cold, brought down to the present period. All the facts collected are here delivered: probably however there may be others which have escaped, especially such as are very recent, or have never been published; but the number already found is greater than was expected on beginning the search. By such a



connected view of the different observations and experiments in any one branch of science, we are furnished with the best opportunity of discriminating what is certain from what is doubtful, and acquire as distinct ideas as the actual state of knowledge will admit. On the present subject of mercurial congelation, the conclusions have in general been noticed, as the premises occurred. Though Mr. Hutchins's experiments did not stand in need of any confirmation, yet still it is pleasant to see their principal result, the freezing point of quicksilver, established by such a body of collateral evidence as, taken together, is absolutely irresistible. But besides the information obtained relative to quicksilver itself, we have been able to correct several vulgar prejudices. The difference between cold climates no longer appears so prodigious, nor the resisting powers of animals and vegetables so astonishing and inconceivable. That extensive scale of heat, which represents its diminutions by artificial means as continued down so many hundreds of degrees below the greatest produced by nature, however specious in prospect, proves to be destitute of foundation. The use of quicksilver for thermometers is at length fully ascertained. From the boiling point, to  $39^{\circ}$  or  $40^{\circ}$  below 0, it must be considered as unexceptionable, all suspicion of its irregular contraction within those bounds being removed, by such a complete explanation of the cause on which its anomalous descent in the lower part of the scale depends. On this principle there might perhaps, adds Dr. B., be some propriety in constructing thermometers of mercury, to fix the cypher at the point of congelation, and thence reckon the degrees of heat upwards.

*XXII. Experiment relating to Phlogiston, and the seeming Conversion of Water into Air. By Joseph Priestley, L L. D., F. R. S. p. 398.*

This paper may be more advantageously consulted in the collection of Dr. Priestley's various writings on air and other branches of natural philosophy. It is the 1st article in the 6th or last volume of that collection, printed in 8vo. at Birmingham, 1786, where it is enriched with additional notes.

*XXIII. Description of an Improved Air-Pump. By Mr. T. Cavallo, F. R. S. p. 435.*

The principal improvements which the air-pump received since it was first invented, were contrived by Mr. Smeaton, F. R. S., and are described in the 47th volume of the Philos. Trans. This gentleman considering the imperfections of the air-pumps usually made, not only found means to correct several of them, but improved almost every part of the machine, so as to render it far superior to any thing of the kind done before. It appears, by some experiments of Mr. Nairne, F. R. S. described in the 47th volume of the Phil. Trans. which were made with an air-pump constructed after Mr. Smeaton's principle, that by means



of the best air-pumps made before Mr. Smeaton's invention, the rarefaction of the air within the receiver could never have been brought to more than 40 or 50 times, if the heat of the place was about  $57^{\circ}$ ; that even with Mr. Smeaton's pump the receiver could not be exhausted beyond  $70^{\circ}$  or  $80^{\circ}$  of rarefaction, when moist leathers were used, or moisture was in any way introduced within the receiver; but that when this pump is quite free from moisture, and is newly cleaned, oiled, and put together, then the air may by it be rarefied about 600 times, and not farther.\*

The chief cause which prevents this pump from exhausting farther than that limit is the weakened elasticity of the air remaining within the receiver; which, decreasing in proportion as the quantity of the air within the receiver is diminished, becomes at last incapable of lifting up the valve, which opens the communication between the receiver and the barrel; consequently no more air can in that case pass from the former to the latter. To remove this imperfection of the best air-pumps had been attempted by several ingenious persons; but Mr. C. thinks was never obtained before the contrivance of the air-pump here described. This, he says, is the contrivance of a Mr. Haas, an ingenious maker of philosophical instruments; and that besides this capital improvement, his air-pump is rendered altogether more convenient for philosophical experiments, by answering several purposes. The description of the pump is then given, and illustrated by 3 large engraven plates. As the chief improvement in the machine is said to consist in the contrivance of the valve which opens the communication between the receiver and the barrel, we shall more particularly attend to that part in our description. Now this valve which lets the air pass upwards, but prevents its return, is so contrived as that, when the piston is drawn quite to the top of the barrel, the least possible quantity of air should be left in the barrel. The parts which form this valve are shown separately in fig. 1, pl. 7; where 1,3 is a brass piece that screws into a proper cavity made for its reception, and which is hollow, except its lower part, where it consists of a thin lamina perforated with a small hole 3. Into the hollow of the last-mentioned part is screwed the other perforated piece 2,4, having a slip of oil-silk stretched over its lower part 4, and tied round a small indenture or groove made on its lower part. This slip of oil-silk answers better than a piece of bladder or leather: it just covers the hole 3, and is about 4 times broader than the diameter of the hole. It will be easily conceived, that when the air is forced through the hole 3, it will lift up the slip

\* The degree of rarefaction shown by what is called the pear-gage, when any vapour of water is within the receiver, is not to be considered as the degree of rarefaction of the elastic fluid in the receiver, but only of the air; for though the air may be exhausted, yet the vapour of water will supply its place; we shall therefore only take notice of the exhaustion when no vapour or moisture is within the receiver. See Nairne's Experiments, Phil. Trans. vol. 47.—Orig.



of oil-silk, and passing by the sides of it, and also through the large perforation of the piece 2, 4, will go upwards, &c.; but can by no means return backwards, since any pressure that the air makes on the upper part of the oil-silk will only stop the passage more effectually.

A valve much like this is in the piston, the parts of which are shown separately in fig. 2: *u* is a perforated brass piece screwed to the cylindrical handle or axis, which is also perforated with a short and bent hole. The piece *x* is screwed into the part *u*, and contains a valve, viz. a small piece 6 with a slip of oil-silk tied round its groove *yy*, which slip of oil-silk bears against the hole 5. The piece *x* screwing into the other piece *u*, fastens the round leathers, about 30 in number, which form the stopping part of the piston, and rub with their edges against the cavity of the barrel. This is a very useful improvement, since the common way of using two leathers turned over corks is both troublesome to make, and seldom fits exactly.

Hence, it appears, that the air can pass through the valve from without to within the barrel, but not vice versâ. It will be also easily conceived, that the air can pass from the cavity of the tube of communication only when the said air has elasticity, or force enough to push up the oil-silk. Now the principal improvement in this machine is, to lift up the oil-silk by a power applied externally, when the weakened elasticity of the air within the cavity of the tube is not capable of doing it by itself, and the description of this mechanism is as follows. The double ring 8, 8, fig. 3, which holds the oil-silk, is fastened to 2 steel wires 9, 9. Those wires pass through collars of leathers held in proper brass boxes *ha*, screwed to the piece *k*, and furnished with caps, 11, 11. The lower extremities of the wires are fastened to the cross bar, 7, 7, of a brass frame. If this frame is moved upwards, the wires 9, 9, and the double ring 8, 8, with the oiled-silk, being all connected together, will be pushed also upwards; consequently, the oiled silk being removed from the hole of the piece *k*, a free communication is opened between the cavity of the tube and the cavity of the barrel, through which the air, however rarefied or weakened in elasticity, can pass without the least impediment.

In order to move the brass frame upwards, the end of a lever bears against it. When the valve is to be opened, the foot of the operator must press on the extremity of the lever, by which means the other extremity, with the frame, the wires 9, 9, and the double ring 8, 8, with the oiled silk, are all lifted up. But in order to bring down again all those parts, and to shut the valve when the pressure of the foot is removed, there is an open brass tube fastened to the piece *k*, which contains a spiral spring, that, bearing against the extremity of the brass frame, pushes it downwards.

There are several other minute and particular appendages to this machine, which seem



rather complex. From the experiments and trials made with it, Mr. C. concludes, that when it is in good order, it exhausts the air about 1000 times.

*XXIV. Observation of the Transit of Mercury over the Sun, of Nov. 12, 1782, observed at Cook's Town, near Dungaannon, in Ireland. By the Rev. James Augustus Hamilton, M. A. p. 453.*

After some introductory lines, Mr. H. proceeds, I observed with an achromatic tube of 3 inches aperture, triple object-glass, and used a magnifying power of about 90 times, which I preferred on account of the state of the atmosphere. At about 2 o'clock I set a stop watch to apparent solar time, and placed myself at the telescope within hearing of the beat of the transit-clock. I kept the part of the disc where I expected the ingress in constant view, my sight being directed by a vertical wire in the eye-tube, and at  $2^h 22^m 3^s$  I stopped the watch, and counted  $20^s$ , to be sure of my having really perceived the first impression (which I apprehend could not have been shown  $1^s$  sooner by the power, &c. I used.) I then stopped seconds to the clock, and counted up to an even minute, and found, that the first external contact happened at  $17^h 33^m 11^s$  by the clock, or  $2^h 21^m 45^s$  apparent time. Mercury came in like a distinct black point, without any preceding haziness or appearance of atmosphere; and at  $17^h 39^m 10^s$  by the clock, or  $2^h 27^m 43^s$  apparent time, the thread of light seemed compleated, and then I date the internal contact. I had no instrument fit to take any micrometer measures, so continued only looking at the planet till the sun got so low, that the limb presented the appearance of a troubled sea at a distant horizon, among the waves of which Mercury once more plunged at about  $18^h 52^m$ , and the sun and planet both left my view at about  $18^h 57^m$ ; but these observations are only good conjectures. From my best observations of eclipses of Jupiter's first satellite, of appulses of the moon's centre to the meridians, and lunar distances with a Hadley's quadrant, I make my longitude  $26^m 35^s$  w. nearly, and my latitude by a mean of many observations, is  $54^\circ 38' 20''$ .

*XXV. Method of finding Curve Lines from the Property of the Variations of Curvature. By Nicholas Landierbeck, Adjunct Mathematical Professor at Upsal. p. 456.*

The subject of this paper may be found treated in several of our books on fluxions.

*XXVI. A Series of Observations on, and a Discovery of, the Period of the Variation of the Light of the bright Star in the Head of Medusa, called Algol. By John Goodricke, Esq. Dated York, May 12, 1783. p. 474.*

The following observations, lately made, exhibit a regular and periodical varia-



tion in the star Algol or  $\beta$  Persei, of a nature hitherto, I believe, unnoticed. The first time I saw it vary was Nov. 12, 1782, between 8 and 9 o'clock at night, when it appeared of about the 4th magnitude; but the next day it was of the 2d magnitude, which is its usual appearance. On Dec. 28, I perceived it vary again thus; at  $5\frac{1}{2}$  h. in the evening, it was about the 4th magnitude, as on the 12th of Nov. but at  $8\frac{1}{2}$  h. I was much surprized to find it so quickly increased as to appear of the 2d magnitude. The usual and greatest magnitude of Algol is this; of the 2d magnitude, much less bright than  $\alpha$  Persei, and not so much as  $\gamma$  Andromedæ; brighter than  $\alpha$  Cassiopeæ and  $\beta$  Arietis, and nearly the same, if not rather brighter, than  $\alpha$  Pegasi and  $\beta$  Cassiopeæ; rather less bright than  $\gamma$  Cassiopeæ, and much brighter than  $\epsilon$  Persei and  $\beta$  Trianguli. The relative brightness of the stars to which I compared it during the progress of its variation, is as follows;  $\alpha$  Cassiopeæ is the brightest, and of near the 2d magnitude;  $\beta$  Arietis is the next, and of between the 2d and 3d magnitude; then  $\epsilon$  Persei and  $\beta$  Trianguli, both of the 3d magnitude;  $\zeta$  Persei is somewhat less bright than  $\epsilon$  Persei, and also of the 3d magnitude;  $\delta$  Persei is less than  $\zeta$  Persei, and rather of between the 3d and 4th magnitude;  $\eta$  Persei, which Algol is equal to at its least brightness, is not so bright as  $\delta$  Persei, and of about the 4th magnitude.

*Observations on Algol, as to its Brightness and Magnitude.*

- Jan. 14, 1783.—At 6 h. it was varied from its usual brightness, but rather brighter than  $\beta$  Arietis.  
 At  $6\frac{3}{4}$  h. equal to  $\beta$  Arietis, but rather a little less bright, and of between the 2d and 3d magnitude.  
 At  $7\frac{1}{4}$  h. 3d magnitude; not so bright as  $\beta$  Arietis, and equal to  $\beta$  Trianguli.  
 At  $7\frac{3}{4}$  h. nearly the same as at  $7\frac{1}{4}$ ; but rather less bright than  $\beta$  Trianguli.  
 At  $8\frac{3}{4}$  h. between the 2d and 3d magnitude; not quite so bright as  $\beta$  Trianguli, and rather less than  $\epsilon$  and  $\zeta$  Persei, but a little brighter than  $\delta$  and  $\eta$  Persei.  
 At  $9\frac{1}{4}$  h. about the 4th magnitude, and equal to  $\eta$  Persei.  
 The weather was cloudy till  $11\frac{3}{4}$  h. when it appeared to be of the 3d magnitude; much brighter than  $\eta$  Persei, and rather brighter than  $\gamma$  Persei.  
 At  $12\frac{1}{4}$  h. between the 2d and 3d magnitude; brighter than  $\zeta$  and  $\epsilon$  Persei and  $\beta$  Trianguli.  
 Jan. 17.—At  $7\frac{3}{4}$  h. of the 3d magnitude, equal to  $\epsilon$  Persei, and rather less than  $\beta$  Trianguli.  
 At 8 h. a very little brighter than  $\epsilon$  Persei, and equal to  $\beta$  Trianguli.  
 At  $8\frac{3}{4}$  h. rather brighter than  $\beta$  Trianguli, but the sky was not favourable.  
 Jan. 31.—At  $10\frac{1}{2}$  h. varied from its usual brightness, but with some doubt.  
 At  $11\frac{1}{4}$  h. certainly less bright; much less than  $\gamma$  Andromedæ, but brighter than  $\zeta$  and  $\epsilon$  Persei, and of between the 2d and 3d magnitude.  
 At  $12\frac{1}{4}$  h. 3d magnitude, and rather brighter than  $\zeta$  and  $\epsilon$  Persei.  
 At 13 h. about the brightness of  $\zeta$  Persei, and much brighter than  $\eta$  Persei.  
 At  $14\frac{1}{4}$  h. the 4th magnitude, and equal to  $\eta$  Persei, but afterwards increased.  
 Feb. 6.—At  $5\frac{1}{2}$  h. rather brighter than  $\beta$  Arietis, and between the 3d and 4th magnitude.  
 At  $6\frac{1}{4}$  h. 3d mag.; not so bright as  $\beta$  Arietis, but brighter than  $\beta$  Trianguli and  $\epsilon$  Persei.  
 At  $6\frac{1}{2}$  h. about the same brightness as  $\beta$  Trianguli and  $\epsilon$  Persei.



At 7 h. between the 3d and 4th magnitude; not quite so bright as  $\beta$  Trianguli, nearly equal to  $\delta$  Persei, and a little brighter than  $\epsilon$  Persei.

At  $7\frac{1}{2}$  h. about equal to  $\epsilon$  Persei, and nearly of the 4th magnitude.

At 8 h. rather a little less bright than  $\epsilon$  Persei; sky unfavourable.

At  $8\frac{1}{2}$  h. between the 3d and 4th magnitude; rather brighter than  $\delta$  and  $\epsilon$  Persei.

At 9 h. certainly brighter than  $\delta$  Persei, and of the 3d magnitude.

At  $9\frac{1}{2}$  h. of the same brightness as  $\epsilon$  Persei; but the sky was not favourable.

At 10 h. brighter than  $\epsilon$  Persei.

At  $10\frac{1}{2}$  h. brighter than at 10 h. and of between the 2d and 3d magnitude.

At  $11\frac{1}{2}$  h. very bright; and now it seems at its usual magnitude.

Feb. 9.—At  $6\frac{3}{4}$  h. nearly equal to  $\beta$  Arietis.

Feb. 23.—At  $10\frac{1}{2}$  h. it was brighter than at  $9\frac{1}{2}$  h. when I observed it at its usual brightness; now of the 3d magnitude, rather brighter than  $\epsilon$  and  $\zeta$  Persei.

At 11 h. about the same brightness as  $\epsilon$  and  $\zeta$  Persei.

At 12 h. between the 3d and 4th magnitude; not so bright as  $\epsilon$  and  $\zeta$  Persei, a little brighter than  $\epsilon$  Persei, and a little less than  $\delta$  Persei.

Feb. 26.—At  $6\frac{1}{4}$  h. between the 2d and 3d magnitude; rather less bright than  $\alpha$  Cassiopeæ.

At  $9\frac{1}{2}$  h. little less bright than  $\epsilon$  Persei, and of the 4th magnitude.

At 10 h. nearly between 3d and 4th magnitude; a little brighter than  $\epsilon$  Persei, and a little less bright than  $\delta$  Persei.

March 1.—At  $8\frac{1}{2}$  h. the 3d magnitude; a little brighter than  $\epsilon$  and  $\zeta$  Persei.

At  $8\frac{3}{4}$  h. brighter than at  $8\frac{1}{2}$  h.

At  $9\frac{1}{4}$  h. between the 2d and 3d magnitude; a little less bright than  $\alpha$  Cassiopeæ.

At 10 h. I believe it now at its usual brightness.

March 21.—At  $7\frac{1}{2}$  h. between the 3d and 4th magnitude; not so bright as  $\delta$  Persei, but brighter than  $\epsilon$  Persei.

At 8 h. rather a little brighter than  $\epsilon$  Persei, and sometimes equal to it.

At  $8\frac{1}{2}$  h. the 4th magnitude; equal to  $\epsilon$  Persei: or a very little brighter.

At 9 h. rather a little brighter than  $\epsilon$  Persei.

At 10 h. about the 3d magnitude; equal to  $\zeta$  and  $\epsilon$  Persei, but rather a little brighter.

At  $10\frac{1}{2}$  h. brighter than  $\zeta$  and  $\epsilon$  Persei.

At 11 h. much brighter than  $\zeta$  and  $\epsilon$  Persei; rather between the 2d and 3d magnitude.

April 10.—At 8 h. the 3d magnitude, and rather brighter than  $\epsilon$  Persei.

At  $8\frac{1}{2}$  h. nearly equal to  $\epsilon$  Persei, though rather a little brighter.

At 9 h. rather less bright than  $\epsilon$  Persei, but brighter than  $\delta$  Persei.

At  $9\frac{1}{4}$  h. rather less bright than  $\delta$  Persei, and between the 3d and 4th magnitude.

At  $9\frac{3}{4}$  h. about the 4th magnitude; not so bright as  $\delta$  Persei, but brighter than  $\epsilon$  Persei.

At 10 h. rather less than at  $9\frac{3}{4}$  h.; believe it now very near its least brightness.

April 13.—At 8 h. it was between the 3d and 4th magnitude; brighter than  $\epsilon$  Persei, but not so bright as  $\delta$  Persei.

At  $8\frac{1}{2}$  h. rather brighter than  $\delta$  Persei, and not so bright as  $\epsilon$  Persei.

At 9 h. rather brighter than  $\epsilon$  Persei. It was too low to observe its farther variation.

May 3.—At  $9\frac{1}{4}$  h. between the 3d and 4th magnitude; somewhat brighter than  $\epsilon$  Persei.

From a comparison of all the particulars in the above observations it appears, first, that this star changes from the 2d to about the 4th magnitude in nearly  $3\frac{1}{2}$  hours, and thence to the 2d magnitude again in the same space of time; so that the whole duration of this singular variation is only about 7 hours. And, 2dly, it



appears also, that this variation probably recurs about every 2 days and 21 hours. This last conclusion will be rendered more conspicuous by the following table; the first column of which shows the days, and exact time of the day, when Algol was observed to be very near, or at its least brightness; the 2d column marks the different intervals of time elapsed between the several observations; the 3d exhibits the quotient arising from a division of these intervals by a certain number of revolutions, each of 2 days and 21 hours, which number of revolutions are expressed in the last column.

The day and time when Algol was observed at or near its least brightness.			The different intervals between the several observations.			The quotients of the divisions of the 2d column by the 4th.			Number of revolu- tions.	
1782	Nov.	12 <sup>d</sup> 8 <sup>h</sup> $\frac{1}{2}$								
	Dec.	28 5 <sup>h</sup> $\frac{1}{2}$	.....	45 <sup>d</sup>	21 <sup>h</sup>	.....	2 <sup>d</sup>	20.8 <sup>h</sup>	.....	16
1783	Jan.	14 9 <sup>h</sup> $\frac{1}{4}$	.....	17	3 <sup>h</sup> $\frac{3}{4}$	.....	2	20.6	.....	6
		31 14 <sup>h</sup> $\frac{1}{2}$	.....	17	5	.....	2	20.8	.....	6
	Feb.	6 8	.....	5	17 <sup>h</sup> $\frac{3}{4}$	.....	2	21.	.....	2
		23 12 +	.....	17	4	.....	2	20.6	.....	6
		26 9 <sup>h</sup> $\frac{1}{2}$	.....	2	21 <sup>h</sup> $\frac{1}{2}$	.....	2	21.5	.....	1
	Mar.	21 8 <sup>h</sup> $\frac{1}{2}$	.....	22	23	.....	2	20.9	.....	8
	April	10 10 +	.....	20	1 <sup>h</sup> $\frac{1}{2}$	.....	2	20.8	.....	7
		13 8	.....	2	22	.....	2	22.*	.....	1
	May	3 9 <sup>h</sup> $\frac{1}{4}$	.....	20	1	.....	2	20.7	.....	7

The results in the 3d column agree so nearly, that there is the greatest probability, not to say certainty, that the singular and quick variation of this star, during the space of 7 hours, as above-mentioned, recurs regularly and periodically about every 2 days and nearly 20 $\frac{3}{4}$  hours.

Whether this singular phenomenon is always the same; or whether it occurs only some years, and ceases entirely in others (as may be presumed from the account of Montanari and Maraldi;) and whether in this case it recurs in regular periods of time or otherwise; are curious objects of investigation, which can only be determined by a long and regular course of observations for many years. If it were not perhaps too early to hazard even a conjecture on the cause of this variation, I should imagine it could hardly be accounted for otherwise than either by the interposition of a large body revolving round Algol, or some kind of motion of its own, by which part of its body, covered with spots or such like matter, is periodically turned towards the earth.

\* The difference of upwards of an hour in this quotient will easily be reduced to the others by remarking, that Algol was observed on the 10th and 13th of April, not when it was at, but only near, its least brightness: and indeed all the little differences of the rest will vanish by making a reasonable allowance of the same kind.—Orig.



*I. On the Variation of Light in the Star Algol. By Sir Henry C. Englefield, Bart., F. R. S. Anno 1784, Vol. 74, p. 1.*

Sir H. first looked out at midnight, July 2, 1783, and readily found the star, though hardly visible to the naked eye from the vapours near the horizon. It appeared much larger than the  $\epsilon$ , and full as large again as the  $\pi$ , also in the field at the same time. At  $12\frac{1}{4}^h$ . he looked again, and saw but little difference, as Algol was then also evidently much brighter than  $\epsilon$ . At that time he faintly perceived it with the naked eye. At  $1^h 10^m$  the star was but very little larger than  $\epsilon$ , the diminution having gone on most rapidly in the interval between the last two observations. Though higher above the horizon it was much less (if at all) visible to the naked eye. At  $1^h 35^m$  it was diminished, though but little, since the former observation. It was still however a very little larger than  $\epsilon$ , but not at all visible to the naked eye. At  $2^h$  it was scarcely at all altered from the last observation; but, if any thing, seemed recovering its light.

The fact of the diminution of Algol is, however fully confirmed, if confirmation was wanting, by this observation, and the accuracy of the period fixed by Mr. Goodricke ascertained, as the phenomenon was certainly within half an hour of the time fixed by Mr. Goodricke, which, divided on 8 periods, gives only an error of 4 minutes on the length of it; and a nearer coincidence is not to be expected in a matter of this nature, where estimation is the only means of determining the brightness, and two persons can hardly agree within a few minutes, from the difference of sight.

*II. and III. Observations on the Obscuration of the Star Algol, by Palitch, a Farmer. Communicated by the Count de Bruhl, F. R. S. p. 4.*

Palitch, a farmer of Prolitz, a village in the neighbourhood of Dresden, saw the greatest obscuration of Algol on Sept. 12, at 8 o'clock, P.M. On Oct. 2d and 5th, he observed the same phenomenon again. On the 5th the greatest diminution of that star's light happened some minutes before 7, when he judged it nearly of the size of a star of the 4th magnitude: it continued increasing in brightness till a quarter past 10 in the evening, at which time it had entirely recovered its usual brilliancy and size.

Oct. 20th, he saw Algol nearly at its greatest obscuration, at 3 o'clock in the morning. Oct. 22, near 12 P.M. he observed it again in the same state. Oct. 25, at about 9 P.M. it appeared to him like a star of the 3d magnitude. He was prevented by clouds from making long observations; but as all those he has had opportunities to make, indicate a period somewhat longer than that of 2 days  $20^h 51^m$ , he is inclined to think that  $2^d 20^h 52^m$ , will come very near the truth.



*IV. Descriptions of the King's Wells at Sheerness, Landguard Fort, and Harwich. By Sir Thomas Hyde Page, Knt., F. R. S. p. 6.*

Sir T. H. Page was the commanding military engineer, employed in the sinking of these wells. He engaged, he says, a very ingenious man, Mr. Cole, engine-maker, of Lambeth, as a chief person in this business, and received every assistance in mechanics; and it is but justice to him to express, that the success of the work greatly depended on his attention and the able assistants he procured from distant parts of the kingdom.

The work, on the King's Well at Sheerness, was begun the 4th of June, 1781, and finished the 4th of July, 1782. A circle of 22 feet diameter was first marked out on the ground, and the space evacuated to the depth of 5 feet; after which, pieces of wood, called ribs, on the curve of a diameter, 21 feet 4 inches, and about 9 inches scantling, were placed, to form a complete circle within the excavated part at the bottom, above which other circles of the same nature were placed, and supported by upright pieces of scantlings, having short boards introduced by the intervals, which afterwards pressed on the circles or ribs, between them and the exterior parts. These, when united, formed one frame of wood from the bottom to the top, or rather higher than the excavated space, and prevented the mud of the upper surface, which was very soft, from falling in on the workmen. In proceeding deeper, care was taken to prevent the sinking of the beforementioned frame by its own weight, in excavating parts only under it till another circle of pieces like the first, called ribs, was formed, and uprights, with boards behind, introduced. The distance between these circles was in the first, or upper part of the work, about 3 feet; but as difficulties increased they were placed nearer, and in many parts joined each other without any boards or uprights, and continued through the whole of the wooden frame, against the weight of the mud, quick-sand, and sea-beach, to the depth of 36 feet. At that depth the wood-work was finished, and 6 feet deeper a firm foundation of hard blue clay discovered. The several parts of the frame were then strengthened wherever it appeared necessary, to prevent separation, and to resist the immense pressure of soft mud, quick-sand, and loose sea-beach, which were supported by it. The salt-water, after proceeding thus far, came in very fast through all the joints of the frame, and holes were left on purpose in certain parts to let it run into the well, that it might not be confined entirely to the bottom of the work, which, from the weight on one part only, might have blown, which is ever to be guarded against with the utmost caution.

The frame being found of sufficient strength, and the workmen able, by constant drawing with four 36-gallon buckets, to keep the bottom of the well dry enough to proceed farther, the greatest difficulty seemed to be overcome. The next process was to cut off or stop out the salt water entirely: to effect which, a



smaller circle was described at the bottom of the well, on the hard clay above-mentioned, of the diameter of 8 feet in the clear, round which a curb, or circular frame of wood, was laid, and a brick steening, of 2 bricks thick in terris, raised gradually towards the top of the well, while, as it proceeded upwards, the space between the back of this steening and the wooden frame was filled with good tempered clay, 4 feet thick, and carefully rammed. During this operation, and raising the brick-work, with the clay behind it, the water continued to run over them into the centre of the well, now reduced to 8 feet diameter, and was constantly drawn out, to leave the workmen on the sides sufficiently dry to raise their work till they had reached the top, and consequently as it was water-tight, cut off the filtration from the sea, precautions having been taken to prevent the danger of blowing at the bottom.

The next proceeding appeared more simple ; but great care was still necessary to avoid damaging the foundation of the works already done, as the least crack might have again introduced the salt water. A smaller circle than the last was therefore described, and ribs, forming circles of wood, raised some feet within the brick-work ; and others, of the same form, were sunk to the depth of 8 feet below the bottom, on which the several works already described rested. After this, a course of bricks was carried up within the lastmentioned ribs or circles, on a diameter of 6 feet, by which they became inclosed and joined with the firstmentioned brick-work, having the clay wall and wooden frame pressing behind them on large diameters. In sinking lower, small curbs were at certain distances placed to support the steening, which consisted of 2 stretching courses of bricks, laid separately, and keyed into the clay or back part of the brick-work by rough pieces of stone, flint, &c. to prevent a slipping or lowering of the steening by its own weight. The work was carried on from this period, without any material difficulty or difference in the clay (except the very extraordinary discovery of a piece of a tree at the depth of 300 feet from the top of the well, till the appearance of water at 328 feet deep, by a small mixture of sand in the clay, with oozing of water from it ; and at 330 feet deep, on boring the whole bottom of the well blew up, and it was with difficulty the workmen escaped the torrents of water that followed them, which was mixed with a quick-sand that rose 40 feet in the bottom of the well, at which height it still remains. The water rose in 6 hours 189 feet, and in a few days within 8 feet deep of the top of the well. It has since been carefully analyzed by a chemist, and found perfectly good for every purpose ; and, it is presumed, the quantity will be equal to every demand of public and private use at that place, as there has been, ever since it was first discovered, a constant drawing of water, and it has hitherto been found impossible to lower the well more than 200 feet ; there has consequently always been a depth left in water of 130 feet. The water is of a very



soft quality, and on being drawn, has a degree of warmth unusual in common well water. It remains yet to be determined whence that warmth proceeds; but as it proved wholesome, the circumstance is fortunate for the soldiers of the garrison, as they will not be liable to complaints that are so frequent among troops (as often happens at Dover Castle) from imprudence in drinking great quantities of very cold well water.

The King's Wells at Landguard-Fort were begun and finished in the year 1782. The peculiar situation of this fort made it very unlikely that springs of fresh water could ever be found; there being great reason to think, that the out-fall of the Ipswich and Manningtree rivers, which unite before they reach the sea, was formerly on the Suffolk side of the fort, but is now on the Essex side; and as the garrison in ancient writings, is described to have been built on the Andrew's Sand, there appeared little probability of any filtration of water through it, except that of the sea. It however seemed proper to try the possibility of sinking through it, to endeavour to find a hard bottom, similar to that discovered at Sheerness, fresh water being of great consequence to the defence of the place. The work was accordingly begun; but about the same time, in making the excavation of a ditch for one of the batteries, at a very few feet from the upper surface of the sand, a small quantity of fresh water was perceived; and it was chance that led to a discovery of its freshness, from one of the labourers happening to taste it. On examining further, it was found that the quantity of water on sinking was considerable, and that it appeared perfectly fresh. The well-sinkers were then ordered to proceed to this depth at another place, where they found a like appearance of good water; and the quantity was so great, as to render it very difficult to keep the bottom of the well, at 12 feet deep, dry enough to sink farther. Every exertion however was used, and with great labour a well was sunk to the depth of low water mark at spring tides, about 18 feet from the upper surface of the sand; when, to the surprize of every person, the water that rose from the bottom became, on a sudden, entirely salt. This put an end to the work for a time, as it seemed impossible to penetrate deeper. It was now directed that sand should be thrown into the well, to bring it a little above what had been the lowest fresh water line (12 feet from the upper surface) and then the water was drawn out which had mixed. After this, the filtration into the well became again perfectly fresh, and in equal quantity to the first appearance. This was therefore fixed as the greatest depth (12 feet); and another well was sunk at 40 feet distance, with a horizontal brick drain, having holes left in the sides for filtration, to collect the water, and the bottoms of both wells were secured with hard materials; that the whole supply of water might be reduced to the drain, which is constructed to prevent as much as possible the mixture of sand with the water, and is found to answer the desired end. This



success arose from various unexpected circumstances ; but I am yet, says Sir T., at a loss for the cause of the fresh water, or whence it comes. But however this may be accounted for, the discovery at Landguard-Fort is of very great consequence to the garrison ; and there is reason to think that in similar situations, where water is wanted, an attention to what has been already explained may be found of use.

The King's Wells at Harwich were begun May 6, and finished Sept. 29, 1781. The wells in this neighbourhood, being very shallow, and only depending on springs from the upper surfaces of the ground, have but little water in the summer, and the quality of it is very bad. It was imagined therefore, that the most likely way to obtain a better spring, was to sink a well from higher ground, and to endeavour to penetrate through a rock which lay a few yards under the level of the country, though the operation might be tedious, on the chance of cutting a spring of better water, that might be unconnected with the land-drains. The experiment answered in every respect, as there was not a drop of water found till the rock had been entirely cut through, when, on finding a considerable quantity of moist sand, and boring into it, a plentiful spring was discovered, which supplied the troops ever after with very good water. And it was thought that this supply, the spring being very powerful, would be found equal to every demand for public and private purposes, in the driest seasons. After this success, as matter of curiosity, an old well was made deeper, by excavating through the rocks, where a good spring was also found : but as that well had been originally sunk from low ground, a great deal of the bad water from the upper drains, &c. mixed with it, and gave it a disagreeable taste.

*V. The Discovery of a Comet. By Edward Pigott, Esq. p. 20.*

This comet Mr. P. discovered at York, Nov. 19, 1783 ; when he made the following observations on it : viz.

	R. A.	North Decl.
Nov. 19 <sup>d</sup> 11 <sup>h</sup> 15 <sup>m</sup> .....	41° 0'	3° 10'
20 10 54 .....	40 0	4 32

Nov. 21, Mr. P. again saw the comet where he expected it, according to the above determinations ; but could not observe it with an instrument. It looked like a nebula, with a diameter of about 2' of a degree. The nucleus, being very faint, is seen, with some difficulty, when the wires of the instrument are illuminated. It is not visible with an opera glass.

*VI. Project for a new Division of the Quadrant. By Charles Hutton, LL. D., F. R. S. p. 21.*

*This project is for constructing the sines, tangents, secants, &c. to equal parts of the radius.—The arbitrary division of the quadrant of the circle into equal*



parts by 60ths, which has been delivered down to us from the ancients, and gradually extended by similar sub-divisions by the moderns, among various uses, serves for trigonometrical and other mathematical operations, by adapting to those divisions of the arc, certain lines expressed in equal parts of the radius, as chords, sines, tangents, &c. "But among all the improvements in this useful branch of science, I have long wished to see a set of tables of sines, tangents, secants, &c. constructed to the arcs of the quadrant as divided into the like equal parts of the radius as those lines themselves. In this natural way, the arcs would not be expressed by divisions of 60ths, in degrees, minutes, &c., but by the common decimal scale of numbers; and the real lengths of the arcs, expressed in such common numbers, would then stand opposite their respective sines, tangents, &c. The uses of such an alteration would be many and great, and are too obvious and important to need pointing out or enforcing. I have therefore had for a long time a great desire to commence this arduous task; but continual interruptions have hitherto prevented me from making any considerable progress in so desirable an undertaking. But I am not without hopes that some future occasion may prove more propitious to my ardent wishes. It is not however to be expected, that this work can be accomplished by the labours of one person only; it will require rather the united endeavours of many." But as this paper will be found in Dr. Hutton's works collected, any further account of it in this place may be spared.

*VII. On the Means of discovering the Distance, Magnitude, &c. of the Fixed Stars, in consequence of the Diminution of the Velocity of their Light, in case such a Diminution should be found to take place in any of them, and such other Data should be procured from Observations, as would be further necessary for that Purpose. By the Rev. John Michell, B. D., F. R. S. p. 35.*

1. The very great number of stars that have been discovered to be double, triple, &c. particularly by Mr. Herschel, if we apply the doctrine of chances, as was done in Mr. M.'s "Inquiry into the probable Parallax, &c. of the Fixed Stars," published in the Philos. Trans. for 1767, (abridg. vol. 12, p. 423,) cannot leave a doubt with any one, who is properly aware of the force of those arguments, that by far the greatest part, if not all of them, are systems of stars so near to each other, as probably to be liable to be affected sensibly by their mutual gravitation; and it is therefore not unlikely, that the periods of the revolutions of some of these about their principles (the smaller ones being, on this hypothesis, to be considered as satellites to the others) may some time or other be discovered.

2. Now, the apparent diameter of any central body, round which any other body revolves, together with their apparent distance from each other, and the



periodical time of the revolving body being given, the density of the central body will be given also. See Newton's Prin. 3, pr. 8, cor. 1. 3. But the density of any central body being given, and the velocity any other body would acquire by falling towards it from an infinite height, or, which is the same thing, the velocity of a comet revolving in a parabolic orbit, at its surface, being given, the quantity of matter, and consequently the real magnitude of the central body, would be also given.

4. Let us now suppose the particles of light to be attracted in the same manner as all other bodies with which we are acquainted; that is, by forces bearing the same proportion to their vis inertię; of which there can be no reasonable doubt, gravitation being, as far as we know, or have any reason to believe, a universal law of nature. On this supposition then, if any one of the fixed stars, whose density was known by the abovementioned means, should be large enough sensibly to affect the velocity of the light issuing from it, we should have the means of knowing its real magnitude, &c.

5. It has been demonstrated by Newton, in prop. 39, b. 1, that if a right line be drawn, in the direction of which a body is urged by any forces whatever, and there be erected at right angles to that line perpendiculars every where proportional to the forces at the points at which they are erected respectively, the velocity acquired by a body beginning to move from rest, in consequence of being so urged, will always be proportional to the square root of the area described by the aforesaid perpendiculars. And hence, 6. If such a body, instead of beginning to move from rest, had already some velocity in the direction of the same line, when it began to be urged by the aforesaid forces, its velocity would then be always proportional to the square root of the sum or difference of the aforesaid area, and another area, whose square root would be proportional to the velocity which the body had before it began to be so urged; that is, to the square root of the sum of those areas, if the motion acquired was in the same direction as the former motion, and the square root of the difference, if it was in a contrary direction. See cor. 2, to the abovesaid proposition.

7. In order to find, by the foregoing proposition, the velocity which a body would acquire by falling towards any other central body, according to the common law of gravity, let  $c$ , in fig. 4, pl. 7, represent the centre of the central body, towards which the falling body is urged, and let  $ca$  be a line drawn from the point  $c$ , extending infinitely towards  $A$ . If then the line  $RD$  be supposed to represent the force, by which the falling body would be urged at any point  $D$ , the velocity which it would have acquired by falling from an infinite height to the place  $D$  would be the same as that which it would acquire by falling from  $D$  to  $c$  with the force  $RD$ , the area of the infinitely extended hyperbolic space  $ADRB$ , where  $RD$  is always inversely proportional to the square of  $DC$ , being equal to the



rectangle  $RC$  contained between the lines  $RD$  and  $CD$ . From hence we may draw the following corollaries.

8. *Corol. 1.* The central body  $DEF$  remaining the same, and consequently the forces at the same distances remaining also the same, the areas of the rectangles  $RC$ ,  $rc$  will always be inversely as the distances of the points  $D$ ,  $d$  from  $C$ , their sides  $RD$ ,  $rd$  being inversely in the duplicate ratio of the sides  $CD$ ,  $cd$ : and therefore, because the velocity of a body falling from an infinite height towards the point  $C$ , is always in the sub-duplicate ratio of these rectangles, it will be in the sub-duplicate ratio of the lines  $CD$ ,  $cd$  inversely. Accordingly, the velocities of comets revolving in parabolic orbits are always in the sub-duplicate ratio of their distances from the sun inversely; and the velocities of the planets, at their mean distances (being always in a given ratio to the velocity of such comets, viz. in the sub-duplicate ratio of 1 to 2) must necessarily observe the same law also.

9. *Cor. 2.* The magnitude of the central body remaining the same, the velocity of a body falling towards it from an infinite height, will always be, at the same distance from the point  $C$ , taken any where without the central body, in the sub-duplicate ratio of its density; for in this case the distance  $cd$  will remain the same, the line  $rd$  only being increased or diminished in the proportion of the density, and the rectangle  $rc$  consequently increased or diminished in the same proportion.

10. *Cor. 3.* The density of the central body remaining the same, the velocity of a body falling towards it from an infinite height will always be as its semi-diameter, when it arrives at the same proportional distance from the point  $C$ : for the weights, at the surfaces of different spheres of the same density, are as their respective semi-diameters; and therefore the sides  $RD$  and  $CD$ , or any other sides  $rd$  and  $cd$ , which are in a given ratio to those semi-diameters, being both increased or diminished in the same proportion, the rectangles  $RC$  or  $rc$  will be increased or diminished in the duplicate ratio of the semi-diameter  $CD$ , and consequently the velocity in the simple ratio of  $CD$ .

11. *Cor. 4.* If the velocity of a body falling from an infinite height, towards different central bodies, be the same, when it arrives at their surfaces, the density of those central bodies must be in the duplicate ratio of their semi-diameters inversely; for by the last cor. the density of the central body remaining the same, the rectangle  $RC$  will be in the duplicate ratio of  $CD$ ; in order therefore that the rectangle  $RC$  may always remain the same, the line  $RD$  must be inversely as  $CD$ , and consequently the density inversely as the square of  $CD$ .

12. *Cor. 5.* Hence the quantity of matter contained in those bodies must be in the simple ratio of their semi-diameters directly; for the quantity of matter being always in a ratio compounded of the simple ratio of the density, and the triplicate ratio of their semi-diameters, if the density be in the inverse duplicate



ratio of the semi-diameters, this will become the direct triplicate and inverse duplicate, that is, when the two are compounded together, the simple ratio of the semi-diameters.

13. The velocity a body would acquire by falling from an infinite height towards the sun, when it arrived at his surface, being the same with that of a comet, revolving in a parabolic orbit in the same place, would be about 20.72 times greater than that of the earth in its orbit at its mean distance from the sun; for the mean distance of the earth from the sun, being above 214.64 of the sun's semi-diameters, the velocity of such a comet would be greater at that distance than at the distance of the earth from the sun, in the sub-duplicate ratio of 214.64 to 1, and the velocity of the comet being likewise greater than that of planets, at their mean distances, in the sub-duplicate ratio of 2 to 1; these, when taken together, will make the sub-duplicate ratio of 429.28 to 1, and the square root of 429.28 is 20.72, very nearly.

14. The same result would have been obtained by taking the line RD proportional to the force of gravity at the sun's surface, and DC equal to his semi-diameter; and thence computing a velocity, which should be proportional to the square root of the area RC when compared with the square root of another area, one of whose sides should be proportional to the force of gravity at the surface of the earth; and the other should be, for instance, equal to 16 feet 1 inch, the space a body would fall through in one second of time, in which case it would acquire a velocity of 32 feet 2 inches per second. The velocity thus found compared with the velocity of the earth in its orbit, when computed from the same elements, necessarily gives the same result. Mr. M. made use of this latter method of computation on a former occasion, as may be seen in Dr. Priestley's History of Optics, p. 787, &c. but he has rather chosen to take the velocity from that of a comet, in the article above, on account of its greater simplicity, and its more immediate connexion with the subject of this paper.

15. The velocity of light exceeding that of the earth in its orbit, when at its mean distance from the sun, in the proportion of about 10310 to 1, if we divide 10310 by 20.72, the quotient 497, in round numbers, will express the number of times which the velocity of light exceeds the velocity a body could acquire by falling from an infinite height towards the sun, when it arrived at his surface; and an area whose square root should exceed the square root of the area RC, where RD is supposed to represent the force of gravity at the surface of the sun, and CD is equal to his semi-diameter, in the same proportion, must consequently exceed the area RC in the proportion of 247009, the square of 497 to 1.

16. Hence, according to article 10, if the semi-diameter of a sphere of the same density with the sun were to exceed that of the sun in the proportion of 500 to 1, a body falling from an infinite height towards it, would have acquired



at its surface a greater velocity than that of light; and consequently, supposing light to be attracted by the same force in proportion to its *vis inertię*, with other bodies, all light emitted from such a body would be made to return towards it, by its own proper gravity.

17. But if the semi-diameter of a sphere, of the same density with the sun, was of any other size less than 497 times that of the sun, though the velocity of the light emitted from such a body would never be wholly destroyed, yet would it always suffer some diminution, more or less, according to the magnitude of the said sphere; and the quantity of this diminution may be easily found in the following manner: suppose  $s$  to represent the semi-diameter of the sun, and  $as$  to represent the semi-diameter of the proposed sphere; then, as appears from what has been shown before, the square root of the difference between the square of  $497s$  and the square of  $as$  will be always proportional to the ultimately remaining velocity, after it has suffered all the diminution it can possibly suffer from this cause; and consequently the difference between the whole velocity of light, and the remaining velocity, as found above, will be the diminution of its velocity. And hence the diminution of the velocity of light emitted from the sun, on account of its gravitation towards that body, will be somewhat less than a 494000th part of the velocity which it would have had, if no such diminution had taken place; for the square of 497 being 247009, and the square of 1 being 1, the diminution of the velocity will be the difference between the square root of 247009 and the square root of 247008, which amounts, as above, to somewhat less than one 494000th part of the whole quantity.

18. The same effects would likewise take place, according to article 11, if the semi-diameters were different from those mentioned in the last 2 articles, provided the density was greater or less in the duplicate ratio of those semi-diameters inversely.

19. The better to illustrate this matter, it may not be amiss to take a particular example. Let us suppose then, that it should appear from some observations made on some one of those double stars above alluded to, that one of the two performed its revolution round the other in 64 years, and that the central one was of the same density with the sun, which it must be, if its apparent diameter, when seen from the other body, was the same as the apparent diameter of the sun would be if seen from a planet revolving round him in the same period: let us further suppose, that the velocity of the light of the central body was found to be less than that of the sun, or other stars whose magnitude was not sufficient to affect it sensibly, in the proportion of 19 to 20. In this case then, according to article 17, the square root of  $247009ss$  must be to the square root of the difference between  $247009ss$  and  $aass$ , as 20 to 19. But the squares of 20 and 19 being 400 and 361, the quantity  $247009ss$  must therefore be to the



difference between this quantity and  $aass$ , in the same proportion, that is as 247009 to 222925.62; and  $aass$  must consequently be equal to 24083.38 $ss$ , whose square root 1552 $s$  nearly, or, in round numbers, 155 times the diameter of the sun, will be the diameter of the central star sought.

20. As the squares of the periodic times of bodies, revolving round a central body, are always proportional to the cubes of their mean distances, the distance of the two bodies from each other must therefore, on the foregoing suppositions, be 16 times greater in proportion to the diameter of the central body, than the distance of the earth from the sun in proportion to his diameter; and that diameter being already found to be also greater than that of the sun in the proportion of 155.2 to 1, this distance will consequently be greater than that of the earth and sun from each other in the proportion of 16 times 155.2, that is 2483.2 to 1.

21. Let us further suppose, that from the observations, the greatest distance of the two stars in question appeared to be only 1 second; we must then multiply the number 2483.2 by 206264.8, the number of seconds in the radius of a circle, and the product 512196750 will show the number of times which such a star's distance from us must exceed that of the sun. The quantity of matter contained in such a star would be  $155.2^3$  or 3738308 times as much as that contained in the sun; its light, supposing the sun's light to take up 8<sup>m</sup> 7<sup>s</sup> in coming to the earth, would, with its common velocity, require 7900 years to arrive at us, and 395 years more on account of the diminution of that velocity; and supposing such a star to be equally luminous with the sun, it would still be very sufficiently visible, I apprehend, to the naked eye, notwithstanding its immense distance.

22. In the elements employed in the above computations, I have supposed the diameter of the central star to have been observed, in order to ascertain its density, which cannot be known without it; but the diameter of such a star is much too small to be observed by any telescopes yet existing, or any that it is probably in the power of human abilities to make; for the apparent diameter of the central star, if of the same density with the sun, when seen from another body, which would revolve round it in 64 years, would be only the 1717th part of the distance of those bodies from each other, as will appear from multiplying 107.32 the number of times the sun's diameter is contained in his distance from the earth, by 16, the greater proportional distance of the revolving body, corresponding to 64 years, instead of 1. Now the 1717th part of a second must be magnified 309060 times in order to give it an apparent diameter of 3'; and 3', if the telescopes were mathematically perfect, and there was no want of distinctness in the air, would be but a very small matter to judge of.

23. But though there is not the least probability that this element, so essen-



tial to be known, in order to determine with precision the exact distance and magnitude of a star, can ever be obtained, where it is in the same circumstances, or nearly the same, with those above supposed, yet the other elements, such as perhaps may be obtained, are sufficient to determine the distance, &c. with a good deal of probability, within some moderate limits; for in whatever ratio the real distance of the two stars may be greater or less than the distance supposed, the density of the central star must be greater or less in the sixth power of that ratio inversely; for the periodic time of the revolving body being given, the quantity of matter contained in the central body must be as the cube of their distance from each other. (Newton's Prin. b. 3, pr. 8, cor. 3.) But the quantity of matter in different bodies, at whose surfaces the velocity acquired by falling from an infinite height is the same, must be, according to art. 12, directly as their semi-diameters; the semi-diameters therefore of such bodies must be in the triplicate ratio of the distance of the revolving body; and consequently their densities, by art. 11, being in the inverse duplicate ratio of their semi-diameters, must be in the inverse sextuplicate ratio of the distance of the revolving body. Hence, if the real distance should be greater or less than that supposed, in the proportion of 2 or 3 to 1, the density of the central body must be less or greater, in the first case, in the proportion of 64, or in the latter of 729 to 1.

24. There is also another circumstance, from which perhaps some little additional probability might be derived, with regard to the real distance of a star, such as that we have supposed; but on which however, it must be acknowledged, that no great stress can be laid, unless we had some better analogy to go on than we have at present. The circumstance I mean is the greater specific brightness which such a star must have, in proportion as the real distance is less than that supposed, and vice versâ; since, in order that the star may appear equally luminous, its specific brightness must be as the 4th power of its distance inversely; for the diameter of the central star being as the cube of the distance between that and the revolving star, and their distance from the earth being in the simple ratio of their distance from each other, the apparent diameter of the central star must be as the square of its real distance from the earth, and consequently, the surface of a sphere being as the square of its diameter, the area of the apparent disc of such a star must be as the 4th power of its distance from the earth; but in whatever ratio the apparent disc of the star is greater or less, in the same ratio inversely must be the intensity of its light, in order to make it appear equally luminous. Hence, if its real distance should be greater or less than that supposed in the proportion of 2 or 3 to 1, the intensity of its light must be less or greater, in the first case, in the proportion of 16, or in the latter of 81, to 1.

25. According to Mons. Bouguer (see his *Traité d'Optique*) the brightness of



the sun exceeds that of a wax candle in no less a proportion than that of 8000 to 1. If therefore the brightness of any of the fixed stars should not exceed that of our common candles, which, as being something less luminous than wax, we will suppose in round numbers to be only one 10000th part as bright as the sun, such a star would not be visible at more than an 100th part of the distance, at which it would be visible, if it was as bright as the sun. Now because the sun would still appear, I apprehend, as luminous, as the star Sirius, when removed to 400000 times his present distance, such a body, if no brighter than our common candles, would only appear equally luminous with that star at 4000 times the distance of the sun, and we might then begin to be able, with the best telescopes, to distinguish some sensible apparent diameter of it; but the apparent diameters of the stars of the less magnitudes would still be too small to be distinguished even with our best telescopes, unless they were yet a good deal less luminous, which may possibly however be the case with some of them; for, though we have indeed very slight grounds to go upon with regard to the specific brightness of the fixed stars compared with that of the sun at present, and can therefore only form very uncertain and random conjectures concerning it, yet from the infinite variety which we find in the works of the creation, it is not unreasonable to suspect, that very possibly some of the fixed stars may have so little natural brightness in proportion to their magnitude, as to admit of their diameters having some sensible apparent size, when they shall come to be more carefully examined, and with larger and better telescopes than have been hitherto in common use.

26. With regard to the sun, we know that his whole surface is extremely luminous, a very small and temporary interruption sometimes from a few spots only excepted. This universal and excessive brightness of the whole surface is probably owing to an atmosphere, which being luminous throughout, and in some measure also transparent, the light, proceeding from a considerable depth of it, all arrives at the eye; in the same manner as the light of a great number of candles would do, if they were placed one behind another, and their flames were sufficiently transparent to permit the light of the more distant ones to pass through those that were nearer, without any interruption.

27. How far the same constitution may take place in the fixed stars we do not know; probably however it may do so in many; but there are some appearances with regard to a few of them, which seem to make it probable, that it does not do so universally. Now, if I am right in supposing the light of the sun to proceed from a luminous atmosphere, which must necessarily diffuse itself equally over the whole surface, and I think there can be very little doubt that this is really the case, this constitution cannot well take place in those stars, which are in some degree periodically more or less luminous, such as that in *Collo Ceti*, &c.



It is also not very improbable, that there is some difference from that of the sun, in the constitution of those stars, which have sometimes appeared and sometimes disappeared, of which that in the constellation of Cassiopeia is a notable instance. And if those conjectures are well founded which have been formed by some philosophers concerning stars of these kinds, that they are not wholly luminous, or at least not constantly so, but that all, or by far the greatest part of their surfaces is subject to considerable changes, sometimes becoming luminous, and at other times being extinguished; it is among the stars of this sort, that we are most likely to meet with instances of a sensible apparent diameter, their light being much more likely not to be so great in proportion as that of the sun, which, if removed to 400,000 times his present distance; would still appear, I apprehend, as bright as Sirius, as I have observed above; whereas it is hardly to be expected, with any telescopes whatever, that we should ever be able to distinguish a well defined disc of any body of the same size with the sun at much more than 10,000 times his distance.

28. Hence the greatest distance at which it would be possible to distinguish any sensible apparent diameter of a body, as dense as the sun, cannot well greatly exceed 5 million times the distance of the sun; for if the diameter of such a body was not less than 500 times that of the sun, its light, as has been shown above, in art. 16, could never arrive at us.

29. If there should really exist in nature any bodies, whose density is not less than that of the sun, and whose diameters are more than 500 times the diameter of the sun, since their light could not arrive at us; or if there should exist any other bodies of a somewhat smaller size, which are not naturally luminous; of the existence of bodies under either of these circumstances, we could have no information from sight; yet, if any other luminous bodies should happen to revolve about them, we might still perhaps from the motions of these revolving bodies, infer the existence of the central ones with some degree of probability, as this might afford a clue to some of the apparent irregularities of the revolving bodies, which would not be easily explicable on any other hypothesis; but as the consequences of such a supposition are very obvious, and the consideration of them somewhat beside my present purpose, I shall not prosecute them any further.

30. The diminution of the velocity of light, in case it should be found to take place in any of the fixed stars, is the principal phenomenon whence it is proposed to discover their distance, &c. Now the means by which we may find what this diminution amounts to, seem to be supplied by the difference which would be occasioned in consequence of it, in the refrangibility of the light, whose velocity should be so diminished. For let us suppose with Sir Isaac Newton (*Optics*, prop. 6, paragr. 4 and 5) that the refraction of light is occasioned



by a certain force impelling it towards the refracting medium, an hypothesis which perfectly accounts for all the appearances. On this hypothesis, the velocity of light in any medium, in whatever direction it falls on it, will always bear a given ratio to the velocity it had before it fell on it, and the sines of incidence and refraction will, in consequence of this, bear the same ratio to each other with these velocities inversely. Thus, according to this hypothesis, if the sines of the angles of incidence and refraction, when light passes out of air into glass, are in the ratio of 31 to 20, the velocity of light in the glass must be to its velocity in air in the same proportion of 31 to 20. But because the areas, representing the forces generating these velocities, are as the squares of the velocities, art. 5 and 6, these areas must be to each other as 961 to 400. And if 400 represent the area which corresponds to the force producing the original velocity of light, 561, the difference between 961 and 400, must represent the area corresponding to the additional force, by which the light was accelerated at the surface of the glass.

31. In art. 19 we supposed, by way of example, the velocity of the light of some particular star to be diminished in the ratio of 19 to 20, and it was there observed, that the area representing the remaining force which would be necessary to generate the velocity 19, was therefore properly represented by  $\frac{361}{400}$  parts of the area, that should represent the force that would be necessary to generate the whole velocity of light, when undiminished. If then we add 561, the area representing the force by which the light is accelerated at the surface of the glass, to 361, the area representing the force which would have generated the diminished velocity of the star's light, the square root of 922, their sum, will represent the velocity of the light with the diminished velocity, after it has entered the glass. And the square root of 922 being 30.364, the sines of incidence and refraction of such light, out of air into glass, will consequently be as 30.364 to 19, or what is equal to it, as 31.96 to 20 instead of 31 to 20, the ratio of the sines of incidence and refraction, when the light enters the glass with its velocity undiminished.

32. Hence a prism, with a small refracting angle, might perhaps be found to be no very inconvenient instrument for this purpose: for by such a prism, whose refracting angle was of 1', for instance, the light with its velocity undiminished would be turned out of its way 33", and with the diminished velocity 35".88 nearly, the difference between which being almost 2" 53"', would be the quantity by which the light, whose velocity was diminished, would be turned out of its way more than that whose velocity was undiminished.

33. Let us now be supposed to make use of such a prism to look at two stars, under the same circumstances as the two stars in the example above-mentioned, the central one of which should be large enough to diminish the velocity of its



light a 20th part, while the velocity of the light of the other, which was supposed to revolve about it as a satellite, for want of sufficient magnitude in the body whence it was emitted, should suffer no sensible diminution at all. Placing then the line, in which the two faces of the prism would intersect each other, at right angles to a line joining the two stars; if the thinner part of the prism lay towards the same point of the heavens with the central star, whose light would be most turned out of its way, the apparent distance of the stars would be increased  $2'' 53'''$ , and consequently become  $3'' 53'''$ , instead of  $1''$  only, the apparent distance supposed above in art. 21. On the contrary, if the prism should be turned half way round, and its thinner part lie towards the same point of the heavens with the revolving star, their distance must be diminished by a like quantity, and the central star therefore would appear  $1'' 53'''$  distant from the other on the opposite side of it, having been removed from its place near 3 times the whole distance between them.

34. As a prism might be made use of for this purpose, which should have a much larger refracting angle than that we have proposed, especially if it was constructed in the achromatic way, according to Mr. Dollond's principles, not only such a diminution, as 1 part in 20, might be made still more distinguishable; but we might probably be able to discover considerably less diminutions in the velocity of light, as perhaps a 100th, a 200th, a 500th, or even a 1000th part of the whole, which, according to what has been said above, would be occasioned by spheres, whose diameters should be to that of the sun, provided they were of the same density, in the several proportions nearly of 70, 50, 30, and 22 to 1 respectively.

35. If such a diminution of the velocity of light, as that above supposed, should be found really to take place, in consequence of its gravitation towards the bodies, whence it is emitted, and there should be several of the fixed stars large enough to make it sufficiently sensible, a set of observations on this subject might probably give us some considerable information with regard to many circumstances of that part of the universe, which is visible to us. The quantity of matter contained in many of the fixed stars might hence be judged of, with a great degree of probability, within some moderate limits; for though the exact quantity must still depend on their density, yet we must suppose the density most enormously different from that of the sun, and more so indeed than one can easily conceive to take place in fact, to make the error of the supposed quantity of matter very wide of the truth, since the density, as has been shown above in art. 11 and 12, which is necessary to produce the same diminution in the velocity of light, emitted from different bodies, is as the square of the quantity of matter contained in those bodies inversely.

36. But though we might possibly hence form some reasonable guess at the



quantity of matter contained in several of the fixed stars; yet, if they have no luminous satellites revolving about them, we shall still be at a loss to form any probable judgment of their distance, unless we had some analogy to go on for their specific brightness, or had some other means of discovering it; there is however a case that may possibly occur, which may tend to throw some light on this matter.

37. I have shown in my Inquiry into the probable Parallax, &c. of the Fixed Stars, in the Philos. Trans. 1767, the extremely great probability there is, that many of the fixed stars are collected together into groups; and that the Pleiades in particular constitute one of these groups. Now of the stars which we there see collected together, it is highly probable, as I have observed in that paper, that there is not one in a hundred which does not belong to the group itself; and by far the greatest part therefore, according to the same idea, must lie within a sphere, a great circle of which is of the same size with a circle, which appears to us to include the whole group. If we suppose therefore this circle to be about  $2^{\circ}$  in diameter, and consequently only about a 30th part of the distance at which it is seen, we may conclude, with the highest degree of probability, that by far the greatest part of these stars do not differ in their distances from the sun by more than about one part in 30, and thence deduce a sort of scale of the proportion of the light produced by different stars of the same group or system in the Pleiades at least; and, by a somewhat probable analogy, we may do the same in other systems likewise. But having yet no means of knowing their real distance, or specific brightness, when compared either with the sun or with each other, we shall still want something more to form a further judgment from.

38. If however it should be found, that among the Pleiades, or any other like system, there are some stars that are double, triple, &c. of which one is a larger central body, with one or more satellites revolving about it, and the central body should likewise be found to diminish the velocity of its light; and more especially, if there should be several such instances met with in the same system; we should then begin to have a kind of measure both of the distance of such a system of stars from the earth, and of their mutual distances from each other. And if several instances of this kind should occur in different groups or systems of stars, we might also perhaps begin to form some probable conjectures concerning the specific density and brightness of the stars themselves, especially if there should be found any general analogy between the quantity of the diminution of the light and the distance of the system deduced from it; as, for instance, if those stars, which had the greatest effect in diminishing the velocity of light, should in general give a greater distance to the system, when supposed to be of the same density with the sun, we might then naturally thence conclude, that they are less in bulk, and of greater specific density, than those stars which diminish the



velocity of light less, and vice versâ. In like manner, if the larger stars were to give us in general a greater or less quantity of light in proportion to their bulk, this would give us a kind of analogy, whence we might perhaps form some judgment of the specific brightness of the stars in general; but, at all adventures, we should have a pretty tolerable measure of the comparative brightness of the sun and those stars, on which such observations should be made, if the result of them should turn out agreeable to the ideas above explained.

39. Though it is not improbable, that a few years may inform us, that some of the great number of double, triple stars, &c. which have been observed by Mr. Herschel, are systems of bodies revolving about each other, especially if a few more observers, equally ingenious and industrious with himself, could be found to second his labours; yet the very great distance at which it is not unlikely many of the secondary stars may be placed from their principals, and the consequently very long periods of their revolutions, leave very little room to hope that any very great progress can be made in this subject for many years, or perhaps some ages to come; the above outlines therefore, of the use that may be made of the observations on the double stars, &c. provided the particles of light should be subject to the same law of gravitation with other bodies, as in all probability they are, and provided also that some of the stars should be large enough sensibly to diminish their velocity, will I hope be an inducement, to those who may have it in their power to make these observations for the benefit of future generations at least, how little advantage soever we may expect from them ourselves; and yet very possibly some observations of this sort, and such as may be made in a few years, may not only be sufficient to do something, even at present, but also to show that much more may be done hereafter, when these observations shall become more numerous, and have been continued for a longer period of years.

*VIII. A Meteorological Journal for the Year 1782, kept at Minehead, Somersetshire. By Mr. John Atkins. p. 58.*

This is a register of the weather, winds, barometer, thermometer, and rain, 3 times every day in the year. The instruments observed with were kept at a house about 30 feet above high water in the Bristol channel. The whole quantity of rain was 31.26 inches.

*IX. Description of a Meteor, Aug. 18, 1783. By Mr. T. Cavallo, F. R. S. p. 108.*

Mr. C. observed this curious meteor on the Castle Terrace at Windsor, in company with other gentlemen. They stood on the north-east corner of the terrace, where they had a perfect view of the whole phenomenon: as every



one of the company remarked some particular circumstances, and the collection of them all furnished the materials for this account, it may be presumed, that this description is as true as the nature of the subject can admit of.

It was in the hazy part of the atmosphere, about the N. by W.  $\frac{1}{2}$  W. point of the compass, that this luminous meteor was first perceived. Some flashes of lambent light, much like the aurora borealis, were first observed on the northern part of the heavens, which were soon perceived to proceed from a roundish luminous body, nearly as large as the semi-diameter of the moon, and almost stationary in the above mentioned point of the heavens, see fig. 5, pl. 7. It was then about 25 minutes after 9 o'clock in the evening. This ball, at the beginning, appeared of a faint bluish light, perhaps from its being just kindled, or from its appearing through the haziness; but it gradually increased its light, and soon began to move, at first ascending above the horizon in an oblique direction towards the east. Its course in this direction was very short, perhaps of 5 or 6 degrees; after which it turned towards the east, and moving in a direction nearly parallel to the horizon, reached as far as the S. E. by E. where it finally disappeared. The whole duration of the meteor was half a minute, or rather less; and the altitude of its track seemed to be about  $25^{\circ}$  above the horizon. A short time after the beginning of its motion, the luminous body passed behind a small cloud, so that during this passage they observed only the light that was cast in the heavens from behind the cloud, without actually seeing the body from which it proceeded, for about the 6th or at most the 5th part of its track; but as soon as the meteor emerged from behind the cloud, its light was prodigious. Every object appeared very distinct; the whole face of the country being instantly illuminated. At this moment the body of the meteor appeared of an oblong form, as at fig. 6; but it presently acquired a tail, and soon after it parted into several small bodies, each having a tail, and all moving in the same direction, at a small distance from each other, and very little behind the principal body, the size of which was gradually reduced after the division, as at fig. 7. In this form the whole meteor moved as far as the S. E. by E. where the light decreasing rather abruptly, the whole disappeared.

During the phenomenon no noise was heard by any of the company, except one person, who thought he heard a crackling noise, something like that which is produced by small wood when burning. But about 10 minutes after the disappearance of the meteor, and when they were about to retire from the terrace, they heard a rumbling noise, as of thunder at a great distance, which probably was the report of the meteor's explosion; and it may be naturally imagined that this explosion happened when the meteor parted into small bodies, viz. at about the middle of its track. Now if this noise was really the report of the explosion which happened in the above-mentioned place, the distance, altitude, course,



and other particulars relating to this meteor, must be very nearly as expressed in the following list; being calculated with mathematical accuracy, on the preceding particulars; and on the supposition that sound travels 1150 feet per second. But if the noise we heard was not that of the meteor's explosion, then the following calculations must be considered as quite useless.

Distance of the meteor from Windsor Castle . . . . . 130 miles.  
 Length of the path it described in the heavens . . . . . 550 miles.  
 Diameter of the luminous body when first seen . . . . . 1070 yards.  
 Its height above the surface of the earth . . . . .  $56\frac{1}{2}$  miles.

The explosion must have happened perpendicularly over Lincolnshire.

*X. An Account of the Meteors of the 18th of August and 4th of October, 1783. By Alex. Aubert, Esq., F.R.S., and S.A. p. 112.*

The 18th of August had been a very sultry day. At the time the meteor made its appearance, though the stars were bright in the upper part of the heavens, the horizon was surrounded with a haziness which did not permit any stars to be seen under an altitude of about  $8^{\circ}$ . Mr. A. was returning to Loampit-hill, near Deptford, in Kent; his face was turned towards the south-west. He was at the foot of Lewisham bridge, when he perceived suddenly a kind of glimmering light, resembling faint but quickly repeated flashes of lightning; soon after which the light increased much towards the north-west; he turned directly to it, and saw it form into a large luminous body like electrical fire, with a tinge of blue round its edges. It rose from the hazy part of the atmosphere, and moved at first almost in a vertical direction, changing its size and figure continually, having all the appearances of successive inflammation, and not of a solid body; it was sometimes round, at others oval and oblong, with its longest diameter in the line of its motion; though it had got high enough to be quite out of the hazy part of the horizon, it was surrounded and accompanied in its whole course with a kind of whitish mist or light vapour. The place from which it rose was about  $38^{\circ}$  from the north towards the west. After rising a little way perpendicularly, it made its progress in a curve, so as to be at the highest when it had reached due east, at an altitude of about  $35^{\circ}$ ; after which, continuing a few degrees beyond the east, and being about  $30^{\circ}$  high, it left behind it several globules of various shapes; the first which detached itself being very small, and the others gradually larger and larger, till the last was nearly as large as the remaining preceding body; soon afterwards they all extinguished gradually, like the bright stars of a sky-rocket, with some inclination downwards. The meteor was at the brightest and at the largest just before its separation; Mr. A. estimated its magnitude or area then to be equivalent to 2 full moons. Its light, during its whole course, was so great, that he could see every object distinctly, and when



it was extinguished the night appeared very dark: Mr. A. could however see by his watch that it was 17<sup>m</sup> after 9. The whole appearance of the meteor, from its first rising out of the hazy part of the atmosphere to its total extinction, did not exceed 10 or 12 seconds of time, during which it moved a space corresponding to about 136° in azimuth. Mr. A. recollected an appearance during its motion, which confirmed him in the idea he had of its not being a solid body. In its progress it did not describe a curve as regular as might have been expected from such a body; but seemed to move in somewhat of a waving line. The meteor appeared extremely near to him, more particularly when it was at the highest; yet from the comparisons made of observations at several distant places, we may reasonably judge that it could not be at less than 40 or 50 miles distance from the surface of the earth.

The meteor of Oct. 4 was of a much shorter duration and path. Mr. A. was near the stones end, in Blackman-street, Southwark; his face turned northward. He saw, towards the N. N. E. a train of fire, resembling in its motion a common meteor, vulgarly called a falling star, but its colour was red; it originated at an altitude of from 25 to 30°, and moved quickly in a straight line eastward, inclining gradually towards the horizon, so as to be, after a course of 15° or 20° in azimuth, about 15° above the horizon, when it spread into a broader train, and became of a lighter colour, it terminated by resolving into a beautiful oblong body of the brightest fire, like electrical fire tinged blue; almost as large as the moon; it illuminated the street and houses much more than any lightning he had seen; those who had not a direct view of it, took it for a long flash of lightning. He thinks its whole course did not exceed 25°, nor the time of its appearance 2 or 3 seconds. It extinguished quickly, and left behind it, in its path, a train of very dull reddish fire, which continued visible to the naked eye above a minute and a half. The time of night was 43 minutes past 6; it was a fine star-light evening, warmer than the preceding ones; the moon beyond the first quarter, and very bright; yet her light was not to be compared to the much greater light of the meteor. Mr. A. did not recollect hearing any noise or report, either during or after the appearance of these meteors.

*XI. On the Remarkable Meteor seen Aug. 18, 1783. By Wm. Cooper, D.D., F. R. S., Archdeacon of York. p. 116.*

No person, says Dr. C., could have a better opportunity of discerning this awful meteor than myself. The weather being, for this climate, astonishingly hot, by Fahrenheit's thermometer, on a north position, and in the open air, having for several days preceding graduated between the hours of 10 in the morning and 7 in the evening from 74° to 82°, I set out on a journey to the sea side. The weather was sultry, the atmosphere hazy, and not a breath of



air stirring. Towards 9 at night it was so dark, that I could scarcely discern the hedges, road, or even the horses heads. As we proceeded, I observed to my attendants, that there was something singularly striking in the appearance of the night, not merely from its stillness and darkness, but from the sulphureous vapours which seemed to surround us on every side. In the midst of this gloom, and on an instant, a brilliant tremulous light appeared to the N.W. by N. At first it seemed stationary; but in a short time it burst from its position, and took its course to the S.E. by E. It passed directly over our heads with a buzzing noise, seemingly at the height of 60 yards. Its tail, as far as the eye could form any judgment, was about 8 or 10 yards in length. At last, this wonderful meteor divided into several glowing parts or balls of fire, the chief part still remaining in its full splendour. Soon after this I heard two great explosions, each equal to the report of a cannon carrying a 9 lb. ball. During its progress, the whole of the atmosphere, as far as I could discern, was perfectly illuminated with the most beautifully vivid light I ever remember to have seen. The horses on which we rode shrunk with fear; and some people whom we met on the road declared their consternation in the most expressive terms.

*XII. On the Meteor of Aug. 18, 1783. By R. L. Edgeworth, Esq., F. R. S.*  
p. 118.

This observation was made near Mullinger, in Ireland. At half past 9 in the evening Mr. E. saw the meteor. Its size appeared to be about one-third of the moon's diameter; and it moved from the north with an equable velocity, at an elevation of 10 or 12 degrees, and in a line parallel to the horizon. It was visible during 10 or 15 seconds, and seemed to be of a parabolic figure, with a luminous tail, 20 or 25 of its diameters in length. It exhibited the most vivid colours; the foremost part being of the brightest blue, followed by different shades of red. Twice during its flight it was eclipsed or extinguished, not gradually, but at once, immersing and emerging with undiminished lustre.

*XIII. Experiments on Air. By H. Cavendish, Esq., F. R. S., & S. A.* p. 119.

The following experiments, says Mr. C., were made principally with a view to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed; and as they seem not only to determine this point, but also to throw great light on the constitution and manner of production of dephlogisticated air, I hope they may be not unworthy the acceptance of this society.

Many gentlemen have supposed that fixed air is either generated or separated from atmospheric air by phlogistication, and that the observed diminution is



owing to this cause; my first experiments therefore were made in order to ascertain whether any fixed air is really so produced. Now, as all animal and vegetable substances contain fixed air, and yield it by burning, distillation, or putrefaction, nothing can be concluded from experiments in which the air is phlogisticated by them. The only methods I know, which are not liable to objection, are by the calcination of metals, the burning of sulphur or phosphorus, the mixture of nitrous air, and the explosion of inflammable air. Perhaps it may be supposed that I ought to add to these the electric spark; but I think it much more likely that the phlogistication of the air, and production of fixed air, in this process, is owing to the burning of some inflammable matter in the apparatus. When the spark is taken from a solution of tournsol, the burning of the tournsol may produce this effect; when it is taken from lime-water, the burning of some foulness adhering to the tube, or perhaps of some inflammable matter contained in the lime, may have the same effect; and when quicksilver or metallic knobs are used, the calcination of them may contribute to the phlogistication of the air, though not to the production of fixed air.

There is no reason to think that any fixed air is produced by the first method of phlogistication. Dr. Priestley never found lime-water to become turbid by the calcination of metals over it:\*. Mr. Lavoisier also found only a very slight and scarce perceptible turbid appearance, without any precipitation, to take place when lime-water was shaken in a glass vessel full of the air in which lead had been calcined; and even this small diminution of transparency in the lime-water might very likely arise, not from fixed air, but only from its being fouled by particles of the calcined metal, which we are told adhered in some places to the glass. This want of turbidity has been attributed to the fixed air uniting to the metallic calx, in preference to the lime; but there is no reason for supposing that the calx contained any fixed air; for I do not know that any one has extracted it from calces prepared in this manner; and though most metallic calces prepared over the fire, or by long exposure to the atmosphere, where they are in contact with fixed air, contain that substance, it by no means follows that they must do so when prepared by methods in which they are not in contact with it.

Dr. Priestley also observed, that quicksilver, fouled by the addition of lead or tin, deposits a powder by agitation and exposure to the air, which consists in great measure of the calx of the imperfect metal. He found too some powder of this kind to contain fixed air;† but it is by no means clear that this air was produced by the phlogistication of the air in which the quicksilver was shaken; as the powder was not prepared on purpose, but was procured from quicksilver fouled by having been used in various experiments, and may therefore have contained other impurities besides the metallic calces.

\* Experiments on Air, vol. i. p. 137.

† Exper. in Nat. Phil. vol. i. p. 144.



I never heard of any fixed air being produced by the burning of sulphur or phosphorus; but it has been asserted, and commonly believed, that lime-water is rendered cloudy by a mixture of common and nitrous air; which, if true, would be a convincing proof that on mixing those 2 substances some fixed air is either generated or separated; I therefore examined this carefully. Now as common air usually contains a little fixed air, which is no essential part of it, but is easily separated by lime-water; and as nitrous air may also contain fixed air, either if the metal from which it is procured be rusty, or if the water of the vessel in which it is caught contain calcareous earth, suspended by fixed air, as most waters do, it is proper first to free both airs from it by previously washing them with lime-water.\* Now I found, by repeated experiments, that if the lime-water was clean, and the two airs were previously washed with that substance, not the least cloud was produced, either immediately on mixing them, or on suffering them to stand upwards of an hour, though it appeared by the thick clouds which were produced in the lime-water, by breathing through it after the experiment was finished, that it was more than sufficient to saturate the acid formed by the decomposition of the nitrous air, and consequently that if any fixed air had been produced, it must have become visible. Once indeed I found a small cloud to be formed on the surface, after the mixture had stood a few minutes. In this experiment the lime-water was not quite clean; but whether the cloud was owing to this circumstance, or to the air's having not been properly washed, I cannot pretend to say.

Neither does any fixed air seem to be produced by the explosion of the inflammable air obtained from metals, with either common or dephlogisticated air. This I tried by putting a little lime-water into a glass globe fitted with a brass cock, so as to make it air tight, and an apparatus for firing air by electricity. This globe was exhausted by an air-pump, and the two airs, which had been previously washed with lime-water, let in, and suffered to remain some time, to show whether they would affect the lime-water, and then fired by electricity. The event was, that not the least cloud was produced in the lime-water, when the inflammable air was mixed with common air, and only a very slight one, or rather diminution of transparency, when it was combined with dephlogisticated air. This however seemed not to be produced by fixed air; as it appeared instantly after the explosion, and did not increase on standing, and was spread

\* Though fixed air is absorbed in considerable quantity by water, as I showed in Phil. Trans. vol. 56, yet it is not easy to deprive common air of all the fixed air contained in it by means of water. On shaking a mixture of 10 parts of common air, and 1 of fixed air, with more than an equal bulk of distilled water, not more than half of the fixed air was absorbed, and on transferring the air into fresh distilled water only  $\frac{1}{2}$  the remainder was absorbed, as appeared by the diminution which it still suffered on adding lime-water.—Orig.



uniformly through the liquor; whereas if it had been owing to fixed air, it would have taken up some short time before it appeared, and would have begun first at the surface, as was the case in the above-mentioned experiment with nitrous air. What it was really owing to I cannot pretend to say; but if it did proceed from fixed air, it would show that only an excessively minute quantity was produced.\* On the whole, though it is not improbable that fixed air may be generated in some chemical processes, yet it seems certain that it is not the general effect of phlogisticating air, and that the diminution of common air is by no means owing to the generation or separation of fixed air from it.

As there seemed great reason to think, from Dr. Priestley's experiments, that the nitrous and vitriolic acids were convertible into dephlogisticated air, I tried whether the dephlogisticated part of common air might not, by phlogistication, be changed into nitrous or vitriolic acid. For this purpose, I impregnated some milk of lime with the fumes of burning sulphur, by putting a little of it into a large glass receiver, and burning sulphur in it, taking care to keep the mouth of the receiver stopped till the fumes were all absorbed; after which the air of the receiver was changed, and more sulphur burnt in it as before, and the process repeated till 122 grs. of sulphur were consumed. The milk of lime was then filtered and evaporated, but it yielded no nitrous salt, nor any other substance except selenite; so that no sensible quantity of the air was changed into nitrous acid. Now, as the vitriolic acid, produced by the burning sulphur, is changed by its union with the lime into selenite, which is very little soluble in water, a very small quantity of nitrous salt, or any other substance which is soluble in water, would have been perceived.

I also tried whether any nitrous acid was produced by phlogisticated common air with liver of sulphur; for this purpose I made a solution of flowers of sulphur by boiling it with lime, and put a little of it into a large receiver, and shook it frequently, changing now and then the air, till the yellow colour of the solution was quite gone; a sign that all the sulphur was, by the loss of its phlogiston, turned into vitriolic acid, and united to the lime, or precipitated; the liquor was then filtered and evaporated, but it yielded not the least nitrous salt. The experiment was repeated in nearly the same manner with dephlogisticated air procured from red precipitate; but not the least nitrous acid was obtained.

It is well known that common selenite is very little soluble in water; whereas that procured in the 2 last experiments was very soluble, and even crystallized readily, and was intensely bitter; this however appeared to be owing merely to the acid with which it was formed being very much phlogisticated; for on evaporating it to dryness, and exposing it to the air for a few days,

\* Dr. Priestley also found no fixed air to be produced by the explosion of inflammable and common air, vol. 5, p. 124.—Orig.



it became much less soluble; so that on adding water to it not much dissolved, and by repeating this process once or twice, it seemed to become not more soluble than selenite made in the common manner. This solubility of the selenite caused some trouble in trying the experiment; for while it continued much soluble it would have been impossible to have distinguished a small mixture of nitrous salt; but by the above-mentioned process I was able to distinguish as small a proportion as if the selenite had been originally no more soluble than usual.

The nature of the neutral salts made with the phlogisticated vitriolic and nitrous acids has not been much examined by the chemists, though it seems well worth their attention; and it is likely that many, besides the foregoing, may differ remarkably from those made with the same acids in their common state. Nitre formed with the phlogisticated nitrous acid has been found to differ considerably from common nitre, as well as sal polychrest from vitriolated tartar.

In order to try whether any vitriolic acid was produced by the phlogistication of air, I impregnated 50 oz. of distilled water with the fumes produced on mixing 52 oz. measures of nitrous air with a quantity of common air sufficient to decompose it. This was done by filling a bottle with some of this water, and inverting it into a basin of the same, and then, by a syphon, letting in as much nitrous air as filled it half-full; after which common air was added slowly by the same syphon, till all the nitrous air was decomposed. When this was done, the distilled water was further impregnated in the same manner, till the whole of the above-mentioned quantity of nitrous air was employed. This impregnated water, which was very sensibly acid to the taste, was distilled in a glass retort. The first runnings were very acid, and smelt pungent, being nitrous acid much phlogisticated; what came next had no sensible taste or smell; but the last runnings were very acid, and consisted of nitrous acid not phlogisticated. Scarcely any sediment was left behind. These different parcels of distilled liquor were then exactly saturated with salt of tartar, and evaporated; they yielded  $87\frac{1}{2}$  grs. of nitre, which, as far as I could perceive, was unmixed with vitriolated tartar or any other substance, and consequently no sensible quantity of the common air with which the nitrous air was mixed was turned into vitriolic acid. It appears, from this experiment, that nitrous air contains as much acid as  $2\frac{3}{4}$  times its weight of saltpetre; for 52 oz. measures of nitrous air weigh 32 grains, and, as was before said, yield as much acid as is contained in  $87\frac{1}{2}$  grains of saltpetre; so that the acid in nitrous air is in a remarkably concentrated state, and I believe more than  $1\frac{1}{2}$  times as much so as the strongest spirit of nitre ever prepared.

Having now mentioned the unsuccessful attempts I made to find out what becomes of the air lost by phlogistication, I proceed to some experiments, which serve really to explain the matter. In Dr. Priestley's last vol. of experiments is



related an experiment of Mr. Warltire's, in which it is said that, on firing a mixture of common and inflammable air by electricity, in a close copper vessel holding about 3 pints, a loss of weight was always perceived, on an average about 2 grs., though the vessel was stopped in such a manner that no air could escape by the explosion. It is also related, that on repeating the experiment in glass vessels, the inside of the glass, though clean and dry before, immediately became dewy; which confirmed an opinion he had long entertained, that common air deposits its moisture by phlogistication. As the latter experiment seemed likely to throw great light on the subject I had in view, I thought it well worth examining more closely. The first experiment also, if there was no mistake in it, would be very extraordinary and curious; but it did not succeed with me; for though the vessel I used held more than Mr. Warltire's, namely, 24,000 grs. of water, and though the experiment was repeated several times with different proportions of common and inflammable air, I could never perceive a loss of weight of more than  $\frac{1}{5}$ th of a grain, and commonly none at all. However, though there were some of the experiments in which it seemed to diminish a little in weight, there were none in which it increased.\* In all the experiments, the inside of the glass globe became dewy, as observed by Mr. Warltire; but not the least sooty matter could be perceived. Care was taken in all of them to find how much the air was diminished by the explosion, and to observe its test. The result is as follows: the bulk of the inflammable air being expressed in decimals of the common air,

Common air.	Inflammable air.	Diminution.	Air remaining after the explosion.	Test of this air in first method.	Standard.
1	1.241	.686	1.555	.055	.0
	1.055	.642	1.413	.063	.0
	.706	.647	1.059	.066	.0
	.423	.612	.811	.097	.03
	.331	.476	.855	.339	.27
	.206	.294	.912	.648	.58

In these experiments the inflammable air was procured from zinc, as it was in all my experiments, except where otherwise expressed: but I made 2 more experiments, to try whether there was any difference between the air from zinc and that from iron, the quantity of inflammable air being the same in both, namely, 0.331 of the common; but I could not find any difference to be depended on between the two kinds of air, either in the diminution which they suffered by the explosion, or the test of the burnt air.

From the fourth experiment it appears, that 423 measures of inflammable air are nearly sufficient to completely phlogisticate 1000 of common air; and that the bulk of the air remaining after the explosion is then very little more than  $\frac{1}{5}$ ths.

\* Dr. Priestley, I am informed, has since found the experiment not to succeed.—Orig.



of the common air employed; so that, as common air cannot be reduced to a much less bulk than that, by any method of phlogistication, we may safely conclude, that when they are mixed in this proportion, and exploded, almost all the inflammable air, and about  $\frac{1}{5}$  part of the common air, lose their elasticity, and are condensed into the dew which lines the glass.

The better to examine the nature of this dew, 500000 grain measures of inflammable air were burnt with about  $2\frac{1}{2}$  times that quantity of common air, and the burnt air made to pass through a glass cylinder 8 feet long and  $\frac{3}{4}$  of an inch in diameter, in order to deposit the dew. The 2 airs were conveyed slowly into this cylinder by separate copper pipes, passing through a brass plate which stopped up the end of the cylinder; and as neither inflammable nor common air can burn by themselves, there was no danger of the flame spreading into the magazines from which they were conveyed. Each of these magazines consisted of a large tin vessel, inverted into another vessel just large enough to receive it. The inner vessel communicated with the copper pipe, and the air was forced out of it by pouring water into the outer vessel; and in order that the quantity of common air expelled should be  $2\frac{1}{2}$  times that of the inflammable, the water was let into the outer vessels by 2 holes in the bottom of the same tin pan, the hole which conveyed the water into that vessel in which the common air was confined being  $2\frac{1}{2}$  times as large as the other. In trying the experiment, the magazines being first filled with their respective airs, the glass cylinder was taken off, and water let, by the 2 holes, into the outer vessels, till the airs began to issue from the ends of the copper pipes; they were then set on fire by a candle, and the cylinder put on again in its place. By this means upwards of 135 grs. of water were condensed in the cylinder, which had no taste nor smell, and which left no sensible sediment when evaporated to dryness; neither did it yield any pungent smell during the evaporation; in short, it seemed pure water.

In my first experiment, the cylinder near that part where the air was fired was a little tinged with a sooty matter, but very slightly so; and that little seemed to proceed from the putty with which the apparatus was luted, and which was heated by the flame; for in another experiment, in which it was contrived so that the luting should not be much heated, scarcely any sooty tinge could be perceived. By the experiments with the globe it appeared, that when inflammable and common air are exploded in a proper proportion, almost all the inflammable air, and near  $\frac{1}{5}$  of the common air, lose their elasticity, and are condensed into dew. And by this experiment it appears, that this dew is plain water, and consequently that almost all the inflammable air, and about  $\frac{1}{5}$  of the common air, are turned into pure water.

In order to examine the nature of the matter condensed on firing a mixture of dephlogisticated and inflammable air, I took a glass globe, holding 8800 grain



measures, furnished with a brass cock, and an apparatus for firing air by electricity. This globe was well exhausted by an air-pump, and then filled with a mixture of inflammable and dephlogisticated air, by shutting the cock, fastening a bent glass tube to its mouth, and letting up the end of it into a glass jar inverted into water, and containing a mixture of 19500 grain measures of dephlogisticated air, and 37000 of inflammable; so that, on opening the cock, some of this mixed air rushed through the bent tube, and filled the globe.\* The cock was then shut, and the included air fired by electricity; by which means almost all of it lost its elasticity. The cock was then again opened, so as to let in more of the same air, to supply the place of that destroyed by the explosion, which was again fired, and the operation continued till almost the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than the 6th part of the mixture, almost the whole of it was exploded in it, without any fresh exhaustion of the globe.

As I was desirous to try the quantity and test of this burnt air, without letting any water into the globe, which would have prevented my examining the nature of the condensed matter, I took a larger globe, furnished also with a stop cock, exhausted it by an air-pump, and screwed it on the cock of the former globe; then, by opening both cocks, the air rushed out of the smaller globe into the larger, till it became of equal density in both; then, by shutting the cock of the larger globe, unscrewing it again from the former, and opening it under water, I was enabled to find the quantity of the burnt air in it; and consequently, as the proportion which the contents of the 2 globes bore to each other was known, could tell the quantity of burnt air in the small globe before the communication was made between them. By this means the whole quantity of the burnt air was found to be 2950 grain measures; its standard was 1.85. The liquor condensed in the globe, in weight about 20 grs., was sensibly acid to the taste, and by saturation with fixed alkali, and evaporation, yielded near 2 grs. of nitre; so that it consisted of water united to a small quantity of nitrous acid. No sooty matter was deposited in the globe. The dephlogisticated air used in this experiment was procured from red precipitate, that is, from a solution of quicksilver in spirit of nitre distilled till it acquires a red colour.

As it was suspected, that the acid contained in the condensed liquor was no essential part of the dephlogisticated air, but was owing to some acid vapour which came over in making it, and had not been absorbed by the water, the experiment was repeated in the same manner, with some more of the same air, which had been previously washed with water, by keeping it a day or 2 in a

\* In order to prevent any water from getting into this tube, while dipped under water to let it up into the glass jar, a bit of wax was stuck on the end of it, which was rubbed off when raised above the surface of the water.—Orig.



bottle with some water, and shaking it frequently; whereas that used in the preceding experiment had never passed through water, except in preparing it. The condensed liquor was still acid. The experiment was also repeated with dephlogisticated air, procured from red lead by means of oil of vitriol; the liquor condensed was acid, but by an accident I was prevented from determining the nature of the acid. I also procured some dephlogisticated air from the leaves of plants, in the manner of Doctors Ingenhousz and Priestley, and exploded it with inflammable air as before; the condensed liquor still continued acid, and of the nitrous kind.

In all these experiments the proportion of inflammable air was such, that the burnt air was not much dephlogisticated; and it was observed that the less phlogisticated it was, the more acid was the condensed liquor. I therefore made another experiment, with some more of the same air from plants, in which the proportion of inflammable air was greater, so that the burnt air was almost completely phlogisticated, its standard being  $\frac{1}{10}$ . The condensed liquor was then not at all acid, but seemed pure water: so that it appears, that with this kind of dephlogisticated air, the condensed liquor is not at all acid, when the two airs are mixed in such a proportion that the burnt air is almost completely phlogisticated, but is considerably so when it is not much phlogisticated.

In order to see whether the same thing would obtain with air procured from red precipitate, I made two more experiments with that kind of air, the air in both being taken from the same bottle, and the experiment tried in the same manner, except that the proportions of inflammable air were different. In the 1st, in which the burnt air was almost completely phlogisticated, the condensed liquor was not at all acid. In the second, in which its standard was 1.86, that is, not much phlogisticated, it was considerably acid; so that with this air, as well as with that from plants, the condensed liquor contains, or is entirely free from, acid, according as the burnt air is less or more phlogisticated; and there can be little doubt but that the same rule obtains with any other kind of dephlogisticated air.

In order to see whether the acid, formed by the explosion of dephlogisticated air obtained by means of the vitriolic acid, would also be of the nitrous kind, I procured some air from turbith mineral, and exploded it with inflammable air, the proportion being such that the burnt air was not much phlogisticated. The condensed liquor manifested an acidity, which appeared, by saturation with a solution of salt of tartar, to be of the nitrous kind; and it was found, by the addition of some terra ponderosa salita, to contain little or no vitriolic acid.

When inflammable air was exploded with common air, in such a proportion that the standard of the burnt air was about  $\frac{4}{10}$ , the condensed liquor was not in the least acid. There is no difference however in this respect, between common



air, and dephlogisticated air mixed with phlogisticated in such a proportion as to reduce it to the standard of common air; for some dephlogisticated air from red precipitate, being reduced to this standard by the addition of perfectly phlogisticated air, and then exploded, with the same proportion of inflammable air as the common air was in the foregoing experiment, the condensed liquor was not in the least acid.

From the foregoing experiments it appears, that when a mixture of inflammable and dephlogisticated air is exploded in such proportion that the burnt air is not much phlogisticated, the condensed liquor contains a little acid, which is always of the nitrous kind, whatever substance the dephlogisticated air is procured from; but if the proportion be such that the burnt air is almost entirely phlogisticated, the condensed liquor is not at all acid, but seems pure water, without any addition whatever; and as, when they are mixed in that proportion, very little air remains after the explosion, almost the whole being condensed, it follows, that almost the whole of the inflammable and dephlogisticated air is converted into pure water. It is not easy indeed to determine from these experiments what proportion the burnt air, remaining after the explosions, bore to the dephlogisticated air employed, as neither the small nor the large globe could be perfectly exhausted of air, and there was no saying with exactness what quantity was left in them; but in most of them, after allowing for this uncertainty, the true quantity of burnt air seemed not more than  $\frac{1}{17}$  of the dephlogisticated air employed, or  $\frac{1}{56}$  of the mixture. It seems however unnecessary to determine this point exactly, as the quantity is so small, that there can be little doubt but that it proceeds only from the impurities mixed with the dephlogisticated and inflammable air, and consequently, that if those airs could be obtained perfectly pure, the whole would be condensed.

With respect to common air, and dephlogisticated air reduced by the addition of phlogisticated air to the standard of common air, the case is different; as the liquor condensed in exploding them with inflammable air, I believe I may say in any proportion, is not at all acid; perhaps, because if they are mixed in such a proportion as that the burnt air is not much phlogisticated, the explosion is too weak, and not accompanied with sufficient heat.

All the foregoing experiments, on the explosion of inflammable air with common and dephlogisticated airs, except those which relate to the cause of the acid found in the water, were made in the summer of the year 1781, and were mentioned by me to Dr. Priestley, who in consequence of it made some experiments of the same kind, as he relates in a paper printed in the preceding volume of the Transactions. During the last summer also, a friend of mine gave some account of them to M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of phlogiston; but at that time



so far was M. Lavoisier from thinking any such opinion warranted, that till he was prevailed on to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water. It is remarkable, that neither of these gentlemen found any acid in the water produced by the combustion; which might proceed from the latter having burnt the two airs in a different manner from what I did; and from the former having used a different kind of inflammable air, namely, that from charcoal, and perhaps having used a greater proportion of it.

Before entering into the cause of these phenomena, it will be proper to take notice, that phlogisticated air appears to be nothing else than the nitrous acid united to phlogiston; for when nitre is deflagrated with charcoal, the acid is almost entirely converted into this kind of air. That the acid is entirely converted into air, appears from the common process for making what is called clyssus of nitre; for if the nitre and charcoal are dry, scarcely any thing is found in the vessels prepared for condensing the fumes; but if they are moist, a little liquor is collected, which is nothing but the water contained in the materials, impregnated with a little volatile alkali, proceeding in all probability from the imperfectly burnt charcoal, and a little fixed alkali, consisting of some of the alkalized nitre carried over by the heat and watery vapours. As far as I can perceive too, at present, the air into which much the greatest part of the acid is converted, differs in no respect from common air phlogisticated. A small part of the acid however is turned into nitrous air, and the whole is mixed with a good deal of fixed, and perhaps a little inflammable air, both proceeding from the charcoal.

It is well known, that the nitrous acid is also converted by phlogistication into nitrous air, in which respect there seems a considerable analogy between that and the vitriolic acid; for this acid, when united to a smaller proportion of phlogiston, forms the volatile sulphureous acid and vitriolic acid air; both of which, by exposure to the atmosphere, lose their phlogiston, though not very fast, and are turned back into vitriolic acid; but, when united to a greater proportion of phlogiston, it forms sulphur, which shows no signs of acidity; unless a small degree of affinity to alkalis can be called so, and in which the phlogiston is more strongly adherent, so that it does not fly off when exposed to the air, unless assisted by a heat sufficient to set it on fire. In like manner the nitrous acid, united to a certain quantity of phlogiston, forms nitrous fumes and nitrous air, which readily quit their phlogiston to common air; but when united to a different, in all probability a larger quantity, it forms phlogisticated air, which shows no signs of acidity, and is still less disposed to part with its phlogiston than sulphur.

This being premised, there seem 2 ways by which the phenomena of the acid



found in the condensed liquor may be explained; first, by supposing that dephlogisticated air contains a little nitrous acid which enters into it as one of its component parts, and that this acid, when the inflammable air is in a sufficient proportion, unites to the phlogiston, and is turned into phlogisticated air, but does not when the inflammable air is in too small a proportion; and, secondly, by supposing that there is no nitrous acid mixed with, or entering into the composition of, dephlogisticated air, but that when this air is in a sufficient proportion, part of the phlogisticated air with which it is debased is, by the strong affinity of phlogiston to dephlogisticated air, deprived of its phlogiston, and turned into nitrous acid; whereas, when the dephlogisticated air is not more than sufficient to consume the inflammable air, none then remains to deprive the phlogisticated air of its phlogiston, and turn it into acid.

If the latter explanation be true, I think we must allow that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston; or, in other words, that water consists of dephlogisticated air united to phlogiston; and that inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose, or else water united to phlogiston;\* since, according to this supposition, these two substances united together form pure water. On the other hand, if the first explanation be true, we must suppose that dephlogisticated air consists of water united to a little nitrous acid and deprived of its phlogiston; but still the nitrous acid in it must make only a very small part of the whole, as it is found, that the phlogisticated air, which it is converted into, is very small in comparison of the dephlogisticated air.

I think the second of these explanations seems much the most likely; as it was found, that the acid in the condensed liquor was of the nitrous kind, not only when the dephlogisticated air was prepared from red precipitate, but also

\* Either of these suppositions will agree equally well with the following experiments; but the latter seems to me much the most likely. What principally makes me think so is, that common or dephlogisticated air do not absorb phlogiston from inflammable air, unless assisted by a red heat, whereas they absorb the phlogiston of nitrous air, liver of sulphur, and many other substances, without that assistance; and it seems inexplicable, that they should refuse to unite to pure phlogiston, when they are able to extract it from substances to which it has an affinity; that is, that they should overcome the affinity of phlogiston to other substances, and extract it from them, when they will not even unite to it when presented to them. On the other hand, I know no experiment which shows inflammable air to be pure phlogiston rather than a union of it with water, unless it be Dr. Priestley's experiment of expelling inflammable air from iron by heat alone. I am not sufficiently acquainted with the circumstances of that experiment to argue with certainty about it; but I think it much more likely that the inflammable air was formed by the union of the phlogiston of the iron filings with the water dispersed among them, or contained in the retort or other vessel in which it was heated; and, in all probability this was the cause of the separation of the phlogiston, as iron seems not disposed to part with its phlogiston by heat alone, without being assisted by the air or some other substance.—Orig.



when it was procured from plants or from turbith mineral: and it seems not likely that air procured from plants, and still less likely that air procured from a solution of mercury in oil of vitriol, should contain any nitrous acid. Another strong argument in favour of this opinion is, that dephlogisticated air yields no nitrous acid when phlogisticated by liver of sulphur; for if this air contains nitrous acid, and yields it when phlogisticated by explosion with inflammable air, it is very extraordinary that it should not do so when phlogisticated by other means.

But what forms a stronger, and I think almost decisive argument, in favour of this explanation is, that when the dephlogisticated air is very pure, the condensed liquor is made much more strongly acid by mixing the air to be exploded with a little phlogisticated air, as appears by the following experiments.

A mixture of 18500 grain measures of inflammable air with 9750 of dephlogisticated air, procured from red precipitate, were exploded in the usual manner; after which, a mixture of the same quantities of the same dephlogisticated and inflammable air, with the addition of 2500 of air phlogisticated by iron filings and sulphur, was treated in the same manner. The condensed liquor, in both experiments, was acid, but that in the latter evidently more so, as appeared also by saturating each of them separately with marble powder, and precipitating the earth by fixed alkali; the precipitate of the 2d experiment, weighing  $\frac{1}{4}$  of a grain, and that of the first being several times less. The standard of the burnt air in the 1st experiment was 1.86, and in the 2d only 0.9. It must be observed, that all circumstances were the same in these two experiments, except that in the latter the air to be exploded was mixed with some phlogisticated air, and that in consequence the burnt air was more phlogisticated than in the former; and, from what has been before said, it appears that this latter circumstance ought rather to have made the condensed liquor less acid; and yet it was found to be much more so; which shows strongly that it was the phlogisticated air which furnished the acid.

As a further confirmation of this point, these 2 comparative experiments were repeated with a little variation, namely, in the first experiment there was first let into the globe 1500 of dephlogisticated air, and then the mixture, consisting of 12200 of dephlogisticated air and 25900 of inflammable, was let in at different times as usual. In the 2d experiment, besides the 1500 of dephlogisticated air first let in, there was also admitted 2500 of phlogisticated air, after which the mixture, consisting of the same quantities of dephlogisticated and inflammable air as before, was let in as usual. The condensed liquor of the 2d experiment was about 3 times as acid as that of the first, as it required 119 grs. of a diluted solution of salt of tartar to saturate it, and the other only 37. The standard of the burnt air was 0.78 in the 2d experiment, and 1.96 in the first.



The intention of previously letting in some dephlogisticated air in the last 2 experiments was, that the condensed liquor was thus expected to become more acid, as proved actually to be the case. In the first of these 2 experiments, in order that the air to be exploded should be as free as possible from common air, the globe was first filled with a mixture of dephlogisticated and inflammable air; it was then exhausted, and the air to be exploded let in; by which means, though the globe was not perfectly exhausted, very little common air could be left in it. In the first set of experiments this circumstance was not attended to, and the purity of the dephlogisticated air was forgot to be examined in both sets.

From what has been said there seems the utmost reason to think, that dephlogisticated air is only water deprived of its phlogiston, and that inflammable air, as was before said, is either phlogisticated water, or else pure phlogiston; but in all probability the former.

As Mr. Watt, in a paper lately read before this Society, supposes water to consist of dephlogisticated air and phlogiston deprived of part of their latent heat, whereas I take no notice of the latter circumstance, it may be proper to mention in a few words the reason of this apparent difference between us. If there be any such thing as elementary heat, it must be allowed that what Mr. Watt says is true; but by the same rule we ought to say, that the diluted mineral acids consist of the concentrated acids united to water and deprived of part of their latent heat; that solutions of sal ammoniac, and most other neutral salts, consist of the salt united to water and elementary heat; and a similar language ought to be used in speaking of almost all chemical combinations, as there are very few which are not attended with some increase or diminution of heat. Now I have chosen to avoid this form of speaking, both because I think it more likely that there is no such thing as elementary heat, and because saying so in this instance, without using similar expressions in speaking of other chemical unions, would be improper, and would lead to false ideas; and it may even admit of doubt, whether the doing it in general would not cause more trouble and perplexity than it is worth.

There is the utmost reason to think, that dephlogisticated and phlogisticated air, as M. Lavoisier and Scheele suppose, are quite distinct substances, and not differing only in their degree of phlogistication; and that common air is a mixture of the two; for if the dephlogisticated air is pretty pure, almost the whole of it loses its elasticity by phlogistication, and, as appears by the foregoing experiments, is turned into water, instead of being converted into phlogisticated air. In most of the foregoing experiments, at least  $\frac{1}{7}$  of the whole was turned into water; and by treating some dephlogisticated air with liver of sulphur, I have reduced it to less than  $\frac{1}{36}$  of its original bulk, and other persons, I believe, have reduced it to a still less bulk; so that there seems the utmost reason to suppose,



that the small residuum which remains after its phlogistication proceeds only from the impurities mixed with it. It was just said, that some dephlogisticated air was reduced by liver of sulphur to  $\frac{1}{30}$  of its original bulk; the standard of this air was 4.8, and consequently the standard of perfectly pure dephlogisticated air should be very nearly 5, which is a confirmation of the foregoing opinion; for if the standard of pure dephlogisticated air is 5, common air must, according to this opinion, contain  $\frac{1}{5}$  of it, and therefore ought to lose  $\frac{1}{5}$  of its bulk by phlogistication, which is what it is actually found to lose. From what has been said, it follows, that instead of saying air is phlogisticated or dephlogisticated by any means, it would be more strictly just to say, it is deprived of, or receives, an addition of dephlogisticated air; but as the other expression is convenient, and can scarcely be considered as improper, I shall still frequently make use of it in the remainder of this paper.

There seemed great reason to think, from Dr. Priestley's experiments, that both the nitrous and vitriolic acids were convertible into dephlogisticated air, as that air is procured in the greatest quantity from substances containing those acids, especially the former. The foregoing experiments however seem to show, that no part of the acid is converted into dephlogisticated air, and that their use in preparing it is owing only to the great power which they possess of depriving bodies of their phlogiston. A strong confirmation of this is, that red precipitate, which is one of the substances yielding dephlogisticated air in the greatest quantity, and which is prepared by means of the nitrous acid, contains in reality no acid. This I found by grinding 400 grs. of it with spirits of sal ammoniac, and keeping them together for some days in a bottle, taking care to shake them frequently. The red colour of the precipitate was rendered pale, but not entirely destroyed; being then washed with water and filtered, the clear liquor yielded on evaporation not the least ammoniacal salt.

It is natural to think, that if any nitrous acid had been contained in the red precipitate, it would have united to the volatile alkali, and have formed ammoniacal nitre, and would have been perceived on evaporation; but in order to determine more certainly whether this would be the case, I dried some of the same solution of quicksilver from which the red precipitate was prepared with a less heat, so that it acquired only an orange colour, and treated the same quantity of it with volatile alkali in the same manner as before. It immediately caused an effervescence, changed the colour to grey, and yielded 52 grs. of ammoniacal nitre. There is the utmost reason to think therefore, that red precipitate contains no nitrous acid; and consequently that, in procuring dephlogisticated air from it, no acid is converted into air; and it is reasonable to conclude therefore, that no such change is produced in procuring it from any other substance.



It remains to consider in what manner these acids act in producing dephlogisticated air. The way in which the nitrous acid acts, in the production of it from red precipitate, seems to be as follows. On distilling the mixture of quicksilver and spirit of nitre, the acid comes over, loaded with phlogiston, in the form of nitrous vapour, and continues to do so till the remaining matter acquires its full red colour, by which time all the nitrous acid is driven over, but some of the watery part still remains behind, and adheres strongly to the quicksilver; so that the red precipitate may be considered, either as quicksilver deprived of part of its phlogiston, and united to a certain portion of water, or as quicksilver united to dephlogisticated air;\* after which, on further increasing the heat, the water in it rises deprived of its phlogiston, that is, in the form of dephlogisticated air, and at the same time the quicksilver distils over in its metallic form. It is justly remarked by Dr. Priestley, that the solution of quicksilver does not begin to yield dephlogisticated air till it acquires its red colour. *Mercurius calcinatus* appears to be only quicksilver which has absorbed dephlogisticated air from the atmosphere during its preparation; accordingly, by giving it a sufficient heat, the dephlogisticated air is driven off, and the quicksilver acquires its original form. It seems therefore that *mercurius calcinatus* and red precipitate, though prepared in a different manner, are very nearly the same thing.

From what has been said it follows, that red precipitate and *mercurius calcinatus* contain as much phlogiston as the quicksilver they are prepared from; but yet, as uniting dephlogisticated air to a metal comes to the same thing as depriving it of part of its phlogiston and adding water to it, the quicksilver may still be considered as deprived of its phlogiston; but the imperfect metals seem not only to absorb dephlogisticated air during their calcination, but also to be really deprived of part of their phlogiston, as they do not acquire their metallic form by driving off the dephlogisticated air.

In procuring dephlogisticated air from nitre, the acid acts in a different manner, as on heating the nitre red-hot, the dephlogisticated air rises mixed with a little nitrous acid, and at the same time the acid remaining in the nitre becomes very much phlogisticated; which shows that the acid absorbs phlogiston from the water in the nitre, and becomes phlogisticated, while the water is thereby turned into dephlogisticated air. On distilling 3155 grs. of nitre in an unglazed earthen

\* Unless we were much better acquainted than we are with the manner in which different substances are united together in compound bodies, it would be ridiculous to say, that it is the quicksilver in the red precipitate which is deprived of its phlogiston, and not the water, or that it is the water and not the quicksilver; all that we can say is, that red precipitate consists of quicksilver and water, one or both of which are deprived of part of their phlogiston. In like manner, during the preparation of the red precipitate, it is certain that the acid absorbs phlogiston, either from the quicksilver or the water; but we are by no means authorized to say from which,—Orig.



retort, it yielded 256000 gr. measures of dephlogisticated air,\* the standard of different parts of which varied from 3 to 3.65, but at a medium was 3.35. The matter remaining in the retort dissolved readily in water, and tasted alkaline and caustic. On adding diluted spirit of nitre to the solution, strong red fumes were produced; a sign that the acid in it was very much phlogisticated, as no fumes whatever would have been produced on adding the same acid to a solution of common nitre; that part of the solution also which was supersaturated with acid became blue; a colour which the diluted nitrous acid is known to assume when much phlogisticated. The solution, when saturated with this acid, lost its alkaline and caustic taste, but yet tasted very different from true nitre, seeming as if it had been mixed with sea-salt, and also required much less water to dissolve it; but on exposing it for some days to the air, and adding fresh acid as fast as, by the flying off of the fumes, the alkali predominated, it became true nitre, unmixed, as far as I could perceive, with any other salt.†

It has been remarked, that the dephlogisticated air procured from nitre is less pure than that from red precipitate, and many other substances; which may perhaps proceed from unglazed earthen retorts having been commonly used for this purpose, and which, conformably to Dr. Priestley's discovery, may possibly absorb some common air from without, and emit it along with the dephlogisticated air; but if it should be found that the dephlogisticated air procured from nitre, in glass or glazed earthen vessels, is also impure, it would seem to show that part of the acid in the nitre is turned into phlogisticated air, by absorbing phlogiston from the watery part.

From what has been said it appears, that there is a considerable difference in the manner in which the acid acts in the production of dephlogisticated air from red precipitate and from nitre; in the former case the acid comes over first, leaving the remaining substance deprived of part of its phlogiston; in the latter the dephlogisticated air comes first, leaving the acid loaded with the phlogiston of the water from which it was formed. On distilling a mixture of quicksilver and oil of vitriol to dryness, part of the acid comes over, loaded with phlogiston, in the form of volatile sulphureous acid and vitriolic acid air; so that the remaining white mass may be considered as consisting of quicksilver deprived of its phlogiston, and united to a certain proportion of acid and water, or of plain quicksilver united to a certain proportion of acid and dephlogisticated air. Accordingly on urging this white mass with a more violent heat, the dephlogisticated air

\* This is, about 81 gr. measures from 1 gr. of nitre; and the weight of the dephlogisticated air, supposing it 800 times lighter than water, is  $\frac{1}{16}$  of that of the nitre. In all probability it would have yielded a much greater quantity of air, if a greater heat had been applied.—Orig.

† This phlogistication of the acid in nitre by heat has been observed by Mr. Scheele; see his experiments on air and fire, p. 45, English translation.—Orig.



comes over, and at the same time part of the quicksilver rises in its metallic form, and also part of the white mass, united in all probability to a greater proportion of acid than before, sublimes; so that the rationale of the production of dephlogisticated air from turbith mineral, and from red precipitate, are nearly similar.

True turbith mineral consists of the above-mentioned white mass, well washed with water, by which means it acquires a yellow colour, and contains much less acid than the unwashed mass. Accordingly it seems likely, that on exposing this to heat, less of it should sublime without being decomposed, and consequently that more dephlogisticated air should be procured from it than from the unwashed mass. This is an instance, that the superabundant vitriolic acid may, in some cases, be better extracted from the base it is united to by water than by heat. Vitriolated tartar is another instance; for, if vitriolated tartar be mixed with oil of vitriol and exposed even to a pretty strong red heat, the mass will be very acid; but if this mass be dissolved in water, and evaporated, the crystals will be not sensibly so.

In all probability, the vitriolic acid acts in the same manner in the production of dephlogisticated air from alum, as the nitrous does in its production from nitre; that is, the watery part comes over first in the form of dephlogisticated air, leaving the acid charged with its phlogiston. Whether this is also the case with regard to green and blue vitriol, or whether in them the acid does not rather act in the same manner as in turbith mineral, I cannot pretend to say, but I think the latter more likely. There is another way by which dephlogisticated air has been found to be produced in great quantities, namely, the growth of vegetables exposed to the sun or day-light; the rationale of which probably is, that plants, when assisted by the light, deprive part of the water sucked up by their roots of its phlogiston, and turn it into dephlogisticated air, while the phlogiston unites to, and forms part of, the substance of the plant.

There are many circumstances which show, that light has a remarkable power in enabling one body to absorb phlogiston from another. Mr. Senebier has observed, that the green tincture procured from the leaves of vegetables by spirit of wine quickly loses its colour when exposed to the sun in a bottle not more than  $\frac{1}{3}$  part full, but does not do so in the dark, or if the bottle is quite full of the tincture, or if the air in it is phlogisticated; whence it is natural to conclude, that the light enables the dephlogisticated part of the air to absorb phlogiston from the tincture; and this appears to be really the case, as I find that the air in the bottle is thus considerably phlogisticated. Dephlogisticated spirit of nitre also acquires a yellow colour, and becomes phlogisticated, by exposure to the sun's rays;\* and I find on trial that the air in the bottle in which it is contained

\* If spirit of nitre is distilled with a very gentle heat, the part which comes over is high coloured and fuming, and that which remains behind is quite colourless, and fumes much less than other ni-



becomes dephlogisticated, or, in other words, receives an increase of dephlogisticated air; which shows that the change in the acid is not owing to the sun's rays communicating phlogiston to it, but to their enabling it to absorb phlogiston from the water contained in it, and so to produce dephlogisticated air. Mr. Scheele also found, that the dark colour acquired by luna cornea on exposure to the light, is owing to part of the silver being revived; and that gold, dissolved in aqua regia and deprived by distillation of the nitrous and superfluous marine acid, is revived by the same means; and there is the utmost reason to think that, in both cases, the revival of the metal is owing to its absorbing phlogiston from the water.

Vegetables seem to consist almost entirely of fixed and phlogisticated air, united to a large proportion of phlogiston and some water; since by burning in the open air, in which their phlogiston unites to the dephlogisticated part of the atmosphere and forms water, they seem to be reduced almost entirely to water and those two kinds of air. Now plants growing in water without earth, can receive nourishment only from the water and air, and must therefore in all probability absorb their phlogiston from the water. It is known also that plants growing in the dark do not thrive well, and grow in a very different manner from what they do when exposed to the light.

From what has been said, it seems likely that the use of light, in promoting the growth of plants and the production of dephlogisticated air from them, is, that it enables them to absorb phlogiston from the water. To this it may perhaps be objected, that though plants do not thrive well in the dark, yet they do grow; and should therefore, according to this hypothesis, absorb water from the atmosphere, and yield dephlogisticated air, which they have not been found to do. But we have no proof that they grew at all in any of those cases in which they were found not to yield dephlogisticated air; for though they will grow in the dark, yet their vegetative powers may perhaps at first be entirely checked by it, especially considering the unnatural situation in which they must be placed in such experiments. Perhaps too plants growing in the dark may be able to absorb phlogiston from water not much impregnated with dephlogisticated air, but not from water strongly impregnated with it; and consequently, when kept under water in the dark, may perhaps at first yield some dephlogisticated air, which, instead of rising to the surface, may be absorbed by the water, and before the water is so much impregnated as to suffer any to escape, the plant may cease to vegetate, unless the water is changed. Unless therefore it could be

trous acid of the same strength, and the fumes are colourless. This is called dephlogisticated spirit of nitre, as it appears to be really deprived of phlogiston by the process. The manner of preparing it, as well as its property of regaining its yellow colour by exposure to the light, is mentioned by Mr. Scheele in the Stockholm Memoirs, 1774.—Orig.



shown that plants growing in the dark, in water alone, will increase in size, without yielding dephlogisticated air, and without the water becoming more impregnated with it than before, no objection can be drawn from thence. Mr. Senebier finds, that plants yield much more dephlogisticated air in distilled water impregnated with fixed air, than in plain distilled water, which is perfectly conformable to the above-mentioned hypothesis; for as fixed air is a principal constituent part of vegetable substances, it is reasonable to suppose that the work of vegetation will go on better in water containing this substance, than in other water.

There are several memoirs of Mr. Lavoisier published by the Academy of Sciences, in which he entirely discards phlogiston, and explains those phenomena which have been usually attributed to the loss or attraction of that substance, by the absorption or expulsion of dephlogisticated air; and as not only the foregoing experiments, but most other phenomena of nature, seem explicable as well, or nearly as well, on this as on the commonly believed principle of phlogiston, it may be proper briefly to mention in what manner I would explain them on this principle, and why I have adhered to the other. In doing this, I shall not conform strictly to his theory, but shall make such additions and alterations as seem to suit it best to the phenomena; the more so, as the foregoing experiments may perhaps induce the author himself to think some such additions proper.

According to this hypothesis, we must suppose that water consists of inflammable air united to dephlogisticated air; that nitrous air, vitriolic acid air, and the phosphoric acid, are also combinations of phlogisticated air, sulphur, and phosphorus, with dephlogisticated air; and that the two former, by a further addition of the same substance, are reduced to the common nitrous and vitriolic acids; that the metallic calces consist of the metals themselves united to the same substance, commonly however with a mixture of fixed air; that on exposing the calces of the perfect metals to a sufficient heat, all the dephlogisticated air is driven off, and the calces are restored to their metallic form; but as the calces of the imperfect metals are vitrified by heat, instead of recovering the metallic form, it should seem as if all the dephlogisticated air could not be driven off from them by heat alone. In like manner, according to this hypothesis, the rationale of the production of dephlogisticated air from red precipitate is, that during the solution of the quicksilver in the acid and the subsequent calcination, the acid is decomposed, and quits part of its dephlogisticated air to the quicksilver, whence it comes over in the form of nitrous air, and leaves the quicksilver behind united to dephlogisticated air, which, by a further increase of heat, is driven off, while the quicksilver re-assumes its metallic form. In procuring dephlogisticated air from nitre, the acid is also decomposed; but with this dif-



ference, that it suffers some of its dephlogisticated air to escape, while it remains united to the alkali itself, in the form of phlogisticated nitrous acid. As to the production of dephlogisticated air from plants, it may be said, that vegetable substances consist chiefly of various combinations of 3 different bases, one of which, when united to dephlogisticated air, forms water, another fixed air, and the third phlogisticated air; and that by means of vegetation each of these substances are decomposed, and yield their dephlogisticated air; and that in burning they again acquire dephlogisticated air, and are restored to their pristine form.

It seems therefore from what has been said, as if the phenomena of nature might be explained very well on this principle, without the help of phlogiston; and indeed, as adding dephlogisticated air to a body comes to the same thing as depriving it of its phlogiston and adding water to it, and as there are perhaps no bodies entirely destitute of water, and as I know no way by which phlogiston can be transferred from one body to another, without leaving it uncertain whether water is not at the same time transferred, it will be very difficult to determine by experiment which of these opinions is the truest; but as the commonly received principle of phlogiston explains all phenomena, at least as well as Mr. Lavoisier's, I have adhered to that. There is one circumstance also, which though it may appear to many not to have much force, I own has some weight with me; it is, that as plants seem to draw their nourishment almost entirely from water and fixed and phlogisticated air, and are restored back to those substances by burning, it seems reasonable to conclude that, notwithstanding their infinite variety, they consist almost entirely of various combinations of water and fixed and phlogisticated air, united according to one of these opinions to phlogiston, and deprived according to the other of dephlogisticated air; so that, according to the latter opinion, the substance of a plant is less compounded than a mixture of those bodies into which it is resolved by burning; and it is more reasonable to look for great variety in the more compound than in the more simple substance.

Another thing which Mr. Lavoisier endeavours to prove is, that dephlogisticated air is the acidifying principle. From what has been explained it appears, that this is no more than saying, that acids lose their acidity by uniting to phlogiston, which, with regard to the nitrous, vitriolic, phosphoric, and arsenical acids, is certainly true. The same thing, I believe, may be said of the acid of sugar; and Mr. Lavoisier's experiment is a strong confirmation of Bergman's opinion, that none of the spirit of nitre enters into the composition of the acid, but that it only serves to deprive the sugar of part of its phlogiston. But as to the marine acid and acid of tartar, it does not appear that they are capable of losing their acidity by any union with phlogiston. It is to be remarked also, that the acids of sugar and tartar, and in all probability almost all the vegetable and



animal acids, are by burning reduced to fixed and phlogisticated air, and water, and therefore contain more phlogiston, or less dephlogisticated air, than those three substances.

*XIV. Remarks on Mr. Cavendish's Experiments on Air. By R. Kirwan, Esq., F.R.S. p. 154.*

Having listened with much attention, and derived much useful information from the very curious experiments of Mr. Cavendish, it is with peculiar regret (says Mr. K.) I feel myself withheld from yielding an entire assent to all he has advanced in his paper; and it is with still greater that I find myself obliged, by reason of the opposition of some of his deductions to those I had the honour to lay before the Society about 2 years ago, to expose the reasons of my dissent from them. In my paper I attributed the diminution of respirable air, observed in common phlogistic processes, to the generation and absorption of fixed air, which is now known to be an acid, and capable of being absorbed by several substances. That fixed air was somehow produced in phlogistic processes, either by separation or composition, I took for granted from the numerous experiments of Dr. Priestley; and among these I selected, as least liable to objection, the calcination of metals, the decomposition of nitrous by mixture with respirable air, the phlogistication of respirable air by the electric spark, and, lastly, that effected by amalgamation. In each of these instances Mr. Cavendish is of opinion, that the diminution of respirable air is owing to the production of water, which, according to him, is formed by the union of the phlogiston, disengaged in those processes, with the dephlogisticated part of common air; and that fixed air is never produced in phlogistic processes, except some animal or vegetable substance is concerned in the operation, from whose decomposition it may arise. To which of these causes the diminution of respirable air is to be attributed, I shall now endeavour to elucidate.

*Of the calcination of metals.*—I attributed the diminution of air by the calcination of metals, to the conversion of the dephlogisticated part of common air into fixed air, by reason of its union with the phlogiston of the metal, for this plain reason; because I find it acknowledged on all hands, that the calces of all the base metals yield fixed air, when sufficiently heated. Mr. Cavendish allows the fact in general, but ascribes the fixed air found in them to their long exposure to the atmosphere, in which he says fixed air pre-exists; but that it exists in common air in any quantity worth attending to, or is extracted from it in any degree, I take the liberty of denying, grounded on the following facts. First, I have frequently agitated 18 cubic inches of common air in 2 of lime-water, and 2 of common air in 18 of lime-water, but could never perceive the slightest milkiness; and yet the 1000th part of a cubic inch of fixed air would thus be



made sensible; for if a cubic inch of it be dissolved in 3 oz. of water, a few drops of that water let into lime-water will produce a cloud. Mr. Fontana says, he frequently agitated 1 cubic inch of the tincture of turnsole in 7 or 800 of common air, without reddening it (23 Roz. p. 188 ;) and yet, according to Mr. Bergman, 1 cubic inch of fixed air is sufficient to redden 50 of tincture of turnsole (1 Bergm. 11 ;) whence I am apt to think, that 700 cubic inches of common air do not even contain  $\frac{1}{50}$  of a cubic inch of fixed air. Dr. Whytt found that 12 oz. of strong lime-water, being exposed to the open air for 19 days, still retained about 1 grain of lime, (on Lime-water, p. 32.) Now 12 oz. of strong lime-water contain at most 9.5 grs. of lime; and 1 grain of lime requires only 0.56 of a cubic inch of fixed air to precipitate it, the thermometer at 55 and the barometer at 29.5, as I have found. Therefore in 19 days this lime-water did not come in contact with more than 4 cubic inches of fixed air; yet it is certain that a large quantity of fixed air is continually disengaged, and thrown into the atmosphere, by various processes, as putrefaction, combustion, &c. but it seems equally certain that it is either decomposed, or more probably absorbed by various bodies. Mr. Fontana let loose 20000 cubic inches of fixed air, in a room whose windows and doors were closed, yet in half an hour after he could not discover the least trace of it (ibid.) Though fixed air perpetually oozes from the floor of the Grotto del Cane, yet at the distance of 4 or 5 feet from the ground none is found; animals may live, lights burn, &c. (Roz. Ibid. Mem. Stockh. 1775). If distilled water be exposed to the atmosphere, it is never found to absorb fixed air, but rather dephlogisticated air, according to Mr. Scheele's experiments, which could never happen if the atmosphere contained any sensible proportion of fixed air; nor has rain-water been ever found to contain any, which it certainly should on the same hypothesis; even Mr. Cavendish himself could find no fixed air in the residuum or products of about 1040 oz. measures of common air, which he burnt with inflammable air. It is true, Dr. Priestley supposed common air to contain  $\frac{1}{55}$  of its bulk of fixed air; but he drew this conclusion not from any direct experiment, but from the quantity of fixed air produced by breathing, which he at that time believed to have been barely precipitated, and not generated, an opinion which he has found reason to alter from his own experiments. I think I may therefore conclude, that the quantity of fixed air contained in the atmosphere is absolutely inappreciable.

Secondly, supposing the atmosphere to contain a very small proportion of fixed air, yet I do not think it can be inferred that metals, during their calcination, extract any, because I find that lime exposed to red heat ever so long, extracts none, though it is formed by a calcination in open air, which lasts at least as long as that of any metal; neither does precipitate per se attract any, though its calcination lasts several months; nor does this proceed from the want of



affinity, for if a saturate solution of mercury in any of the acids be precipitated by a mild vegetable alkali, very little effervescence is perceived, and the precipitate weighs much more than the quantity of mercury employed, and that this increase of weight arises in part from the fixed air absorbed will presently be seen.

Since then metals may be calcined in close vessels; since they then absorb a 4th part of the common air to which they are exposed; since all metallic calces (except those of mercury, which I shall presently mention) yield fixed air; since common air contains scarce any fixed air; is it not apparent that the fixed air thus found was generated by the very act of calcination, by the union of the phlogiston of the metal with the dephlogisticated part of the common air, since after the operation the metal is deprived of its phlogiston, and the air of its dephlogisticated part? But Mr. Cavendish objects, that no one has extracted fixed air from metals calcined in close vessels. To which I answer, that this further proof is difficult, and no way necessary; it is difficult, because the operation can easily be performed only on small quantities; it is unnecessary, because it differs from the operation in open air only by the quantities of the materials employed; in every other respect it is exactly the same. Since Mr. Cavendish suspects the results are different, it is incumbent on him to show that difference; but till then, according to Sir Isaac Newton's second rule, "to natural effects of the same kind the same causes are to be assigned, as far as it may be done," that is, till experience points out some other cause.

It may further be urged, that precipitate per se yields only dephlogisticated air, that minium also yields a large proportion of it. This difficulty I have formerly answered by asserting, that these calces are in fact united only to fixed air, and that they yield dephlogisticated air, merely because the fixed air is decomposed by the total or partial revivification of the metallic substances; this I think may be demonstrated by the following experiments. Let sublimate corrosive singly be treated in any manner, it will not yield dephlogisticated air (4 Pr. 240;) but let a solution of sublimate corrosive be precipitated by a mild fixed alkali, this precipitate washed, dried, and distilled in a pneumatic apparatus, will yield dephlogisticated air, and the mercury will be revived; but, if the solution of sublimate corrosive be precipitated by lime-water, it seems no air will be produced. Here then we see, 1st, that the calx of mercury unites with fixed air; and, 2dly, that this fixed air is, during the revivification of the mercury, converted into dephlogisticated air. Again: let 1 oz. of red precipitate, which, according to Mr. Cavendish, contains no nitrous acid, be distilled with 2 oz. of iron filings; this quantity of precipitate, which, if distilled by itself, would yield 60 oz. measures of dephlogisticated air, will, when distilled with this proportion of iron filings, yield 40 oz. measures of fixed air, as Dr. Priestley has shown in his last



paper: whichever way this is explained, some or other of my opinions are confirmed; for either the mercurial calx is already combined with fixed air, which I believe to be the case, and this air passes undecomposed, because the mercury extracts phlogiston from the iron; or it contains dephlogisticated air, which is converted into fixed air by its union with the phlogiston of the iron.

If precipitate per se be digested in marine acid, the mercury will be revived (3 Bergm. 415.) Now this calx does not dephlogisticate the marine acid; for this acid, when dephlogisticated, dissolves mercury; how then does it revive it, if not by expelling the fixed air contained in it, which in the moment of its expulsion is decomposed, leaving its phlogiston to the mercury, which is thus revived? Again: if litharge be heated in a gun-barrel, it will afford more fixed and less dephlogisticated air, than if heated in glass or earthen vessels. Does not this happen, because the calx of lead, receiving some phlogiston from the metal, does not dephlogisticate so great a proportion of the fixed air as it otherwise would?

Further: there is no substance which yields dephlogisticated air, but yields also fixed air, even precipitate per se not excepted (3 Priest. 16;) and, what is remarkable, they all yield fixed air first, and dephlogisticated air only towards the end of the process. Does not this happen because metallic calces attract phlogiston so much more strongly, as they are more heated? Thus many calci-form iron ores become magnetic by calcination, though they were not so before; so also do all the calces of iron when exposed to the focus of a burning glass (5 Dict. Chy. 179.) Thus mercury cannot be calcined but in a heat inferior to that in which it boils; thus minium cannot be formed but in a moderate heat; and if heated still more, it returns to the state of massicot, in which it was before it became minium, and much of it is reduced. So if a solution of luna cornea in volatile alkali be triturated with mercury, the silver will be revived, and the marine acid unite to the mercury; which shows this acid has a stronger attraction to mercury than to silver; yet if sublimate corrosive and silver be distilled in a strong heat, the mercury will be revived, and the marine acid unite to the silver; which shows that the attraction of mercury to phlogiston increases with the heat applied.

Before concluding this head, I will mention another experiment, which I think decisive in favour of my opinion of the composition of fixed air. If filings of zinc be digested in a caustic fixed alkali in a gentle heat, the zinc will be dissolved with effervescence, and the alkali will be rendered in great measure mild. But if, instead of filings of zinc, flowers of zinc be used, and treated in the same manner, there will be no solution, and the alkali will remain caustic. In the first case the effervescence arises from the production of inflammable air, which phlogisticates the common air contiguous to it, and produces fixed air,



which is immediately absorbed by the alkali, and renders it mild. In the second case, no inflammable air is produced, the common air is not phlogisticated, and consequently the alkali remains caustic.\* This experiment also proves that metallic calces attract fixed air more strongly than alkalis attract it; for the calces of zinc are known to contain fixed air, and yet alkalis digested with them remain caustic; and this accounts for the slight turbidity of lime-water when metals are calcined over it; for as soon as the phlogiston is disengaged from the metal, and before it has absorbed the whole quantity of fire requisite to throw it into the form of inflammable air, it meets with the dephlogisticated part of the common air on the surface of the metal, and there forms fixed air, which is instantly absorbed by the calx with which it is in contact; so that it is not to be wondered that it does not unite to the lime from which it is distant.

*Of the decomposition of nitrous air by mixture with common air.*—As soon as I had heard Mr. Cavendish's paper read, I set about trying whether lime would be precipitated from lime-water during the process, an experiment I had never made before with common air, taking it for granted that it was so, from the repeated experiments of Dr. Priestley, and indeed of all others who had treated this subject:† and, in effect, when I made the experiment with nitrous air prepared and confined by the water of my tub, I found lime-water admitted to it instantly precipitated. But after I had read Mr. Cavendish's paper, which he had the politeness to permit me, and had, according to his direction, received the nitrous air over lime-water, I did not then perceive the least milkiness after admitting common air. After 12 hours I indeed perceived a whitish dust, on the bottom of the glass vessel in which the experiment was made, which I cannot assure to be calcareous; and, on breathing into the lime-water, an evident milkiness ensued; so that I little doubt but the precipitation I observed in the first experiment arose from the decomposition of the aerial selenite contained in the water of the tub. And it is very possible that the precipitation of lime, which I perceived some years ago on mixing dephlogisticated air and nitrous air, might have arisen from the same cause, or from fixed air pre-contained in the dephlogisticated, as this last had not been washed in lime-water. Yet I do not think the failure of this experiment at all conclusive against the supposed production of fixed air on this occasion, because the quantity of fixed air is so small, that it may well be supposed to unite to the nitrous selenite formed in the lime-water. It is well known that a small quantity of fixed air is capable of uniting to all neutral salts: thus Dr. Priestley has extracted it from tartar vitriolate and alum (2 Pr. 115, 116,) and gypsum, (2 Pr. 80;) and Dr. Macbride found it in

\* See Mr. Lassone's Experiments on zinc. Mem. Par. 1777, p. 7 and 8.—Orig.

† See 1 Pr. 114, 189. 2 Pr. 218. Font. Recherches Phys. p. 77. 1 Chy. Dij. 324.—Orig.



nitre and common salt, though in small quantity. But to try whether nitrous selenite would attract any, I made a solution of chalk in nitrous acid, which, when saturated, weighed 381.25 grs.; but being exposed to the air for a few hours, it weighed 382.25. I afterwards took a very dilute nitrous acid, in which an acid taste was barely perceptible, and impregnated it with a very small proportion of fixed air, and then let fall a few drops of it into lime-water; not the smallest cloud was perceived, and yet when I breathed into it afterwards it became milky in a few seconds; so that this experiment is perfectly analogous to that in which nitrous and common air were mixed.

But if nitrous air and common air be mixed over dry mercury, the result is entirely adverse to the opinion of Mr. Cavendish, and favourable to mine; for in this case the common air is not at all diminished till water is admitted to it, and the mixture agitated a few minutes, and then the diminution is nearly the same as if the mixture were made over water. Thus, when I mixed 2 cubic inches of common air with 1 of nitrous air, they occupied the space of  $2\frac{1}{8}$  inches, and the surface of the mercury was immediately calcined; which shows that the inch of nitrous air was decomposed, and produced nitrous acid; but the common air was undiminished; and the  $\frac{1}{8}$  of an inch over and above the 2 inches of common air, proceeded from an addition of new nitrous air, formed by the corrosion of the surface of the mercury. That the common air should remain undiminished, is easily explained in my system, because fixed air is formed, which, on this occasion, must remain unabsorbed, at least for a long time, as there is nothing at hand that can immediately receive it; and hence, if water be admitted soon after the mixture of both airs, the diminution will be nearly the same as if the mixture had been originally made over water, though not exactly the same; because the nitrous air, produced by the union of the newly formed nitrous acid with the mercury, is not entirely absorbable by water. But, in Mr. Cavendish's hypothesis, the common air should be diminished just as much as if the mixture were made over water; for, according to him, this diminution arises from the conversion of the dephlogisticated part of the common air into water, which water should immediately unite to the nitrous salt of mercury, and leave the common air lessened in its bulk by a portion commensurate to that converted into water; or, if he will not allow the water to have immediately united to the mercurial salt, at least by the difference of the bulk of the water produced, and that of an equal weight of the common air converted into it: but neither happens; for the common air is not at all diminished; nor can he explain, consistently with his system, why the admission of water should immediately produce a diminution in the common air, as, according to him, it contains nothing that can be absorbed. Dr. Priestley has remarked, that if a mixture of both airs be suffered to stand several hours, even the admission of water will produce no diminution.



This is owing to 2 causes, 1st, because a large quantity of nitrous air is produced, by the continued action of the concentrated nitrous acid newly formed; and, 2dly, because the fixed air, on whose absorption the diminution depends, is absorbed by the mercurial salt, as may be inferred from the experiment in Lavoisier, p. 248.

*Of the diminution of common air by the electric spark.*—Of all the instances of the artificial production of fixed air, by the union of phlogiston with the de-phlogisticated part of common air, there is none perhaps so convincing as that exhibited by taking the electric spark through common air, over a solution of litmus, or lime-water; for the common air is diminished  $\frac{1}{4}$ , the litmus reddened, and the lime-water precipitated. Mr. Cavendish indeed attributes the redness of the litmus to fixed air; but he thinks it proceeds from a decomposition of some part of the vegetable juice, as all vegetable juices contain fixed air. Yet that such a decomposition does not take place, I think may be inferred from the following reasons: first, if the electric spark be taken through phlogisticated or inflammable air confined by litmus, no redness is produced, the air not being in the least diminished; and, 2dly, if the litmus were decomposed, inflammable air should be produced as well as fixed air; and then there should be an addition of bulk, instead of a diminution; but what sets the origin of the fixed air from the phlogistication of the common air beyond all doubt is, that if lime-water be used instead of litmus, the diminution is the same, and the lime is precipitated. Here Mr. Cavendish says, the fixed air proceeds either from some dirt in the tube; a supposition which, being neither necessary nor probable, is not admissible; or else from some combustible matter in the lime; but lime contains no combustible matter, except perhaps phlogiston, which cannot produce fixed air unless by uniting to the common air, according to my supposition; but it is much more probable that the diminution does not arise from any phlogiston in the lime, as it is exactly the same whether lime-water be used or not; and the lime does not appear to be in the least altered, and in fact contains scarcely any phlogiston.

*Of the diminution of common air, by the amalgamation of mercury and lead.*—I attributed this diminution to the phlogistication of the common air by the process of amalgamation, and the consequent production and absorption of fixed air. On this Mr. Cavendish observes, “that mercury, fouled by the addition of lead or tin, deposits a powder which consists in great measure of the calx of the metal; he found also, that some powder of this sort contained fixed air; but it is not clear that this air was produced by the phlogistication of the air in which the mercury was shaken, as the powder was not prepared on purpose, but was formed from mercury fouled by having been used for various purposes, and may therefore contain other impurities, besides the metallic calx.” On this I



remark, that Dr. Priestley did not indeed at first prepare this powder on purpose; but he afterwards did so prepare it (4 Priest. p. 148, 149) and obtained a powder exactly of the same sort; and it is certain that the fixed air found in it proceeded from the common air, both because metallic calces, not formed by amalgamation, will not unite with mercury, as is well known; and because this calx cannot be formed by agitation of the mercury and lead, in phlogisticated, inflammable, or any other air which is not respirable; and the fixed air cannot proceed from any impurity, as mercury will not unite in its running form to any other but metallic substances, which it always partially dephlogisticates, like other menstruums (3 Chy. Dijon, 425).

*Of the diminution of respirable air by combustion.*—Though I have no doubt but the diminution of respirable air, by the combustion of sulphur and phosphorus, proceeds also in great measure from the production and absorption of fixed air, yet I avoided mentioning this operation, as the presence of a stronger acid renders the presence of a weaker impossible to be proved, more especially as both these acids precipitate lime from lime-water; but the great increase of weight which the phosphoric acid gains is a strong additional inducement to think that it absorbs fixed air. During the combustion of vegetable substances, I think it highly probable that fixed air is formed, both from my own experiments on the combustion of wax candles, and that mentioned in the 1st volume of Dr. Priestley's Observations, p. 136; but when inflammable air from metals and dephlogisticated air are fired, as a great diminution takes place, and yet no fixed air is found, I am nearly convinced, by Mr. Cavendish's experiments, that water is really produced; nor am I surprized that, in this instance, the union of phlogiston and dephlogisticated air should form a compound very different from that which it forms in other instances of phlogistication, but should rather be led to expect it a priori; for in this case the phlogiston is in its most rarefied known state, and unites to dephlogisticated air, the substance to which it has the greatest affinity, in circumstances the most favourable to the closest and most intimate union; for both, in the act of inflammation, are rarefied to the highest degree; both give out their specific fire, the great obstacle to their union, it being by the inflammation converted into sensible heat (a circumstance which, in my opinion, constitutes the very essence of flame); the resulting compound having then lost the greatest part of its specific fire, is necessarily reduced, according to Dr. Black's theory, into a denser state, which the present experiment shows to be water; whereas, in common cases of combustion, the phlogiston being denser and less divided, unites less intimately with the dephlogisticated part of common air, consequently expels less of its specific fire, and therefore forms less dense compounds, viz. fixed and phlogisticated airs; and so much the more, as a great part entirely escapes combustion; but it seems probable that in



very strong and bright inflammations, the union is more perfect, and water formed.

Water being then the result of the closest and most intimate union of dephlogisticated air and phlogiston, it seems to me very improbable, that it is ever decomposed by the affinity of any acid to phlogiston, as all the experiments hitherto made seem to prove, that phlogiston has a stronger affinity to dephlogisticated air than to any other substance, except hot metallic calces; and these, in my opinion, are incapable of forming any union with water, except as far as they are saline; but they never can be reduced by it. So also water is incapable of uniting with any more phlogiston, as sulphur is, both being already saturated.

Mr. Cavendish is inclined to think, that pure inflammable air is not pure phlogiston, because it does not immediately unite with dephlogisticated air, when both airs are simply mixed with each other; this reason seems to me of no moment, because I see several other substances, having the strongest affinity to each other, that refuse to unite suddenly, or even at all, from the very same cause that dephlogisticated and inflammable airs refuse to unite; viz. on account of the specific fire which they contain, and must lose, before such union can take place: thus fixed air will never unite to dry lime, though they be kept ever so long together; thus, if water be poured on the strongest oil of vitriol, they will remain several weeks in contact, without uniting, as I have experienced; and yet, in both cases, the specific fire need be expelled only from one of the substances, and not from both: but after a long time they will unite; so also will inflammable and dephlogisticated air, as Dr. Priestley has discovered since his last publication.

That phlogisticated air should consist of supersaturated nitrous air I think improbable, as it retains its phlogiston much more strongly than nitrous air, which, according to the general laws of affinities, it should not, if it contained an excess of phlogiston; and as Dr. Priestley and Mr. Fontana repeatedly assure us, they have converted it into common air, by washing it in water, in contact with the atmosphere.

*XV. Mr. Cavendish's Answer to the foregoing Remarks. p. 170.*

In my paper containing many experiments on air, I gave, says Mr. C., my reasons for supposing that the diminution which respirable air suffers by phlogistication, is not owing either to the generation or separation of fixed air from it; but without any arguments of a personal nature, or which related to any one person who espouses the contrary doctrine more than to another. This being contrary to the opinion maintained by Mr. Kirwan, he has written a paper in answer to it. As I do not like troubling the Society with controversy, I shall take no notice of the arguments used by him, but shall leave them for the reader



to form his own judgment of; much less will I endeavour to point out any inconsistencies or false reasonings, should any such have crept into it; but as there are two or three experiments mentioned there, which may perhaps be considered as disagreeing with my opinion, I beg leave to say a few words concerning them.

Mr. De Lassone found that filings of zinc, digested in a caustic fixed alkali, were partially dissolved with a small effervescence, and that the alkali was rendered in some measure mild. This mildness of the alkali Mr. Kirwan accounts for by supposing that the inflammable air, which is separated during the solution, and causes the effervescence, unites to the atmospheric air contiguous to it, and thus generates fixed air, which is absorbed by the alkali. But, in reality, the only circumstance from which Mr. de Lassone judged the alkali to become mild, was its making some effervescence when saturated with acids; and this effervescence is more likely to have proceeded from the expulsion of inflammable air than of fixed air, as it seems likely, that the zinc might be more completely deprived of its phlogiston by the acid than by the alkali.

In the above-mentioned paper I say, Dr. Priestley observed, that quicksilver fouled by the addition of lead or tin, deposits a powder by agitation and exposure to the air, which consists in great measure of the calx of the imperfect metal. He found too some powder of this kind to contain fixed air; but it must be observed, that the powder used in this experiment was not prepared on purpose, but was procured from quicksilver fouled by having been used in various experiments, and may therefore have contained other impurities besides the metallic calces. On this Mr. Kirwan remarks, that Dr. Priestley did not at first prepare this powder on purpose, but he afterwards did so prepare it (4 Priest. p. 148 and 149), and obtained a powder exactly of the same sort. It was natural to suppose from this remark, that Dr. Priestley must have obtained fixed air from the powder prepared on purpose, and that I had overlooked the passage; but, on turning to the pages referred to, I was surprized to find that it was otherwise, and that Dr. Priestley not so much as hints that he procured fixed air from the powder thus prepared.

With regard to the calcination of metals it may be proper to remark, that this operation is usually performed over the fire, by methods in which they are exposed to the fumes of the burning fuel, and which are so replete with fixed air, that it is not extraordinary, that the metallic calx should, in a short time, absorb a considerable quantity of it; and in particular red lead, which is the calx on which most experiments have been made, is always so prepared. There is another kind of calcination however, called rusting, which is performed in the open air; but this is so slow an operation, that the rust may easily imbibe a sufficient quantity of fixed air, notwithstanding the small quantity of it usually contained in the atmosphere.



Mr. Kirwan allows that lime-water is not rendered cloudy by the mixture of nitrous and common air; but contends that this does not prove that fixed air is not generated by the union, as he thinks it may be absorbed by the nitrous selenite produced by the union of the nitrous acid with the lime. This induced me to try how small a quantity of fixed air would be perceived in this experiment. I accordingly repeated it in the same manner as described in my paper, except that I purposely added a little fixed air to the common air, and found that when this addition was  $\frac{1}{75}$  of the bulk, or  $\frac{1}{50}$  of the weight of the common air, the effect on the lime-water was such as could not possibly have been overlooked in my experiments. But as those who suppose fixed air to be generated by the mixture of nitrous and common air, may object to this manner of trying the experiment, and say, that the quantity of fixed air absorbed by the lime-water was really more than  $\frac{1}{75}$  of the bulk of the common air, being equal to that quantity over and above the air generated by the mixture, I made another experiment in a different manner; namely, I filled a bottle with lime-water, previously mixed with as much nitrous acid as is contained in an equal bulk of nitrous air, and having inverted it into a vessel of the same, let up into it, in the same manner as in the above-mentioned experiments, a mixture of common air with  $\frac{1}{75}$  of its bulk of fixed air, till it was half full. The event was the same as before; namely, the cloudiness produced in the lime-water was such that I could not possibly have overlooked. It must be observed, that in this experiment no fixed air could be generated, and a still greater proportion of the lime-water was turned into nitrous selenite than in the above-mentioned experiments; so that we may safely conclude, that if any fixed air is generated by the mixture of common and nitrous air, it must be less than  $\frac{1}{75}$  of the bulk of the common air.

As for the nitrous selenite, it seems not to make the effect of the fixed air at all less sensible, as I found by filling two bottles with common air mixed with  $\frac{1}{100}$  of its bulk of fixed air, and pouring into each of them equal quantities of diluted lime-water; one of these portions of lime-water being previously diluted with an equal quantity of distilled water, and the other with the same quantity of a diluted solution of nitrous selenite, containing about  $\frac{1}{400}$  of its weight of calcareous earth; when I could not perceive that the latter portion of lime-water was rendered at all less cloudy than the former. Though the nitrous selenite however does not make the effect of the fixed air less sensible, yet the dilution of the lime-water, in consequence of some of the lime being absorbed by the acid, does; but I believe not in any remarkable degree.

There is an experiment, mentioned by Mr. Kirwan, which, though it cannot be considered as an argument in favour of the generation of fixed air, as he only supposes, without any proof, that fixed air is produced in it, does yet deserve to be taken notice of as a curious experiment. It is, that, if nitrous and common



air be mixed over dry quicksilver, the common air is not at all diminished, that is, the bulk of the mixture will be not less than that of the common air employed, till water is admitted, and the mixture agitated for a few minutes. The reason of this in all probability is, that part of the phlogisticated nitrous acid, into which the nitrous air is converted, remains in the state of vapour till condensed by the addition of water. A proof that this is the real case is, that, in this manner of performing the experiment, the red fumes produced on mixing the airs remain visible for some hours, but immediately disappear on the addition of water and agitation.

The most material experiment alleged by Mr. Kirwan is one of Dr. Priestley's, in which he obtained fixed air from a mixture of red precipitate and iron filings. This at first seems really a strong argument in favour of the generation of fixed air; for though plumbago, which is known to consist chiefly of that substance, has lately been found to be contained in iron, yet one would not have expected it to be decomposed by the red precipitate, especially when the quantity of pure iron in the filings was much more than sufficient to supply the precipitate with phlogiston. The following experiment however shows that it was really decomposed; and that the fixed air obtained was not generated, but only separated by means of this decomposition.

500 gr. of red precipitate mixed with 1000 of iron filings yielded, by the assistance of heat, 7800 gr. measures of fixed air, besides 2400 of a mixture of dephlogisticated and inflammable air, but chiefly the latter. The same quantity of iron filings, taken from the same parcel, was then dissolved in diluted oil of vitriol, so as to leave only the plumbago and other impurities. These mixed with 500 gr. of the same red precipitate, and treated as before, yielded 9200 gr. measures of fixed air, and 4200 of dephlogisticated air, of an indifferent quality, but without any sensible mixture of inflammable air. It appears therefore, that less fixed air was produced when the red precipitate was mixed with the iron filings in substance, than when mixed only with the plumbago and other impurities; which shows that its production was not owing to the iron itself, which seems to contain no fixed air, but to the plumbago, which contains a great deal. The reason, in all probability, why less fixed air was produced in the first case than the latter is, that in the former more of the plumbago escaped being decomposed by the red precipitate than in the other. It must be observed however, that the filings used in this experiment were mixed with about  $\frac{1}{13}$  of their weight of brass, which was not discovered till they were dissolved in the acid, and which makes the experiment less decisive than it would otherwise be. The quantity of fixed air obtained is also much greater than, according to Mr. Bergman's experiment, could be yielded by the plumbago usually contained in 1000 gr. of iron; so that though the experiment seems to show that the fixed air was



only produced by the decomposition of the impurities in the filings, yet it certainly ought to be repeated in a more accurate manner.

Before concluding this paper, it may be proper to sum up the state of the argument on this subject. There are 5 methods of phlogistication considered by me in my paper on air, namely, 1st, the calcination of metals, either by themselves or when amalgamated with quicksilver; 2dly, the burning sulphur or phosphorus; 3dly, the mixture of nitrous air; 4thly, the explosion of inflammable air; and, 5thly, the electric spark; and Mr. Kirwan has not pointed out any other which he considers as unexceptionable. Now the last of these I by no means consider as unexceptionable, as it seems much more likely, that the phlogistication of the air in that experiment is owing to the burning or calcination of some substance contained in the apparatus.\* It is true, that I have no proof of it; but there is so much probability in the opinion, that till it is proved to be erroneous, no conclusion can be drawn from such experiments in favour of the generation of fixed air. As to the first method, or the calcination of metals, there is not the least proof that any fixed air is generated, though we certainly have no direct proof of the contrary; nor did I in my paper insinuate that we had. The same thing may be said of the burning of sulphur and phosphorus. As to the mixture of nitrous air, and the combustion of inflammable air, it is proved, that if any fixed air is generated, it is so small as to elude the nicest test we have. It is certain too, that if it had been so much as  $\frac{1}{70}$  of the bulk of the common air employed, it would have been perceived in the first of these methods, and would have been sensible in the second, though still less. So that out of the 5 methods enumerated, it has been shown, that in 2 no sensible quantity is generated, and not the least proof has been assigned that any is in 2 of the others; and as to the last, good reasons have been assigned for thinking it inconclusive; and therefore the conclusion drawn by me in the above-mentioned paper seems sufficiently justified; namely, that though it is not impossible that fixed air may be generated in some chemical processes, yet it seems certain, that it is not the general effect of phlogisticating air, and that the diminution of common air by phlogistication is by no means owing to the generation or separation of fixed air from it.

*XVI. Mr. Kirwan's Reply to Mr. Cavendish's Answer. p. 178.*

Mr. Cavendish says, that in Mr. Lassone's experiments the effervescence proceeded not from any fixed air in the alkali, but from the further action of the acid on the zinc from which inflammable air was disengaged. But this could

\* In the experiment with the litmus I attribute the fixed air to the burning of the litmus, not decomposition, as Mr. Kirwan represents it, which is a sufficient reason why no fixed air should be found when the experiment is tried with air in which bodies will not burn.—Orig.



not have happened; for, 1st, the zinc, instead of being further acted on by the acid, was precipitated according to Mr. Lassone's own account, p. 8; and, 2dly, the acid was only added by degrees, and undoubtedly would unite to the alkali preferably to the zinc; therefore it was from the alkali, and not from the zinc, that the effervescence arose.

2dly. With regard to the calcination of lead; though in England the smoke and flame may come in contact with the metal, yet in Germany red lead is formed without any communication between them, according to Mr. Nose, who has given an ample account of this manufactory, p. 86. Is not lime formed in contact with fuel, flame, and smoke? Mr. Macquer even thinks it probable, that the contact of flame is hurtful to the production of minium, 2 Dict. Chy. 639. Mr. Monnet made minium by melting lead in a cuppel, in such a manner that it was impossible it could come in contact with the least particle of flame or smoke, Mem. Turin. 1769, p. 71.

Mr. Cavendish expresses his surprize at my asserting, that the black powder, which Dr. Priestley formed out of an amalgam of mercury and lead, was exactly the same as that out of which he had extracted fixed air; but I think I have assigned very sufficient reasons for my opinion: how far I was right will best appear by Dr. Priestley's own letter, in the hands of the secretary, of which the following is an extract. "I certainly imagined the two black powders you write about to be of the same nature, and therefore did not attempt to extract any air from the latter; but immediately on the receipt of your favour of yesterday, I dissolved an ounce of lead in mercury, and expelling it by agitation, put the black powder, which weighed near 12 oz., into a coated glass retort; then applying heat, I got from it about 20 oz. measures of very pure fixed air, not  $\frac{1}{30}$  of which remained unabsorbed by water."

4thly. It is impossible to attribute the fixed air, produced by the distillation of red precipitate and filings of iron, to the decomposition of the plumbago contained in the iron; for the quantity of fixed air produced in Mr. Cavendish's own experiment is more than twice the weight of the whole quantity of plumbago contained in the quantity of iron he used, supposing the whole of the plumbago to consist of fixed air, which is not pretended; and more than 8 times the weight of the quantity of fixed air, which plumbago really contains. For Mr. Cavendish employed in his experiment 1000 gr. of iron and 500 gr. of red precipitate, and obtained 7800 gr. measures of fixed air, which are equal to 30 cubic inches, and weigh 17 gr. Now 100 gr. of bar iron contain, according to Mr. Bergman, at most,  $\frac{3}{10}$  of a grain of plumbago: and consequently 1000 gr. of this iron contain but 2 gr. of plumbago; and plumbago, according to Mr. Scheele, contains but  $\frac{1}{3}$  of its weight of fixed air; so that here, supposing the plumbago to be decomposed, we can have at most but  $\frac{7}{10}$  of a grain of fixed air, or little more



than 1 cubic inch. If we suppose the filings to be from steel, 1000 grs. of steel containing 8 of plumbago, we may have about 2.5 of fixed air, or about 1.5 cubic inch, and this is the strongest supposition, and the most favourable to Mr. Cavendish. What shall we then say, if we consider that these filings were mixed with copper or brass, which contain no plumbago? and, above all, that plumbago cannot be supposed decomposable by red precipitate, since even the nitrous acid cannot decompose it?

5thly. With regard to the power which nitrous selenite has of absorbing fixed air, I must allow the experiments of Mr. Cavendish to be just and agreeable to my own; but it only follows, that when fixed air is in its nascent state, it is more absorbable. Thus many metallic calces take it from alkalis in its nascent state, though in other circumstances they will take none. Lastly, the permanence of a mixture of nitrous and common air, made over mercury, cannot be attributed to nitrous vapour, as vapour is not elastic in cold; besides, I have often made the mixture without producing any such durable vapour, and this will always happen, when the nitrous air is made from nitrous acid sufficiently diluted.

*XVII. On a Method of Describing the Relative Positions and Magnitudes of the Fixed Stars; with some Astronomical Observations. By the Rev. F. Wollaston, LL.B. F. R. S. p. 181.*

From some alterations which have of late years been discovered, in the relative positions and apparent magnitudes of a few of the stars we call fixed, it seems not unreasonable to conclude, that there may be many changes among others that we little suspect. This thought has led me into a wish, that some method were adopted to detect such motions. The first idea which occurred to me was, to make a proposal to astronomers in general; that each should undertake a strict examination of a certain district in the heavens; and, not only by a re-examination of the catalogues hitherto published, but by taking the right ascension and declination of every star in their several allotment, to frame an exact map of it, with a corresponding catalogue; and to communicate their observations to one common centre. This is what I could be glad to see begun. Every astronomer must wish it, and therefore every one should be ready to take his share in it. Such a plan, undertaken with spirit, and carried on gradually with care, would, by the joint labours and emulation of so many astronomers as are now in Europe, produce a celestial Atlas far beyond any thing that has ever yet appeared.

But this would be a work of time, and not within the compass of every one. What I mean now to propose is more immediate; and not out of the reach of any who amuse themselves with viewing the heavenly bodies. Meridian altitudes and transits can be taken but once in 24 hours; and, though accurate, are therefore tedious. Neither can any re-examination of them be made, but with the same



labour as at the first. Equatorial sectors are in the hands of few; and require great skill. Some more general method seemed wanting; to discover variations which, when detected or only surmised, should be consigned immediately to a more strict investigation.

Turning this in my thoughts, I considered, that the noting down at the time the exact appearance of what one sees, would be far more simple, and show any alterations in that appearance more readily, than any other method. A drawing once made would remain, and could be consulted at any future period; and if it were drawn at first with care, a transient review would discover to one, whether any sensible change had taken place since it was last examined. Catalogues, or verbal descriptions of any kind, could not answer that end so well. To do this with ease and expedition was then the requisite: and a telescope with a large field, and some proper sub-divisions in it, to direct the eye and assist the judgment, seemed to bid most fair for success. The following is the method which, after various trials, I have adopted, and think I may now venture to recommend.

To a night-glass, but of Dollond's improved construction, which magnifies about 6 times, and takes in a field of just about as many degrees of a great circle, I have added cross wires, intersecting each other at an angle of  $45^{\circ}$ . More wires may be crossed in other directions; but I apprehend these will be found sufficient. This telescope I mount on a polar axis. One coarsely made, and without any divisions on its circle of declination, will answer this purpose, since there is no great occasion for accuracy in that respect: but as the heavenly bodies are more readily followed by an equatorial motion of the telescope, so their relative positions are much more easily discerned when they are looked at constantly as in the same direction. An horizontal motion, except in the meridian, would be apt to mislead the judgment. It is scarcely necessary to add, that the wires must stand so as for one to describe a parallel of the equator nearly. Another will then be a horary circle; and the whole area will be divided into 8 equal sectors.

Thus prepared, the telescope is to be pointed to a known star; which is to be brought into the centre or common intersection of all the wires. The relative positions of such other stars as appear within the field, are to be judged-of by the eye: whether at  $\frac{1}{2}$ , or  $\frac{1}{3}$ , or  $\frac{1}{4}$  from the centre towards the circumference, or vice versâ; and so with regard to the nearest wire respectively. These, as one sees them, are to be noted down with a black lead pencil on a large message card held in the hand, on which a circle, similarly divided, is ready drawn. (One of 3 inches diameter seems most convenient.) The motion of the heavenly bodies in such a telescope is so slow, and the noting down of the stars so quickly done, that there is most commonly full time for it without moving the telescope. When that is wanted, the principal star is easily brought back again into the centre of the field at pleasure, and the work resumed. After a little practice, it is astonish-



ing how near one can come to the truth in this way: and though neither the right ascensions nor the declinations are laid down by it, nor the distances between the stars measured; yet their apparent situations being preserved in black and white, with the day and year, and hour if thought necessary, written underneath, each card becomes a register of the then appearance of that small portion of the heavens; which is easily re-examined at any time with little more than a transient view; and which yet will show on the first glance, if there should have happened in it any variation of consequence. It is obvious however that very delicate observations are not to be made in this way.

My design was, after marking down all such stars as are visible with so small a magnifier, to go over the whole again with another telescope of a higher power, divided in the same way; and after that, with a 3d and 4th; so as to comprehend every star I could discern. That would discover smaller changes: but it must be a work of time, if attempted at all. After such a rough map of the constellation is made, the endeavouring to ascertain the right ascensions and declinations of these, may perhaps be adviseable in the next place, rather than searching for more.

In observing in this way, it is manifest that the places of such stars as happen to be under or very near any one of the wires, must be more to be depended on, than of what are in the intermediate spaces, especially if towards the edges of the field: so also what are nearest to the centre, because better defined, and more within the reach of one wire or another. For this reason, different stars in the same set must successively be made central, or brought towards one of the wires, where any suspicion arises of a mistake, in order to approach nearer to a certainty: but if the stand of the telescope be tolerably well adjusted and fixed in its place, that is soon done. This then is the method I would recommend to the practical astronomer, for becoming acquainted with the appearance of the stars, and setting a watch over the heavenly motions. After a very few trials, every one would find this easy. And if each person of every rank among astronomers would take a constellation or two under his care, the numbers who could undertake it in this way would compensate for the defects of a plan which cannot aspire at great accuracy. The labour of it, even at first, is but little.

Before concluding this head, I shall add a few hints. Whether this method be followed, or any other, if a general plan be set on foot, whoever undertakes a constellation, or district, should determine to examine it with as great accuracy as he can; yet never be ashamed to let others know of his mistakes. The error of one proves a caution to another. Such a rough sketch, once made, will be found of great use in knowing which star next to examine with greater care. He who can do no more than this, will do a useful work by going thus far: and his frequently sweeping over his district in this way, may lead him to a discovery



which might escape a more regular astronomer. But whoever can, ought to do more. By degrees the exact positions of every star he has noted down may be ascertained, by the method practised by Mr. de la Caille in his Southern Hemisphere, or by any other which shall be esteemed more convenient. To render this more complete, it were to be wished that each should give in a copy of his original observations, with an account of the instruments he used; since they ought to be preserved as data whence his deductions were made, which may then be re-examined at any future time. Yet must it be desired that no one would trust himself without carrying on his calculations as fast as the observations are made: they will otherwise multiply on his hands till the labour will dishearten him from attempting it at all. A heap of crude, undigested observations would be an unwelcome present to the public.

Since my former papers, the longitude of this place (Chislehurst) has been ascertained by comparative observations on the bursting of some rockets, let off on purpose; which, on a mean of several, turns out to be  $19^{\circ}.02$  in time E. of Greenwich Observatory; that is, it may hereafter be considered as  $19^{\circ}$ , instead of  $19^{\circ}.6$  as I had before calculated it trigonometrically from the bearings.

Mr. W. then adds a collection of new astronomical observations, made since his last communication; the first of which is an eclipse of the moon,  $\text{♂}$  July 30, 1776: observed with a  $3\frac{1}{4}$  feet achromatic telescope, and a power magnifying 29 times (that is, a single eye-glass belonging to the day-tube) the aperture of the telescope being reduced to  $1\frac{1}{4}$  inches. The night very clear and still. The beginning was not properly observed: the several spots on the moon's disc were there noted when eclipsed. At  $11^{\text{h}} 7^{\text{m}} 57^{\text{s}}$ , apparent time, the eclipse was seemingly total. The beginning of the emersion judged to be about  $12^{\text{h}} 43^{\text{m}} 0^{\text{s}}$ . The end of the eclipse was at  $13^{\text{h}} 42^{\text{m}} 0^{\text{s}}$ .

2. The next is an observation of an eclipse of the sun,  $\text{♀}$  June 24, 1778: observed with a  $3\frac{1}{4}$  feet achromatic telescope magnifying 75 times. The aperture reduced to 2 inches, to prevent breaking the smoked glasses. The beginning at  $3^{\text{h}} 40^{\text{m}} 33\frac{1}{4}^{\text{s}}$  ap. time; and the end at  $5^{\text{h}} 25^{\text{m}} 24^{\text{s}}$ .

3. The 3d is an eclipse of the moon  $\text{♂}$  November 23, 1779: observed with the same telescope, magnifying 75 times. The aperture reduced to 2 inches. Night clear and frosty. No wind. The eclipse total at  $7^{\text{h}} 7^{\text{m}} 31^{\text{s}}$ ; the moon's edge began to emerge at  $8^{\text{h}} 46^{\text{m}} 23^{\text{s}}$ . The end could not be seen for the haze.

4. The 4th is an eclipse of the sun,  $\text{♂}$  Oct. 16, 1781: observed with the same telescope and magnifying power. The beginning not visible; sun too low. The end at  $20^{\text{h}} 22^{\text{m}} 13^{\text{s}}.5$ .

5. The 5th was an eclipse of the moon,  $\text{♀}$  Sept. 10, 1783: observed with the same telescope, viz.  $3\frac{1}{4}$  feet achromatic, with the aperture reduced to 2 inches; but with a small magnifying power of 36 times, made by Mr. Dollond. The



beginning of the shadow at  $9^h 45^m 35^s$ , rather doubtful. Total darkness judged at  $10^h 46^m 34^s$ . The moon seems beginning to emerge at  $12^h 23^m$ ; the emersion certainly begun at  $12^h 25^m$ . At  $13^h 25^m 38^s$  the shadow quitted the moon, between Langrenus and M. Crisium.

6. The next was a transit of mercury over the sun's disc,  $\text{♂}$  Nov. 12, 1782: observed with the same telescope, and a power of 75 times. The aperture reduced to 2 inches. At  $2^h 51^m 49^s$ , ap. t. the first impression observed. It could not be  $2^s$  sooner. At  $2^h 54^m 57^s$  the thread of light completed; but seen through clouds. The planet seemed to hang on the sun's limb  $30^s$  at least. At  $4^h 6^m 0^s$  through a break in the clouds, of short duration,  $\text{♀}$  seemed to have quitted the sun; but indeed the clouds were very unfavourable the whole time.

7. Occultation of Saturn by the moon,  $\text{♄}$  Feb. 18, 1775: observed with the same telescope. At  $9^h 6^m 9^s$  the preceding limb of the planet immersed.

Several occultations of fixed stars, and eclipses of Jupiter's satellites were also observed.

*XVIII. An Account of some late fiery Meteors; with Observations. By Chas. Blagden, M. D. Sec. R. S. p. 201.*

This account respects chiefly the two most remarkable of the meteors that had lately appeared, and is founded partly on private communications, and partly on such accounts as were published in the newspapers. These meteors were of the kind known to the ancients by the names of  $\text{Λαμπαδες}$ ,  $\text{Πιθοι}$ , Bolides, Faces, Globi, &c. from particular differences in their shape and appearance, and sometimes, it seems, under the general term of comets; in the Philos. Trans. they are called indiscriminately fire-balls or fiery meteors; and names of a similar import have been applied to them in the different languages of Europe. The most material circumstances observed of such meteors may be brought under the following heads. 1. Their general appearance. 2. Their path. 3. Their shape or figure. 4. Their light and colours. 5. Their height. 6. Their noise. 7. Their size. 8. Their duration. 9. Their velocity.

Dr. B. begins with the first of these meteors, which was seen Aug. 18, 1783.

§ 1. Its general appearance in these parts of Great Britain was that of a luminous ball, which rose in the N.N.W. nearly round, became elliptical and gradually assumed a tail as it ascended, and in a certain part of its course seemed to undergo a remarkable change compared to bursting; after which it proceeded no longer as an entire mass, but was apparently divided into a great number or a cluster of balls, some larger than the others, and all carrying a tail or leaving a train behind; under this form it continued its course with a nearly equable motion, dropping or casting off sparks, and yielding a prodigious light, which illuminated all objects to a surprising degree; till having passed the east, and verging considerably to



the southward, it gradually descended, and at length was lost out of sight. The time of its appearance was 9<sup>h</sup> 16<sup>m</sup> P. M. mean time of the meridian of London, and it continued visible about half a minute.

§ 2. How far north the meteor may have begun there are no materials to determine with precision; but, as it was seen in Shetland, and at sea between the Lewes and Fort William, and appeared to persons at Aberdeen and Blair in Athol ascending from the northward, and to an observer in Edinburgh as rising like the planet Mars, there can be little doubt but its course commenced beyond the farthest extremity of this island, somewhere over the northern ocean. General Murray, F. R. S. being then at Athol House, saw it pass over his head as nearly vertical as he could judge, tracing it from about 45° of elevation north-north-westward to 30° or 20° south-south-eastward, where a range of buildings intercepted it from his view. From near the zenith of Athol House, it passed on a little westward of Perth, and probably a little eastward of Edinburgh; and continuing its progress over the south of Scotland, and the western parts of Northumberland and the Bishopric of Durham, proceeded almost through the middle of Yorkshire, leaving the capital of that county somewhat to the eastward. Hitherto its path was as nearly S. S. E. as can be ascertained; but somewhere near the borders of Yorkshire, or in Lincolnshire, it appears to have gradually deviated to the eastward, and in the course of that deviation to have suffered the remarkable change already noticed under the denomination of bursting. After this division, the compact cluster of smaller meteors seems to have moved for some time almost S. E. thus traversing Cambridgeshire and perhaps the western confines of Suffolk; but gradually recovering its original direction, it proceeded over Essex and the Straits of Dover, entering the continent probably not far from Dunkirk, where, as well as at Calais and Ostend, it was thought to be vertical. Afterwards it was seen at Brussels, Paris, and Nuits in Burgundy, still holding on its course to the southward; nay, there is an intimation, though of doubtful authority, that it was perceived at Rome. Our information of its progress over the continent is indeed very defective and obscure; yet we have sufficient proof that it traversed in all 13 or 14 degrees of latitude, describing a track of 1000 miles at least over the surface of the earth; a length of course far exceeding the utmost that has been hitherto ascertained of any similar phenomenon.

§ 3. This meteor was described by most spectators under 3 different forms, and is so represented by Mr. Sandby in his beautiful drawing; but the first 2 of those do not imply any real variation in its shape, depending only on a difference in the point of view. Accordingly, in the first part of its course over Scotland, it was seen to have a tail, and is thus described by General Murray when it passed Athol House. Two causes concur in this deception; first, the fore-shortening, and even occultation, of the tail, when the object is seen nearly in front; and, 2dly,



that the light of most part of the tail is of so inferior a kind, as to be difficultly perceived at a great distance, especially when the eye is dazzled by the overpowering brilliancy of the body. The length and shape of the tail however were perpetually varying; nor did the body continue always of the same magnitude and figure, but was sometimes round, at other times elliptical, with a blunt or pointed protuberance behind. From such changes of figure in this and other meteors it is, that they have been compared to columns or pyramids of fire, comets, barrels, bottles, flasks, paper-kites, trumpets, tadpoles, glass-drops, quoits, torches, javelins, goats, and many other objects; whence the multifarious appellations given to them by the ancients were borrowed.

Respecting the tails of meteors, it is here necessary to distinguish between 2 different parts of which they consist. The brightest portion seems to be of the same nature as the body, and indeed an elongation of the matter composing it; but the other, and that commonly the largest portion, might more properly be called the train, appearing to be a matter left behind after the meteor has passed; it is far less luminous than the former part, and often only of a dull or dusky red colour. A similar train or streak is not unfrequently left by one of the common falling stars, especially of the brighter sort; and vestiges of it sometimes remain for several minutes. It often happens, that even the large fire-balls have no other tail but this train, and this of the 18th of August appeared at times to be in that state; its tail was thought by some spectators to be spiral.

Under this changeable form, but still as a single body, it proceeded regularly till a certain period, when expanding with a great increase of light, it separated into a cluster of smaller bodies or ovals, each extended into a tail and producing a train. At the same time a great number of sparks appeared to issue from it in various directions, but mostly downward, some of which were so bright as also to leave a small train. Most fire-balls have suffered a bursting or explosion of this kind; but in general they have been thought to disappear immediately afterwards. This however continued its course, becoming more compact, or perhaps reuniting, and seems to have undergone other similar explosions before it left our island, and again on the continent. The different accounts tend to show, that its first separation or bursting happened somewhere over Lincolnshire, perhaps near the commencement of the fens. It is observable, that the great change in this meteor corresponds with the period in which it suffered a deviation from its course, as if there was some connexion between those two circumstances; and there are traces of something of the same kind having happened to other meteors. If the explosion be any sort of effort, we cannot wonder that the body should be moved by it from a straight line; but on the other hand it seems equally probable, that if the meteor be forced, by any cause, to change its direction, the consequence should be a division or separation of its parts.



§ 4. Nothing relative to these meteors strikes the beholders with so much astonishment as the excessive light they afford, sufficient to render very minute objects visible on the ground in the darkest night, and larger ones to the distance of many miles from the eye. The illumination is often so great as totally to obliterate the stars, to make the moon look dull, and even to affect the spectators like the sun itself; nay, there are many instances in which such meteors have made a splendid appearance in full sun-shine. The colour of their light is various and changeable, but generally of a bluish cast, which makes it appear remarkably white. A curious effect of this was observed at Brussels the 18th of August, that while the meteor was passing, "the moon appeared quite red, but soon recovered its natural light." The brightness alone of the meteor is not sufficient to explain this, for the moon does not appear red when seen by day; but it must have depended on the contrast of colour, and shows how large a proportion of blue rays enters into the composition of that light, which could make even the silver moon appear to have excess of red. Prismatic colours were also observed in the body, tail, and sparks of this meteor, variously by different persons; some compared them to the hues of gems. The moment of its greatest brightness seems to have been when it burst the first time; but it continued long to be more luminous after that period, than it was before.

The body of the fire-ball, even before it burst, did not appear of a uniform substance or brightness, but consisted of lucid and dull parts, which were perpetually changing their respective positions; so that the whole effect was to some eyes like an internal agitation or boiling of the matter, and to others like moving chasms or apertures. Similar expressions have been used in the description of former meteors. The luminous substance was compared to burning brimstone or spirits, Chinese fire, the stars of a rocket, a pellucid ball or bubble of fire, liquid pearl, lightning and electrical fire; few persons fancied it to be solid, especially when it came near the zenith. Different spectators observed the light of the meteor to suffer at times a sudden diminution and revival, which produced an appearance as of successive inflammation; but might, in some cases at least, be owing to the interposition of small clouds in its path.

§ 5. When, in consequence of a more accurate attention to natural philosophy, such observations were first made on fire-balls as determined their height, the computers were with reason surprized to find them moving in a region so far above that of the clouds and other familiar meteors of our atmosphere; especially as to every uninformed spectator they appear extremely near, or as if bursting over his head, a natural effect of their great light when seen without intervening objects. Their real height is to be collected from observations made at distant stations, which, for the greatest accuracy, ought to be so situated, that the line



joining them may cut the path of the meteor at right-angles, and that, at its greatest elevation, it may appear from both of them about  $45^\circ$  above the horizon, on opposite sides of the zenith. Dr. B. laments that most of the observations in his possession of the meteor of August 18, give its altitude by estimation only; yet their correspondence with each other may gain them a degree of credit, to which, if single, they would not be entitled. Dr. B. here relates 8 or 9 accounts of the height and motion of this meteor, from which he calculates its perpendicular height above the earth's surface in most of these instances, and finds they run from 57 to 60 miles in height. And further observes, this agreement of the different altitudes is nearer than could be expected; and therefore we may safely conclude, that it must have been more than 50 miles above the surface of the earth, in a region where the air is at least 30000 times rarer than here below.

§ 6. That a report was heard some time after the meteor of the 18th of August had disappeared, is a fact which rests on the testimony of too many witnesses to be controverted, and is conformable to what has been observed in most other instances. In general it was compared to the falling of some heavy body in a room above stairs, or to the discharge of one or more large cannon at a distance. That rattling noise, like a volley of small arms, which has been remarked after other meteors, does not seem to have been heard on this occasion. From a comparison of the different accounts, it appears as if the report was loudest in Lincolnshire and the adjacent countries, and again in the eastern parts of Kent; in the intermediate places it was so indistinct as generally not to have been noticed, and all observers of credit in Scotland deny that they heard any thing of the sort. If this report then be connected with the bursting of the meteor, it would seem as if that sound was produced two separate times, namely at the first explosion over Lincolnshire, and again when it seemed to burst soon after entering the continent. Ingenious men have availed themselves of this sound, to calculate the distance and height of meteors; and the exactness attained by this method, in the computation of the late fire-ball from the report heard at Windsor, is very remarkable; but in general the accounts disagreed so much, that it would have been impossible to conclude any thing from them. Besides the report as of explosions which was heard after the meteor, another sort of sound was said to attend it, more doubtful in its nature, and less established by evidence, viz. a kind of hissing, whizzing, or crackling, as it passed along. That sound should be conveyed to us in an instant from a body above 50 miles distant, appears so irreconcilable to all we know of philosophy, that perhaps we should be justified in imputing the whole to an affrighted imagination, or an illusion produced by the fancied analogy of fireworks. The testimony in support of it is however so considerable, on the occasion of this as well as former meteors, that Dr. B. cannot



venture to reject it, however improbable it may be thought, but leaves it as a point to be cleared up by future observers.

§ 7. To determine the bulk of the fire-ball, we must not only have calculated its distance, but also know the angle under which it appeared. For this purpose the moon is the usual term of comparison; but as it was thought, at very different distances, to present a disc equal to that luminary's, and the same expressions have been applied to most preceding fire-balls, Dr. B. conceives this estimation rather to be a general effect of the strong impression produced by such splendid objects on the mind, than to convey any determinate idea of their size. However, if we suppose its transverse diameter to have subtended an angle of  $30'$  when it passed over the zenith, which probably is not very wide of the truth, and that it was 50 miles high, it must have been almost half a mile across. The tail sometimes appeared 10 or 12 times longer than the body; but most of this was train, and the real elongation behind seems seldom to have exceeded twice or thrice its transverse diameter, consequently was between 1 and 2 miles long. Now if the cubical contents be considered, for it appeared equally round and full in all directions, such an enormous mass, moving with extreme velocity, affords just matter of astonishment.

§ 8. The duration of the meteor is very differently stated, partly because some observers had it in view a much longer time than others, and partly because they formed different judgments of the time. Those who saw least of it seem to have perceived its illumination about 10 seconds, and those who saw most of it about a minute: hence the various accounts may in some measure be reconciled. Mr. Herschel, F. R. S., at Windsor, must have kept it in sight long after other observers had thought it extinct: for though probably he did not see the beginning, as it never appeared to him like a single ball, he watched it as much as "40 or 45 seconds, the last 20 or 25 of which it remained almost in one situation, within a few degrees of the horizon." This confirms the foreign accounts of its long progress to the southward.

§ 9. From the apparent motion of the meteor compared with its height, some computation may be formed of its astonishing velocity. As at the height of 50 miles above the surface of the earth, it might be visible from the same station for a tract of more than 1200 miles, and the longest continuance of its illumination scarcely exceeded a minute, we have hence some presumption that it moved not less than 20 miles in a second. The Rev. Mr. Watson, in his letter to Lord Mulgrave, says, that the arc described by it while in his view could not be less than  $70^\circ$  or  $80^\circ$ , and yet the time could not exceed  $4^s$ , or  $5^s$  at most: this, with an altitude of  $60^\circ$ , and height of 50 miles, gives for its velocity about 21 miles in a second. The observer at Newton Ardes estimated its motion to be 30 miles in a second. Mr. Herschel found it describe an arch of  $167^\circ$  during the 40 or



45 seconds he observed it, which gives a velocity of more than 20 miles in a second. Finally, Mr. Aubert, F. R. S. thought it described an arch of  $136^{\circ}$  of azimuth in 10 or 12 seconds, which would make its velocity above 40 miles in a second. Dr. B. is sensible of the objections that may be made to all these computations; undoubtedly they are too vague; and yet, all taken together, perhaps they may have some weight, especially as they correspond so well with the different phenomena of the meteor's duration, and other fire-balls have been computed to move as fast. Stating the velocity at the lowest computation of 20 miles a second, it exceeds that of sound above 90 times, and begins to approach toward that of the earth in her annual orbit. At such a rate, it must have passed over the whole island of Great-Britain in less than half a minute, and might have reached Rome within a minute afterwards, or in 7 minutes have traversed the whole diameter of the earth! From this calculation it will be evident, that there is little chance of determining the velocity of meteors from the times of their passing the zenith of different places; and that therefore we must principally depend on observing carefully, with a watch that shows seconds, their apparent velocity through the heavens.

The fire-ball which appeared Oct. 4, at 43<sup>m</sup> past 6 in the evening, was much smaller than that already described, and of much shorter duration. It was first perceived to the northward as a stream of fire, like the common shooting stars, but large; and having proceeded some way under this form, it suddenly burst out into that intensely bright bluish light which is peculiar to such meteors. At this period Dr. B. saw it, and could compare the colour to nothing so well as to the blue lights of India, and some of the largest electrical sparks. The illumination was very great; and on that part of its course where it had been so bright, a dusky red streak or train was left, which remained visible perhaps a minute even with a candle in the room, and was thought by some gradually to change its form. Except this train, he thought the meteor had no tail, but was nearly a round body, or perhaps a little elliptical. After moving not less than  $10^{\circ}$  in this bright state, it became suddenly extinct, without any appearance of bursting or explosion. This meteor was seen for so short a way, that it was scarcely possible to determine the direction of its course with accuracy; but as in proceeding to the eastward it very perceptibly inclined towards the horizon, it certainly moved somewhere from the north-westward to the south-eastward. Its duration was so short, that many persons thought it passed in an opposite direction; for his own part, he found himself absolutely unable to determine whether the motion was from or toward the S. E. Some spectators were of opinion, that it changed its course the moment it became bright, proceeding no longer in the same straight line; but his information was not sufficient to determine this question. From his own and another observation Dr. B. calculates that the height of the meteor



above the surface of the earth, after all proper allowances are made, must have been between 40 and 50 miles.

As there was no appearance of bursting at the extinction of this fire-ball, so no report was heard after it ; nor did any sound attend it. Some observers thought this meteor also near as large as the moon, but to Dr. B. it did not appear above one quarter of her diameter, which would make its breadth somewhat above a furlong. If the whole of the meteor's track be included, it seems to have lasted as much as 3 seconds, but in the bright state its duration was less than 2, he thinks not much above 1. Supposing it described an arc of  $14^{\circ}$  in  $1\frac{1}{4}$  second, or, according to Mr. Aubert's observation, of  $25^{\circ}$  in  $3^s$ , its real velocity was about 12 miles a second.

Such meteors as these, which pass like a flash of lightning, and describe so short a course, are very unfavourable for calculating the velocity, but afford great advantages for determining the height, as they must be seen nearly at the same moment and in the same place by the different observers. Other instances are found of fire-balls beginning with a dull red light like a falling star, particularly the great one of March 19, 1719, treated of so fully by Dr. Halley\* and Mr. Whiston.† It is remarkable, that a similar meteor had appeared the same day, that is, Saturday the 4th of October, about 3 in the morning, though, on account of the early hour, it was seen by fewer spectators. They represent it as rising from the northward to a small altitude, and then becoming stationary with a vibratory motion, and an illumination like day-light ; it vanished in a few moments, leaving a train behind. This sort of tremulous appearance has been noticed in other meteors, as well as their continuing stationary for some time, either before they began to shoot forward, or after their course was ended.

I find it impossible to quit this subject, says Dr. B., without some reflections about the cause, that can be capable of producing such appearances at an elevation above the earth, where, if the atmosphere cannot absolutely be said to have ceased, it is certainly to be considered as next to nothing. The first idea which suggested itself, that they were burning bodies projected with such a velocity, was quickly abandoned, from the want of any known power to raise them up to that great height, or, if there, to give them the required impetus ; and the ingenuity of Dr. Halley soon furnished him with another hypothesis, in which he thought both these difficulties obviated. He supposes there is no projection of a single body in the case ; but that a train of combustible vapours, accumulated in those lofty regions, is suddenly set on fire, whence all the phenomena are produced by the successive inflammation.‡ But Dr. Halley gives no just

\* Phil. Trans., vol. 3, N<sup>o</sup> 350, p. 978. † Account of a surprizing meteor seen March 19, 1719.—Orig. ‡ Phil. Trans., vol. 30, N<sup>o</sup> 360.



explanation of the nature of these vapours, nor of the manner in which they can be raised up through air so extremely rare; nor, supposing them so raised, does he account for their regular arrangement in a straight and equable line of such prodigious extent, or for their continuing to burn in such highly rarefied air. Indeed, it is very difficult to conceive, how vapours could be prevented, in those regions where there is in a manner no pressure, from spreading out on all sides in consequence of their natural elasticity, and instantly losing that degree of density which seems necessary for inflammation. Besides, it is to be expected, that such trains would sometimes take fire in the middle, and so present the phenomenon of 2 meteors at the same time, receding from each other in a direct line.

These difficulties have induced other philosophers to relinquish Dr. Halley's hypothesis, and propose, instead of it, one of a very opposite nature, that meteors are permanent solid bodies, not raised up from the earth, but revolving round it in very eccentric orbits; or, in other words, that they are terrestrial comets. The objections to this opinion however seem equally great. Most observers describe the meteors, not as looking like solid bodies, but rather like a fine luminous matter, perpetually changing its shape and appearance. Of this many defenders of the opinion are so sensible, that they suppose the revolving body gets a coat or atmosphere of electricity, by means of which it becomes luminous; but whoever carefully peruses the various accounts of fire-balls, and especially ours of the 18th of August when it divided, will perceive that their phenomena do not correspond with the idea of a solid nucleus enveloped in a subtile fluid, any more than with the conjecture of another learned gentleman, that they become luminous by means of a contained fluid, which occasionally explodes through the thick solid outer shell.

A strong objection to this hypothesis of permanent revolving bodies, is derived from the great number of them there must be to answer all the appearances. Such a regular gradation is observed, from those large meteors which strike all beholders with astonishment, and occur but rarely, down to the minute fires called shooting stars, which are seen without being regarded in great numbers every clear night, that it seems impossible to draw any line of distinction between them, or deny that they are all of the same name. But such a crowd of revolving bodies could scarcely fail to announce their existence by some other means than merely a luminous train in the night; as, for instance, by meeting or justling sometimes near the earth, or by falling to the earth in consequence of various accidents; at least we might expect they would be seen in the day-time, either with the naked eye, or by telescopes, by some of the numerous observers who are constantly examining the heavens.

Another argument of great weight against the hypothesis that fire-balls are



terrestrial comets, is taken from their great velocity. A body falling from infinite space toward the earth, would have acquired a velocity of no more than 7 miles a second, when it came within 50 miles of the earth's surface; whereas these meteors seem to move at least 3 times faster. And this objection, if there be no mistake in regard to the velocity of the meteor, as I think there is not, absolutely oversets the whole hypothesis.

What then can these meteors be? The only agent in nature with which we are acquainted, that seems capable of producing such phenomena, is electricity.\* I do not mean that by what is already known of that fluid, all the difficulties relative to meteors can be solved, as the laws, by which its motions on a large scale are regulated in those regions so nearly empty of air, can scarcely, I imagine, be investigated in our small experiments with exhausted vessels; but only that several of the facts point out a near connexion and analogy with electricity, and that none of them are irreconcilable to the discovered laws of that fluid.

1. Electricity moves with such a prodigious velocity, as to elude all the attempts hitherto made by philosophers to detect it; but the swiftness of meteors, stating it at 20 miles a second, is such as no experiments yet contrived could have discovered, and which seems to belong to electricity alone. This is, perhaps, the only case in which the course or direction of that fluid is rendered perceptible to our senses, in consequence of the large scale on which these fire-balls move.

2. Various electrical phenomena have been seen attending meteors. Lambent flames are described as settling on men, horses, and other objects; and sparks coming from them, or the whole meteor itself, it is said, have damaged ships, houses, &c. in the manner of lightning. These facts, I must own, are but obscurely related, yet still they do not seem to be destitute of foundation. If there be really any hissing noise heard while meteors are passing, it seems explicable on no other supposition than that of streams of electric matter issuing from them, and reaching the earth with a velocity equal to that of the meteor, namely, in 2 or 3 seconds. Accordingly, in one of our late meteors, the hissing was compared to that of electricity issuing from a conductor. The sparks flying

\* Since the above was written, other ways of accounting for these meteors have been discovered, and such indeed as, agreeing very well with all the phenomena, seem to be a probable solution, or at least a possible one; which is far more than can be said of the notion from electricity, which hardly agrees with any one of the numerous extraordinary circumstances attending these meteors, which have been observed in many instances to be the same as the stony masses that have often fallen through the atmosphere on many parts of the earth. For a particular account of such phenomena, see our note at p. 100, &c. of vol. 6, of these abridgments. It is remarkable of the present large meteor, that its calculated velocity is nearly equal to that of the earth in its annual motion in the opposite direction.—Orig.



off so perpetually from the body of fire-balls, may possibly have some connection with these streams. In the same manner the sound of explosions may perhaps be brought to us quicker, than if it were propagated through the whole distance by air alone. Should these ideas be well founded, the change of direction which meteors seem at times to undergo, may possibly be influenced by the state of the surface of the earth over which they are passing, and to which the streams are supposed to reach. A similar cause may occasion the apparent explosion, the opening of more channels giving new vent and motion to the electric fluid. May not the deviation and explosion which appear to have taken place in the fire-ball of the 18th of August over Lincolnshire, have been determined by its approach towards the fens, and an attraction produced by that large body of moisture?

3. A further argument for the electric origin of meteors is deduced from their connection with the northern lights, and the resemblance they bear to these electrical phenomena, as they are now almost universally allowed to be, in several particulars. Instances are recorded, where northern lights have been seen to join and form luminous balls, darting about with great velocity, and even leaving a train behind like the common fire-balls. This train I take to be nothing but the rare air left in such a highly electrified state as to be luminous; and some streams of the northern lights are very much like it. The aurora borealis appears to occupy as high, if not a higher, region above the surface of the earth, as may be judged from the very distant countries to which it has been visible at the same time; indeed the great accumulation of electric matter seems to lie beyond the verge of our atmosphere, as estimated by the cessation of twilight. Also with the northern lights a hissing noise is said to be heard in some very cold climates; Gmelin speaks of it in the most pointed terms, as frequent and very loud in the north eastern parts of Siberia; and other travellers have related similar facts.

But, in my opinion, the most remarkable analogy of all, and that which tends most to elucidate the origin of these meteors, is the direction of their course, which seems, in the very large ones at least, to be constantly from or toward the north or north-west quarter of the heavens, and indeed to approach very nearly to the present magnetical meridian. This is particularly observable in those meteors of late years whose tracks have been ascertained with most exactness; as that of November 26, 1758, described by Sir John Pringle; that of July 17, 1771, treated of by M. Le Roy; and this of the 18th of last August. The largest proportion of the other accounts of meteors confirm the same observation, even those of a more early period; nay I think some traces of it are perceivable in the writings of the ancients. Whether their motion shall be from the northern quarter of the heavens or toward it, seems nearly indifferent, as the numbers of those going each way are not very unequal; I consider them in



the former case as masses of the electric fluid repelled or bursting from the great collected body of it in the north; and in the latter case as masses attracted to that accumulation; a distinction probably much the same in effect, as that of positive and negative electricity near the surface of the earth.

This tendency toward the magnetic meridian however, seems to hold good only with regard to the largest sort of fire-balls; the smaller ones move more irregularly, perhaps because they become farther within the verge of our atmosphere, and are thus more exposed to the action of extraneous causes. That the smaller sort of meteors, such as shooting stars, are really lower down in the atmosphere, is rendered very probable by their swifter apparent motion; perhaps it is this very circumstance which occasions them to be smaller, the electric fluid being more divided in more resisting air. But as those masses of electricity, which move where there is scarcely any resistance, so generally affect the direction of the magnetic meridian, the ideas which have been entertained of some analogy between these two obscure powers of nature, seem not altogether without foundation.

If the foregoing conjectures be just, distinct regions are allotted to the electrical phenomena of our atmosphere. Here below we have thunder and lightning, from the unequal distribution of the electric fluid among the clouds; in the loftier regions, whither the clouds never reach, we have the various gradations of falling stars; till beyond the limits of our crepuscular atmosphere the fluid is put into motion in sufficient masses to hold a determined course, and exhibit the different appearances of what we call fire-balls; and probably at a still greater elevation above the earth, the electricity accumulates in a lighter less condensed form, to produce the wonderfully diversified streams and coruscations of the aurora borealis.

*XIX. On the Remarkable Appearances at the Polar Regions of the Planet Mars, the Inclination of its Axis, the Position of its Poles, and its Spheroidical Figure; with a few Hints relating to its Real Diameter and Atmosphere. By William Herschel, Esq., F. R. S. p. 233.*

What Dr. H. offers on the abovementioned subjects is founded on a series of observations delivered in this paper. When he found that the poles of Mars were distinguished with remarkable luminous spots, it occurred to him that we might obtain a good theory for settling the inclination and nodes of the planet's axis, by measures taken of the situation of those spots. But, not to proceed on grounds that wanted confirmation, it became necessary to determine by observation, how far these polar spots might be depended on as permanent; and in what latitude of the globe of Mars they were situated; for, if they should either be changeable, or not be at the very poles, we might be led into great mistakes



by overlooking these circumstances. The following observations will assist us in the investigation of the preliminary points. Dr. H. here details the numerous observations he made of the spots on the planet, and exhibits the several views of its disc in the numerous figures. Hence he concludes in general, that none of the bright spots on Mars were exactly at the poles, though they could certainly not be far from them. But, more particularly, from the appearance and disappearance of the bright north polar spot in 1781, he collects that the circle of its motion was at some considerable distance from the pole. By a calculation, made according to the principles hereafter explained, its latitude must have been about  $76^{\circ}$  or  $77^{\circ}$  north; for, to the inhabitants of Mars, the declination of the sun, June 25,  $12^{\text{h}} 15^{\text{m}}$  of our time, was about  $9^{\circ} 56'$  south; and the spot must have been at least so far removed from the north pole as to fall a few degrees within the enlightened part of the disc, to become visible to us. The south pole of Mars could not be many degrees from the centre of the large bright southern spot of the year 1781; though the spot was of such a magnitude as to cover all the polar regions farther than the 70th or 65th degree, and in that part which was on the meridian July 3, at  $10^{\text{h}} 54^{\text{m}}$ , perhaps a little farther.

The bright polar spots therefore on Mars being the most convenient objects for determining the situation of the axis of this planet; Dr. H. collects, in one view, all the measures he had taken of these spots for that purpose. He then applies the observations to determine the situation of the axis of Mars. To this end, we see that, in the first place, the measures must be corrected for the latitude of the spot; next, they must be reduced to a heliocentric observation, which will also correct them from the difference occasioned by the different situation of the planet when they were taken. This being done, we may select two observations at a proper distance; from which, by trigonometry, we shall have the node and inclination of the axis. When these elements are obtained, it will be easy to see how other observations agree with them; which will afford the means of correcting or verifying the former calculations.

Let  $\tau$ , fig. 8, pl. 7, be the earth;  $\odot a q \vee$  the ecliptic as seen from  $\tau$ ;  $p$  the point of the heavens towards which the north pole of the earth is directed;  $m$  the place of the orbit of Mars  $\mu m m$ , where an observation of the poles of that planet has been made, which is to be reduced to its heliocentric measure. And first suppose it to have been made at the time of the opposition of that planet. Then, the place  $m$  or  $a$  in the ecliptic being given, we have the sides  $a \odot$ ,  $\odot p$ ; whence the angle  $a$ , of the right angled triangle  $p \odot a$ , is found. This being added to, or taken from, the observed angle of position of the axis of Mars, according to circumstances easily to be determined, reduces it to its heliocentric position. But if this observation was not made at the time of an



opposition, but at some other place  $m$ , a second correction is to be applied in the following manner. Let the angle  $q$ , of the triangle  $p \odot q$ , be found as before, and properly applied to the position of the axis of Mars now at  $m$ ; then make the single  $ms\mu$ , at the sun  $s$ , equal to the angle  $smr$ , and  $\mu$  will be the heliocentric place, where the angle of position, when seen from  $s$ , will appear to be as it was found at  $m$ , after the application of the first correction: for  $s\mu$  being parallel to  $rm$ , and supposing the axis of Mars to preserve its parallelism while it moves from  $m$  to  $\mu$ , appearances of Mars at  $\mu$  to an eye at  $s$ , must be the same as they are at  $m$  to an eye at  $r$ . Dr. H. then sets down in a table the results of the calculations of the foregoing observations. And then adds, as we have no particular reason to select one measure rather than another, a mean of all the 13, (the number of them) will probably be nearest the truth; so that, by these observations, which are reduced to the 4th of Oct. 1783, we find the position of the axis of Mars that day to have been  $55^{\circ} 41'$  south following.

From the appearances of the south polar spot in 1781, he concludes, that its centre was nearly polar. It continued visible all the time Mars revolved on his axis; and, to present us generally with a pretty equal share of the luminous appearance, a spot which covered from  $45^{\circ}$  to  $60^{\circ}$  of a great circle on the globe of Mars could not have any considerable polar distance: however, a small correction in the angle of position seems to be necessary which should be taken from the measure of the 15th of July, because that branch of the spot which probably extended farthest towards the equator was then in the following quadrant. The measure of both the spots on June the 25th, 1781, is still more to be depended on, as giving very nearly the position of the true pole; for it appears evident from the phenomena of the bright north polar spot, that the spot was in the meridian when the measure was taken, while the southern spot was in the preceding quadrant near its greatest limit. Now, since an angle at the circumference of a circle is but half the angle at the centre, when the arches which subtend these angles are equal, the correction necessary to be applied to the measure taken through the two spots will be but one half of the correction which would have been requisite had it been through the centre; therefore, in order to reduce this to the condition of the former, we may suppose it to have been taken through the centre of Mars when the spot was only 30, or 150 degrees from the meridian. It is also necessary to add  $1^{\circ} 54'$  to the angle of July 15, which it would have measured more had the planets remained where they were June 25. This done, we may have the polar distance of the centre of the spot as before. Half the sum of the sines (of  $231^{\circ} 38'$  and  $150^{\circ}$ ) to radius, as  $50'$  (half the difference between  $74^{\circ} 32'$  and  $76^{\circ} 12'$ ) to a 4th number, which is  $1^{\circ} 18'$ .

Dr. H. observes here, that the measures of the angle of position would be too large before the spot came to the meridian, and too small afterwards, the axis



of Mars being south preceding; whereas they would be too small before, and too large after, the meridian passage, the pole being south following. These two observations arranged, and reduced to the time of the 25th of June, will stand as follows.

Time of observ.	Angles taken.	Angle $\alpha$ .	First correction.	Second Correc.	Corrected Angle.
June 25 <sup>d</sup> 11 <sup>h</sup> 36 <sup>m</sup> ....	74° 32'....	— 10° 14'....	+ } half of 1° 18'....	— 0° 0'....	75° 11'
July 15 10 12 ....	74 18 ....	— 8 20 ....	— 1 1 ....	+ 1 54....	75 11

He remarks, that having here admitted both measures as equally good, therefore the result is a mean of them both, and shows the axis of Mars, June 25, 1781, to have been 75° 11' south preceding.

The next business will be to reduce two geocentric observations to a heliocentric measure. This is to be done, as shown before, by a calculation of the angle  $\alpha$ , fig. 8. The result of it shows, that 10° 14' are to be subtracted from the mean corrected angle of position, reduced to June 25, 1781, and 23° 18' to be added to the angle which is the corrected mean of 13 measures, reduced to Oct. 4, 1783. Hence we learn, that on those days and hours, when the heliocentric places of Mars were 9<sup>s</sup> 24° 35', and 0<sup>s</sup> 7° 15' (which would happen about July 18, 1781, and Sept. 29, 1783) an observer placed in the sun would have seen, on the former, the axis of Mars inclined to the ecliptic 64° 57' the north pole being towards the left; and on the latter, he would have seen the same axis inclined to the ecliptic 78° 59', the north pole being then towards the right.

The first conclusion we may draw from these principles is, that the north pole of Mars must be directed towards some point of the heavens between 9<sup>s</sup> 24° 35' and 0<sup>s</sup> 7° 15'; because the change of the situation of the pole from left to right, which happened in the time the planet passed from one place to the other, is a plain indication of its having gone through the node of the axis. Next, we may also conclude, that the node must be considerably nearer the latter point of the ecliptic than the former; for whatever be the inclination of the axis, it will be seen under equal angles at equal distances from the node. But, by a trigonometrical process of solving a few triangles, we soon discover both the inclination of the axis, and the place where it intersects the ecliptic at rectangles (which, for want of a better term, Dr. H. calls its node). Accordingly he finds, by calculation, that the node is in 17° 47' of Pisces, the north pole of Mars being directed towards that part of the heavens; and that the inclination of the axis to the ecliptic is 59° 42'.

Dr. H. now compares the observations of an earlier date with these principles, to see how far they agree. Some of the particulars and calculations relating to them are as follow.



Times of Observ.	Estim.	Geoc. Long.	Angle $\alpha$ .	2d correc.
1779, May 9 <sup>d</sup> 12 <sup>h</sup> 0 <sup>m</sup> ....	42°....	7° 22' 20"....	+ 14° 45'....	+ 0
May 11 12 0 ....	52 ....	7 21 40 ....	+ 15 11 ....	+ 26
1777, Apr. 17 7 50 ....	63 ....	6 3 34 ....	+ 23 26	

May the 9th, 1779, the angle of position was roughly estimated at  $42^\circ$ , and May 11, at  $62^\circ$ . The great disagreement of these coarse estimations is undoubtedly owing to the very different situation of the dark spot from which they were taken; however, since he did not mean to use these observations in the calculations, they may suffice in a general way to show, that the axis of Mars was actually about that time in such a situation as the principles give it: for, reducing the two positions to the 9th of May, that of the 11th, from an allowance of  $26'$  for the situation of the planets, will become  $62^\circ 26'$ ; and a mean of the two,  $50^\circ 13'$  south preceding; which, reduced to a heliocentric observation gives  $66^\circ 30'$ , the north pole lying towards the left. Now, on calculating from the position of the node and inclination of the axis before determined, we find, that the heliocentric angle was  $62^\circ 49'$ , the north pole pointing towards the left; and a nearer agreement with these principles could hardly be expected from estimations so coarse. If we go to the year 1777, and take the position of the two bright spots observed the 17th of April, we have  $63^\circ$  south preceding; this, reduced to a heliocentric quantity, gives  $86^\circ 26'$  of inclination, the north pole being to the left. By calculating we find, that the pole was then actually  $81^\circ 27'$  inclined to the ecliptic, and pointed towards the left as seen from the sun.

The inclination and situation of the node of the axis of Mars with respect to the ecliptic being found, may thus be reduced to that planet's own orbit. Let  $EC$ , fig. 9, be a part of the ecliptic;  $OM$  part of the orbit of Mars;  $PEO$  a line drawn from  $P$ , the celestial pole of Mars, through  $E$ , that point which has been determined to be the place of the node of the axis of Mars in the ecliptic, and continued to  $O$  where it intersects the orbit of Mars. Now if, according to Mr. De Lalande, we put the node of the orbit of Mars for 1783, in  $1^\circ 17' 58''$ , we have from the place of the node of the axis (that is,  $11^\circ 17' 47''$ ) to the place of the node of the orbit, an arch  $EN$  of  $60^\circ 11'$ ; in the triangle  $NEO$ , right-angle at  $E$ , there is also given the angle  $ENO$ , according to the same author,  $1^\circ 51'$ , which is the inclination of the orbit of Mars to the ecliptic. Hence we find the angle  $EON$   $89^\circ 5'$ , and side  $ON$   $60^\circ 12'$ . Again, when Mars is in the node of its orbit, we have, by calculation from our principles, the angle  $PNE = 63^\circ 7'$ , to which adding the angle  $ENO = 1^\circ 51'$ , we have  $PNO = 64^\circ 58'$ ; from which two  $PON$  and  $PNO$ , with the distance  $ON$ , we obtain the inclination of the axis of Mars, and the place of its node with respect to that planet's own orbit; the inclination being  $61^\circ 18'$ , and the place of the node of the axis



$58^{\circ} 31'$  preceding the intersection of the ecliptic with the orbit of Mars, or in our  $19^{\circ} 28'$  of Pisces.

Being thus acquainted with what the inhabitants of Mars will call the obliquity of their ecliptic, and the situation of their equinoctial and solstitial points, we are furnished with the means of calculating the seasons on Mars; and may account, in a manner highly probable, for the remarkable appearances about its polar regions. The analogy between Mars and the earth is perhaps by far the greatest in the whole solar system. Their diurnal motion is nearly the same; the obliquity of their respective ecliptics, on which the seasons depend, not very different; of all the superior planets, the distance of Mars from the sun is by far the nearest alike to that of the earth: nor will the length of the martial year appear very different from that which we enjoy, when compared to the surprizing duration of the years of Jupiter, Saturn, and the Georgium Sidus. If then we find that the globe we inhabit has its polar regions frozen and covered with mountains of ice and snow, that only partly melt when alternately exposed to the sun, we may well be permitted to surmise that the same causes may probably have the same effects on the globe of Mars; that the bright polar spots are owing to the vivid reflection of light from frozen regions; and that the reduction of those spots is to be ascribed to their being exposed to the sun. In the year 1781, the south polar spot was extremely large, which we might well expect, since that pole had but lately been involved in a whole twelve-months darkness and absence of the sun; but in 1783 it was considerably smaller than before, and it decreased continually from the 20th of May till about the middle of Sept., when it seemed to be at a stand. During this last period the south pole had already been above 8 months enjoying the benefit of summer, and still continued to receive the sun-beams; though, towards the latter end, in such an oblique direction as to be but little benefited by them. On the other hand, in the year 1781, the north polar spot, which had then been its twelve-month in the sunshine, and was but lately returning to darkness, appeared small, though doubtless increasing in size. Its not being visible in the year 1783 is no objection to these phenomena, being owing to the position of the axis, by which it was removed out of sight; most probably, in the next opposition we shall see it renewed, and of considerable extent and brightness; as, by the position of the axis of Mars, the sun's southern declination will then be no more than  $6^{\circ} 25'$  on that planet.

*Of the spheroidical figure of Mars.*—That a planetary globe, such as Mars, turning on an axis, should be of a spheroidical form, will easily find admittance, when two familiar instances in Jupiter and the Earth, as well as the known laws of gravitation and centrifugal force of rotatory bodies, lead the way to the recep-



tion of such doctrines. So far from creating difficulties or doubts, it will rather appear singular, that the spheroidical form of this planet, which the following observations will establish, has not already been noticed by former astronomers; and yet, reflecting on the general appearances of Mars, we soon find that opportunities for making observations on its real form cannot be very frequent: for, when it is near enough to be viewed to advantage, we see it generally gibbous, and its oppositions are so scarce, and of so short a duration, that in more than 2 years' time we have not above 3 or 4 weeks for such observations. Besides, astronomers being already used to see this planet generally distorted, the spheroidical form might easily be overlooked. Dr. H. then gives a number of observations relating to the polar flattening of Mars, showing that there is always a difference between the observed polar and equatorial diameters of that planet, the former being the less in proper positions.

We find that the quick alterations in the visible disc of Mars, during the time it is in the best situation for us to observe it, are such, that if we were to use many measures which have been taken of its diameters, we should be obliged to have recourse to a computation of its phases, in order to make proper allowance for them. Now, since these changes are in a longitudinal direction, and the poles of Mars are not perpendicular to the ecliptic, it would bring on a calculation of small quantities, which it is always best not to run into where it can be avoided. For this reason, Dr. H. at once settles the proportion of the equatorial to the polar diameter of this planet, from the measures which were taken on the very day of the opposition. He prefers them also on another account, which is, that they were made in a very fine, clear air, and were repeated with a very high power, and with two different instruments, of whose faithful representation of celestial objects, the many observations on very close double stars he had made with them gave him very evident proofs.

Being only in quest of the proportion of one diameter to the other, the measures of the 20-foot reflector, though not given in angular quantities, will equally suffice for the purpose. By them we have the equatorial diameter to the polar as 103 to 98, or as 1355 to 1289. He turned the proportion into the latter numbers by way of comparing them the better with the measures of the 7-foot reflector. By that instrument the equator of Mars, Oct. 1, was measured 3 times; but from the remarks annexed to the different results, he thinks the 3d measure should be used. Indeed, on taking the difference of the first two, which is  $34''$ , and dividing by 3, we have the quotient  $11\frac{1}{3}''$ ; then, allowing  $\frac{2}{3}$  to the first, because the remark says positively "narrow measure," it becomes  $22'' 34\frac{2}{3}''$ , and taking  $\frac{1}{3}$  from the 2d, which is expressed doubtfully, "rather too full," it becomes  $22'' 35''$ : this reflection on the first two measures gives additional validity to the 3d, which is  $22'' 35''$ , or  $1355''$ . The polar diameter was



measured twice; and as no reason appears against either of the observations, he takes the mean of both, which is  $21'' 29'''$ , or  $1289'''$ ; so that by these measures the equatorial diameter of Mars is to the polar as 1355 to 1289. A less perfect agreement between the proportions of the diameters arising from the measures of the 20-feet reflector and those now deduced from the 7-feet, would have been sufficient for our purpose, as we might easily have excused 1 or 2000ths of the whole quantity; however, we have no cause to be displeased with this coincidence, though it should in part be owing to accident, and therefore shall admit the above proportion, and proceed to a further examination of it.

In the first place, it will be necessary to see whether any correction be required on account of the different heliocentric and geocentric south latitude of Mars; which would apparently compress the polar diameter a little, by the defect of illumination on the north. On computation we find, that a difference arising from that cause would give the longitudinal diameter to the latitudinal as 20000 to 19987; which being much less than a 1000th part of the whole, may therefore be neglected. But next, a very considerable correction must be admitted, when we take into account the position of the axis of Mars. The declination of the sun on that planet, at the time the measures were taken, was not less than  $27^\circ$  south; so that the poles were not in the circumference of the disc by all that quantity. On a supposition then, that the figure of Mars is an elliptical spheroid, we are now to find the real quantity of the polar diameter from the apparent one. It has been proved, that, in the ellipsis, the excesses of any diameters above the polar one are as the squares of the cosines of the latitudes; but the diameter at rectangles to the equator of Mars, which was exposed to view in the late opposition, was not the polar one, but such as must take place in a latitude of  $63^\circ$ . Putting therefore  $m = \text{cosine of } 63^\circ$ ,  $a = 1355$ ,  $b = 1289$ ,  $x = \text{the polar axis}$ , we have  $1 : m^2 :: a - x : b - x$ , which gives  $x = 1272$  nearly, for the polar diameter. The true proportion therefore, of the equatorial to the polar diameter, will be as 1355 to 1272; which, reduced to smaller but less accurate numbers, is as 16 to 15 nearly.

On the subject of the figure of Mars, says Dr. H., I ought to remark also, that perhaps the measures which were taken of its diameters during the last opposition will enable us to ascertain its real size with greater accuracy than has been done before. The micrometer which can distinguish with precision between the equatorial and polar diameters of this small planet, will certainly be admitted as an evidence of considerable consequence; and since the result of these measures is pretty different from what former observations give us, I should not omit mentioning it. We have seen that the equatorial diameter, on the day of the opposition, measured  $22'' 35'''$ . The distance of Mars from the earth at that time was .40457, the mean distance of the earth from the sun being 1; therefore,  $22'' 35'''$  reduced to the same distance will be no more than  $9'' 8'''$ .



I shall conclude this subject with a consideration relating to the atmosphere of Mars. Dr. Smith, Optics, § 1096, reports an observation of Cassini's, where "a star in the water of Aquarius, at the distance of 6 minutes from the disc of Mars, became so faint before its occultation, that it could not be seen by the naked eye, nor with a 3-feet telescope." It is not mentioned what was the magnitude of the star; but, from the circumstance of its becoming invisible to the naked eye, we may conclude that it must have been of the 6th or 7th magnitude at least. The result of this observation would indicate an atmosphere of such an extraordinary extent, since at the distance of 36 semi-diameters of the planet it should still be dense enough to render so considerable a star invisible, that it will certainly not be amiss to give an observation or two which seem of a very different import. These were 1783, Oct. 26 and 27, when 2 small fixed stars preceding Mars, of different sizes, were viewed with different powers, when they were distinctly seen, on the former day at the distance of  $3' 9''$ ; and on the latter  $2' 56''$ .

The larger of the 2 stars on which the above observations were made cannot exceed the 12th, and the smaller the 13th or 14th magnitude; and there is no reason to suppose that they were any otherwise affected by the approach of Mars, than what the brightness of its superior light may account for. From other phenomena it appears however, that this planet is not without a considerable atmosphere; for, besides the permanent spots on its surface, I have often noticed occasional changes of partial bright belts; and also once a darkish one, in a pretty high latitude. And these alterations we can hardly ascribe to any other cause than the variable disposition of clouds and vapours floating in the atmosphere of that planet.

*Result of the contents of this paper.*

The axis of Mars is inclined to the ecliptic  $59^{\circ} 42'$ .

The node of the axis is in  $17^{\circ} 47'$  of Pisces.

The obliquity of the ecliptic on the globe of Mars is  $28^{\circ} 42'$ .

The point Aries on the martial ecliptic answers to our  $19^{\circ} 28'$  of Sagittarius.

The figure of Mars is that of an oblate spheroid, whose equatorial diameter is to the polar one as 1355 to 1272, or as 16 to 15 nearly.

The equatorial diameter of Mars, reduced to the mean distance of the earth from the sun, is  $9'' 8'''$ .

And that planet has a considerable but moderate atmosphere, so that its inhabitants probably enjoy a situation in many respects similar to ours.

XX. *A Description of the Teeth of the Anarrhichas Lupus Linnæi, and of those of the Chætodon Nigricans of the same Author; to which is added, an Attempt*



*to prove that the Teeth of Cartilaginous Fishes are perpetually renewed. By Mr. Wm. Andre, Surgeon. p. 274.*

The amazing variety in the external form of fishes must be obvious to a common observer; and whoever examines will be convinced, that the same variety prevails in their internal structure. No parts perhaps afford a more convincing proof of the last assertion than the teeth of fishes. To adduce a few instances, let us only recollect the tuberculated teeth in the thorn-back; the triangular serrated teeth in the shark; the slender flexible teeth in the chætodontes, or angel fishes. There is not only a difference of their form, but also in the substances of which they are composed; some being of a soft horny nature; others made up of bone; others of that substance we call enamel in the teeth of quadrupeds; and some having the apparent hardness and transparency of crystal. We may also notice their uncommon situation; many fishes having teeth not only in their jaws, but on the tongue, the palate, and about the fauces.

To illustrate in some degree this part of natural history, Mr. A. describes the teeth of the anarrhichas lupus, or sea wolf, and those of the chætodon nigricans, a species of angel fish. The former have been but imperfectly described, and never represented distinct from the fish, without which it is impossible to have any exact idea of their disposition, number, or form, while the true shape and composition of the latter, from their minuteness, have been entirely overlooked. He then attempts to prove, that a continual renovation of the teeth takes place in cartilaginous fishes.

The sea wolf, a fierce and ravenous fish, is found in the northern parts of the globe, where it frequently grows to the length of 4 feet and upwards. The jaws of the wolf fish are made up of several bones, to each of which a greater or less number of teeth are affixed. The palate, marked A in fig. 10, pl. 7, is a kind of basis or support to the other bones, to which they are all more or less connected. This is a thick and firm bone united above to the bones of the cranium and nose, and ending below in a flat oval surface, on which are incrustated about 12 or 13 strong, blunt, and rather flat teeth of the molar or grinder kind. Their external edges are the most prominent; by which means a hollow is formed in the middle of the palate.

The upper jaw is composed of 3 bones, 2 of which, B, B, are placed laterally, forming the sides of the upper jaw, and the 3d, C, anteriorly, making the fore part of the jaw. The 3d bone might be divided through its middle into 2 portions, firmly connected together. The side bones of the upper jaw have nearly the shape of an italic *f*. At their posterior ends may be observed a smooth articular surface, for their connection with a similar surface on the posterior extremities of the lower jaw; and on their anterior ends there are 2 rows of teeth.



The external row consists of 3 or 4 sharp or conical teeth; and the internal row of 4 or 5 blunt and rather flat ones. These bones are connected to the palate and bones of the nose by loose but strong ligaments. The 3d bone of the upper jaw, which may be called the anterior or nasal portion, is of a triangular form, connected above to the bones of the nose, and ending below in a flat surface, thick-set with sharp conical teeth. The external teeth, about 4 in number, are large and strong, and bend a little inwards; but the internal ones are small, and nearly straight, of which we may reckon about 10.

The lower jaw D, consists of 2 bones, united at their fore-parts by a strong ligament, which allows of some motion. On their anterior extremities are placed 6 large and as many small sharp and conical teeth; the large teeth are placed externally, and their points are bent a little inwards; while the small ones, which stand within them, are nearly straight. Behind these are 2 or 3 rows of grinder teeth. The external teeth stand nearly upright; but the internal ones are placed obliquely, inclining towards each other. The teeth are formed of a hard bony matter, not covered with enamel as in some animals; nor is there an equal distribution of enamel and bone as in some others. They are not fixed in sockets, but are fastened to the jaws in the same manner as the epiphyses are united to the bodies of the bones in young animals.

From the foregoing description it will appear, that the anterior sharp teeth of the sea wolf are admirably calculated for seizing its prey, while the posterior grinding teeth serve to break down the hard shells of lobsters, crabs, muscles, scollops, &c. which this animal is known to feed on. The external teeth on the sides of the upper and lower jaw being higher than those placed within them, a hollow is formed above and below, in which the convex shells of crustaceous animals, &c. are confined during their compression between the jaws, which is effected by the action of strong muscles placed on the sides of the head. The jaws being made up of a number of pieces, and connected by loose ligaments, a freedom of motion is allowed, and the collision or shock arising from the comminution of hard bodies is so much the less by being divided among a number of bones.

*Of the chaetodon nigricans.*—The individual which furnished the following account was brought from the West Indies, and measured about 5 inches in length.\* Its teeth were so small as to require the assistance of a microscope to discover their real shape. There were 14 in each jaw. They consist of a cylindrical body fixed in the jaw, above which they spread out into a broad and rather flat surface, on the edges of which are 12 or 13 denticuli, making an uncommon appearance, and totally different from the teeth of any other animal. Another singularity is

\* This fish is well represented in Du Hamel *Traité général des Pêches*, tom. 3, seconde partie, section 4, planche 12, under the name of *Chirurgien ou porte lancette*.—Orig.



their being transparent, unless viewed with a deep magnifier, when a few opaque lines may be perceived, which point out the cellular part of the tooth through which the blood vessels ramify, which are destined for its growth and nourishment. Those in the anterior parts of the jaws are the longest, whence they gradually diminish in length as they approach the angles of the mouth. From this description of the teeth of the *chætodon nigricans*, this fish seems to be misplaced in the *Systema Naturæ* of Linneus; since one generic distinction of the *chætodontes* is to have numerous, slender, and flexible teeth; whereas the teeth of the *chætodon nigricans* are few in number, placed in one row, and of a crystalline hardness.

*Of the teeth of cartilaginous fishes.*—When Steno examined the teeth of the shark, he was surprized to find a great number of them placed on the inside of each jaw, lying close to the bone, and many of them buried in a loose spongy flesh; concluding that these internal teeth could be of little or no use to the animal. Mr. Herissant afterwards showed the use of these internal or posterior teeth, by proving, that as the anterior teeth of each row are broken off, drop out, or wear away, the posterior ones come forward to supply their places. But though it be certain that the anterior teeth, when lost, are replaced by the posterior ones, none of the naturalists have attempted to ascertain how often this circumstance happens. Whether the renovation be perpetual during life; or whether that operation be suspended after a limited number of teeth have been supplied. From a singular circumstance, which Mr. A. once met with, he is inclined to think the former is the fact; or, that in cartilaginous fishes, such as sharks, rays, &c. there is a perpetual renovation of the teeth. Being engaged in dissecting the jaws of a very large shark, he found a portion of that sharp bearded bone found in the tail of the fire-flaire, or sting-ray, driven quite through the lower jaw among the posterior teeth, and fixed almost immoveably. How this happened must be obvious to every one.

The posterior teeth of cartilaginous fishes are always found in a soft, membranous state, and but imperfectly formed; yet they have the whiteness of teeth from a small quantity of calcareous earth already deposited within their substance. Their hardness and perfect form is acquired as they advance towards the anterior parts of the jaws. Of the 3 angles in each tooth of the shark, one is placed towards the right, another towards the left, and the other, which is in the middle, and the most acute angle, is directed inwardly towards the tongue or fauces. They are placed then in such a manner as that the angles of the teeth on the left side in one row, approach the angles of the teeth on the right side in the next row. As it is certain, that the anterior teeth were formerly posterior ones, and as the teeth in each row were all deficient in one angle, it follows, that they must have been formed posterior to the insertion of this extraneous body. Again,



if we allow that before the accident the animal was in possession of perfect teeth, it follows also, that they were consumed and replaced by imperfect ones. There were 6 teeth in each row, and 52 rows, making together about 312 teeth. Now allowing the consumption to have been equal in all parts of the jaws, it follows, that the animal had already consumed 312 teeth, and was in possession of a like number for future consumption. The teeth of sharks, rays, &c. may be divided into active and passive. The active teeth are the anterior ones of each row, standing with their points upwards. The passive teeth are the remaining ones, lying one over another, like the tiles on a house (imbricated,) with their points downwards. It appears from the foregoing account, that the anterior or active teeth had been replaced 6 times; and that they might have been renewed 6 times more, making in all 12 times. From which it may be reasonably concluded, that this does not happen any precise number of times; but that the renovation is perpetual during the life of the animal.

*XXI. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1783. By Thomas Barker, Esq. p. 283.*

		Barometer.			Thermometer.						Rain.		
		Highest.	Lowest.	Mean.	In the House.			Abroad.			Lyndon.	Selbourn Hamp.	S. Lamb. Surry.
		Inches.	Inches.	Inches.	Hig.	Low	Mean	Hig.	Low	Mean	Inch.	Inch.	Inch.
Jan.	Morn.	29.87	28.38	29.04	47	33½	41	46½	18½	35	1.805	4.43	1.51
	Aftern.				48	35	42	50	27½	40			
Feb.	Morn.	30.12	28.08	29.28	48	36	43	49	17½	36½	2.313	5.54	2.98
	Aftern.				49½	37	43½	53½	30	43			
Mar.	Morn.	30.01	27.88	29.28	47½	33½	40	44½	21½	33	1.604	2.16	0.93
	Aftern.				49½	33½	41½	55	33	43			
Apr.	Morn.	30.14	29.15	29.70	58	46	51	52	32½	43	0.558	0.88	0.59
	Aftern.				62½	47½	53	67½	41½	55½			
May	Morn.	29.82	29.13	29.48	63½	47	53	55½	31½	44	4.218	2.84	2.36
	Aftern.				66	48½	55	72½	44	56			
June	Morn.	29.85	28.80	29.47	65½	55½	60	63½	46	55	3.033	2.82	4.00
	Aftern.				68½	56½	61½	79	55	67			
July	Morn.	29.89	29.16	29.55	72	61½	66	68	55	61½	2.663	1.45	0.78
	Aftern.				75	67	68½	85	67	74			
Aug.	Morn.	29.83	29.17	29.49	67	57½	62	67½	46	57	1.102	2.24	2.23
	Aftern.				75	58½	64½	84	57	67			
Sept.	Morn.	29.87	28.77	29.36	62½	54	57½	59	40	50½	1.440	5.53	4.30
	Aftern.				64	56	59	68	55	60			
Oct.	Morn.	29.88	28.99	29.48	60½	46½	53	58	34	44½	0.658	1.71	0.72
	Aftern.				61½	48	54	64½	44	54			
Nov.	Morn.	29.96	28.42	29.45	51	42	46½	52½	30½	40	1.783	3.01	1.63
	Aftern.				52	42½	47	54½	37½	45½			
Dec.	Morn.	29.99	28.49	29.29	46	28	40½	43½	8½	32½	1.602	1.10	1.22
	Aftern.				46½	28	40	45½	19	37			
Mean of all .....			29.41			52		49			22.779	33.71	23.25



*XXII. On the Periods of the Changes of Light in the Star Algol. By John Goodricke, Esq. p. 287.*

As Mr. G. was now able, by collating some of his late observations on Algol with those given at p. 456, to determine with greater precision the periodical return of its changes, he wished to add this as a kind of supplement to that account. The method here pursued is by taking the intervals between accurate observations of Algol's least brightness or greatest diminution of light, made at long distances of time from each other, and dividing those intervals by a certain number of revolutions. The reason for chusing long intervals is, that the number of revolutions being greater, the errors of observation may be diminished: all error cannot however as yet be excluded, but he thinks the period is now, by the following calculation, ascertained within 10 or 15 seconds.

Mean times of Algol's  
least brightness.

1783		h	m				d	h	m	s
Jan.	14	9	25	} an interval of 99 revolutions, each of 2			2	20	49	14
Oct.	25	6	39							
Jan.	14	9	25	} .....	106	.....	2	20	49	10
Nov.	14	8	17							
Jan.	14	9	25	} .....	107	.....	2	20	49	2
Nov.	17	4	52							
Feb.	6	8	15	} .....	91	.....	2	20	49	3
Oct.	25	6	39							
Feb.	6	8	15	} .....	98	.....	2	20	48	59
Nov.	14	8	17							
Feb.	6	8	15	} .....	99	.....	2	20	48	51
Nov.	17	4	52							
Feb.	26	9	43	} .....	84	.....	2	20	49	14
Oct.	25	6	39							
Feb.	26	9	43	} .....	91	.....	2	20	49	9
Nov.	14	8	17							
Feb.	26	9	43	} .....	92	.....	2	20	49	0
Nov.	17	4	52							
Jan.	31	14	29	} .....	100	.....	2	20	49	4
Nov.	14	8	17							
Mar.	21	8	36	} .....	84	.....	2	20	48	46
Nov.	17	4	52							

Hence the period of Algol's variation is, on a mean, 2 20 49 8

It appears now, that the duration of the variation is about 8 hours.

*XXIII. Experiments on the Terra Ponderosa, &c. By Wm. Withering, M.D. p. 293.*

SECT. 1.—\**Terra Ponderosa Aerata*.—This substance was got out of a lead-mine at Alston-Moor, in Cumberland. Its general appearance is not much unlike that of a lump of alum; but on a closer inspection it seems to be composed of slender spiculæ in close contact, but more or less diverging. It may be cut

\* Carbonate of Barytes.



with a knife. Its specific gravity is from 4.300 to 4.338. It effervesces with acids, and melts under the blow-pipe, though not very readily. Placed in a covered crucible, in a hot parlour fire, it lost its transparency. After exposure to a moderate heat in a melting furnace, it adhered to the crucible, and exhibited signs of fusion; but was not diminished in weight, did not feel caustic when applied to the tongue, nor had it lost its property of effervescing with acids. Hence it is probable, that its loss of transparency was rather occasioned by numerous small cracks, than by any escape of the water of crystallization, or of its ærial acid.

*Exper.* A. 500 grs. dissolved in muriatic acid, in such a manner that nothing but elastic fluid could escape, lost in solution 104 grs. and there remained an insoluble residuum of nearly 3 grs. 2. In another experiment 100 grs. lost in solution 21 grs. and there remained 0.6 of a gr. of insoluble matter.

B. 100 grs. dissolving in dilute muriatic acid, gave out 25 oz. measures of air. This air was received in quicksilver, and when the spar was wholly dissolved, the solution was boiled in order to drive out what air might be lodged in it. 2. This air was heavier than atmospheric air; it was readily absorbed by agitation in water, it precipitated lime from lime-water, and it extinguished flame. The water which had absorbed it changed the blue colour of litmus slowly\* to a red; so that this elastic fluid was undoubtedly fixed air.

C. The solution B, by the addition of mild fossil fixed alkali, afforded a precipitate which, after proper washing and drying, weighed 100 grs. 2. This precipitate, on being again dissolved in marine acid, yielded only 20 oz. measures of fixed air.

D. To a saturated solution in marine acid mild fixed vegetable alkali was added; the earth was precipitated, and a quantity of fixed air escaped. 2. The same thing happened when mild fossil alkali was added. 3. When caustic vegetable alkali was used, the precipitation took place, but without any appearance of effervescence. 4. 50 parts dissolved in marine acid lost, during the solution, nearly 10.5. This solution, on the addition of caustic vegetable alkali, let fall a precipitate which, when washed and dried, weighed 45.5. 5. Phlogisticated alkali precipitated the whole of the earth from part of the solution D; for mild fixed alkali afterwards added to the filtered liquor occasioned no further precipitation.

E. Part of the precipitates D, 1, 2, after exposure to a strong heat in a crucible, was thrown into water. Next morning the water was completely covered with an ice-like crust, and had the acrid taste of lime-water in a very high degree.

\* Other acids turn the blue of litmus instantly to a red, whilst water, impregnated with fixed air, does not change the litmus immediately; but, after some seconds, the red colour begins to appear, and then gradually grows more distinct.—Orig.



2. The smallest portion of vitriolic acid added to this water occasioned an immediate and copious precipitation; and when this acrid water was diluted with 200 times its bulk of pure water, the precipitation on the addition of vitriolic acid was yet sufficiently obvious. 3. A single drop of this acrid water, added to solutions of tartar of vitriol, Glauber's salt, vitriolic ammoniac, alum, Epsom salt, selenite, occasioned an immediate precipitation in all of them.

F. The precipitate thrown down by the caustic vegetable alkali (D, 3,) was put into water, in expectation that it would make lime-water; but neither on standing, nor after boiling, did this water exhibit any precipitation when concentrated vitriolic acid was dropped in it; nor had it any acrimonious or other peculiar taste.

G. Concentrated vitriolic acid was added to one portion of the precipitate D 3; concentrated nitrous acid to a 2d portion; and marine acid to a 3d portion. No effervescence could be observed, nor was there any appearance of solution. After standing 1 hour water was added; and the acids thus diluted were suffered to remain on the portions of the precipitate for another hour. They were then decanted, and saturated with mild fossil fixed alkali, but without any appearance of precipitation.

H. The part precipitated by the phlogisticated alkali, when mixed with nitre and borax, and fluxed by a blow-pipe on charcoal, formed a black glass; on flint-glass, a white; and on a tobacco pipe an opaque yellowish white one. 2. Another portion melted with soap and borax in a crucible, formed a black glass, but without any metallic appearance.

I. The insoluble residuum A was boiled in water, the water decanted, and mild fixed alkali added, but without any subsequent precipitation. 2. This insoluble powder was not attacked by the nitrous or marine acids; but being put into vitriolic acid, and boiled a considerable time till the acid became highly concentrated, it dissolved entirely, and separated again on the addition of water. It will appear in the sequel, that the same thing happens to marmor metallicum, when dissolved by boiling in the acid of vitriol.

Hence it appears, that 100 parts of this spar contain, terra ponderosa pura 78.6, marmor metallicum 0.6, fixed air 20.8.

*Observations.*—1st. The quantity of mild fixed alkali necessary to saturate an acid, previously united to the terra ponderosa, contains more fixed air than is necessary to saturate that quantity of terra ponderosa D 1, 2. 2dly. The terra ponderosa, when precipitated from an acid by means of a mild fixed alkali (D 1, 2,) readily burns to lime; and this lime-water proves a very nice test of the presence of vitriolic acid. E 2, 3. 3dly. It is very remarkable, that the terra ponderosa spar, in its native state, will not burn to lime. In the lower degrees of heat it suffers no change, as before observed, besides the loss of its



transparency. When urged with a stronger fire, it melts and unites to the crucible, but does not become caustic.\*

I buried it in charcoal-dust in a covered crucible, and then exposed it to a pretty strong heat; but it did not part with its air. May we not conjecture then, that as caustic lime cannot unite to fixed air without the intervention of moisture, and as this spar seems to contain no water in its composition, that it is the want of water which prevents the fixed air assuming its elastic ærial state? This supposition becomes still more probable, if we observe that when the solution of the spar in an acid is precipitated by a mild alkali, c 1, 2, some water enters into the composition of the precipitate, for it weighs the same as before it was dissolved, and yet contains only 20 oz. measures of fixed air, while the native spar contained 25 oz. measures; so that there is an addition of weight equal to that of 5 oz. measures of fixable air, or  $3\frac{3}{4}$  grs. to be accounted for, which can only arise from the water; and this precipitate, thus united to water, readily loses its ærial acid in the fire, E 1.

4thly. Professor Bergman supposes the terra ponderosa to be a metallic earth; its entire separation from an acid by means of the phlogisticated alkali (D 5) certainly favours such a supposition; but, if it be so, it is evident from experiments H 1, 2, that other means than those commonly employed must be used to effect its reduction. 5thly. The precipitate made by the caustic vegetable alkali D 4 taking some of the alkali down with it, and thus forming a substance neither soluble in water nor in acids, is a very curious phenomenon. I afterwards varied the experiment by adding the terra ponderosa lime-water E to caustic vegetable and caustic fossil alkali. In both cases this insoluble compound was immediately formed; but not so when caustic volatile alkali was used.

6thly. As it appears from experiments D 1, 2, 3, 4, that fixed alkalis, both mild and caustic, separated the terra ponderosa from marine acid, I was at a loss to know why Professor Bergman, in his admirable table of simple elective attractions, placed the terra ponderosa caustica immediately under the vitriolic, nitrous, and marine acids, and consequently above the caustic alkalis. I was interested in the reality of the facts, because I had so seldom seen reason to doubt the observations of that very excellent chemist, and therefore made the following experiments. To different portions of terra ponderosa salita and terra ponderosa nitrata I added, drop by drop, caustic vegetable, caustic fossil, and caustic volatile alkalis. In every case the earth was thrown down; and I have so often repeated these experiments to satisfy myself and others, that I am persuaded the

\* If the crucible is of clay; but Dr. Hope, professor of chemistry at Edinburgh, expelled the carbonic acid from this fossil and obtained the barytic earth in a caustic state, by subjecting it, in a black-lead crucible, to the heat of a forge.



terra ponderosa caustica ought to be placed below the alkalis, except in the column appropriated to the vitriolic acid; and it may be separated even from that acid, by the vegetable fixed alkali, if the alkali be applied via sicca, as will appear in the next section.

7thly. The necessity for using pure acids on many occasions, and the difficulty of obtaining them pure, are sufficiently obvious. The vitriolic acid, as made in the large way, is generally pure enough for most purposes. It is apt to get coloured by inflammable matter; but this is seldom an inconvenience; and, when it would be so, it is easy to drive it off by boiling the acid in a Florence flask over a common fire. But there is another cause of impurity in this acid, which appears on diluting it with water; for then it becomes milky, and in a short time a powder subsides.\* The acid may be freed from this powder either by distillation in glass vessels, which is a tedious and dangerous process, or by the affusion of water; and after the powder has subsided, a gentle evaporation will drive off most of the superfluous water.

Nitrous acid may be freed from vitriolic and marine acids, by solution of silver in the acid of nitre, as is daily practised; but the marine acid has not, to my knowledge, been purified by any other method than the laborious one of re-distilling it from common salt. It is generally mixed with vitriolic acid, and often in large proportion. There is no temptation, and scarcely an opportunity, for it to be contaminated by nitrous acid. From the vitriolic acid then it may be readily purified by the addition of terra ponderosa caustica dissolved in water, or by the terra ponderosa salita. If the latter be used, a small portion of the acid must first be tried in a diluted state, whence we must judge how much of the terra ponderosa salita the whole will require; or else the whole of the acid must be diluted with water. Whether we use the terra ponderosa dissolved in water or in marine acid; in either case the acid of vitriol immediately seizes on it, and subsides with it in form of an insoluble powder.

SECT. 2.—*Terra Ponderosa Vitriolata*.† Bergman's *Sciagraphia*, §§ 58, 89.

\* About 2 years ago I examined this powdery matter; both that which was thrown down by dilution with water, and also some which Dr. Priestley gave me, being the residuum of vitriolic acid distilled to dryness in a flint-glass retort. 1st, Repeated boiling in water, reduced  $6\frac{1}{2}$  grs. to 2 grs. 2dly, This solution, by gentle evaporation, afforded 5 grs. of crystals, as hard and as tasteless as selenite. 3dly, To these crystals, re-dissolved in water, mild fossil alkali was added, and a white powder precipitated. 4thly, This powder, after exposure to a pretty sharp heat, was thrown into water; part of it dissolved, and the water got the taste and other properties of lime-water. 5thly, The insoluble part (1) suffered no change by boiling in nitrous acid; one-half of it mixed with borax, and exposed to the blow-pipe on charcoal, vitrified; the other half, mixed with borax and charcoal-dust, likewise vitrified. It appears then, that the greater part of this substance was calx vitriolata, or selenite; the remainder a vitrifiable earth. I had before found, that the terra ponderosa vitriolata, or heavy gypsum, would dissolve in concentrated vitriolic acid; but always separated in a powdery form on the affusion of water; and now it appears that calx vitriolata, or selenite, does the same.—Orig.

† Sulphate of barytes.



Variety; heavy gypsum. Ponderous spar. Marmor metallicum. Cronstedt Min. § 18, 2, 19, c.

From Kilpatrick hills near Glasgow. A sort, with smaller crystals, among the iron ore about Ketley in Shropshire. In the lead mines at Alston-Moor. It is white; nearly transparent, but has not the property of double refraction; composed of laminæ of rhomboidal crystals; decrepitates in the fire. Specific gravity from 4.402 to 4.440.

*Exper.* A. 100 grs. exposed to a red heat for 1 hour, in a black lead crucible, lost 5 grs. in weight; but as a sulphureous smell was perceptible, I suspected that a decomposition had taken place, and therefore exposed another portion to a similar heat for the same space of time in a tobacco-pipe. This had no smell of sulphur, nor was it diminished in weight. 2. It is barely fusible under the blow-pipe; but with borax fluxes readily into a white opaque glass.

B. 100 grs. ground in a mortar, and washed over extremely fine by repeated additions of water, were boiled in the same water, and after settling the water was poured off. The powder, when dried, had not sensibly lost weight. 2. To separate portions of the washing water, were added mild vegetable and mild fossil alkali; but without any appearance of precipitation. Nitre of mercury gave a very slight brownish cloud, barely discernible; and nitre of silver an extremely slight bluish appearance. 3. The same powder, boiled again in fresh water, did not affect the water at all; for it stood the test of nitre of silver without any change.

C. Portions of the powder B were boiled in vitriolic, nitrous, and muriatic acids, of the usual strength, for several minutes. The acids were then saturated with vegetable fixed alkali, but without any appearance of precipitation, nor had the portions of powder lost any weight. 2. But when boiled in vitriolic acid, till that acid became very much concentrated and nearly red-hot, the whole of it dissolved; but, separated again on the addition of water, was not altered in its weight, was not acted on by acids of the usual strength, and under the blow-pipe had the properties mentioned at A 2. 3. Some of the solution in the concentrated vitriolic acid was left exposed to the atmosphere, that the acid might slowly attract water. After some days, beautiful crystals appeared in the shapes of stars, fasciæ, and other radiated forms. 4. To another portion of this solution mild fixed vegetable alkali was added; but the precipitate appeared to be the marmor metallicum unchanged.

D. One ounce of this marmor metallicum in fine powder was fluxed in a crucible with 2 oz. of salt of tartar, till it ran thin. This substance, boiled with water in a Florence flask, left a residuum of 6 drams.

E. This residuum was thrown into water, and pure nitrous acid added, till there was no more effervescence. The undissolved part weighed 52 grs.

F. This undissolved part appeared to be the original substance no ways



changed; for it did not dissolve in nitrous or marine acids, but wholly dissolved in the greatly concentrated and boiling vitriolic acid, from which it was again separated by the addition of water. (c 2.)

G. The solution D was saturated with distilled vinegar, and then evaporated to dryness, but with less than a boiling heat. The sal diureticus, thus formed, was washed away with alcohol. The remaining salt weighed 5 drs. nearly. 2. This salt had the appearance and the taste of vitriolated tartar; it decrepitated in the fire; roasted with charcoal-dust, it formed a hepar sulphuris; and with muria calcarea gave a precipitation of selenite.

H. The salt formed with the nitrous acid E shot readily into beautiful permanent crystals, of a rough bitterish taste. 2. Some of this salt was deflagrated with nitre and charcoal, and the alkali afterwards washed away. 3. The residuum, being the earth of the marmor metallicum, was very white, burnt to lime, and again formed an insoluble compound with acid of vitriol.

I. 100 grs. of terra ponderosa aërata were dissolved in diluted marine acid. Vitriolic acid was dropped into this solution, till no more precipitation ensued. The precipitate, after very careful washing and drying, was exposed to a red heat in a covered tobacco-pipe for half an hour: when cool, it weighed 117 grs. 2. 50 grs. of terra ponderosa aërata in a lump were put into diluted vitriolic acid; but the action of the acid on it was hardly sensible; even when made hot. Marine acid was then added, and after some time an effervescence appeared. The terra ponderosa vitriolata, thus formed, after proper washing and drying, was exposed to a red heat for an hour: it then weighed 58.4 grs.

*Conclusions.*—1st, It appears that the marmor metallicum is composed of vitriolic acid and terra ponderosa, D, E, F, G, H. 2dly, That this compound, though probably soluble in water, has so little solubility as almost to escape detection by the nicest chemical tests, B 1, 2, 3. 3dly, That it is not soluble in acids of the usual strength; but that it perfectly and entirely dissolves in highly concentrated vitriolic acid, from which it again separates entire and unchanged on the affusion of water, C 1, 2. 4thly, That it cannot be decomposed (via humida) by mild fixed alkali, c 4. 5thly, That it may be decomposed (via sicca) by the vegetable fixed alkali, D, E, G, H. 6thly, That it may be decomposed by inflammable matter, uniting to its acid, and forming sulphur; but that it cannot be decomposed by heat alone, A 1. 7thly, From experiments I 1, 2, it appears that 100 parts of this substance contain, of vitriolic acid pure 32.8, terra ponderosa pure 67.2. For the 100 parts of terra ponderosa aërata, used in the experiment I 1, would lose during the solution 20.8 of fixed air (§ 1st, A;) then, deducting 0.6 for the marmor metallicum contained in the terra ponderosa aërata (§ 1st A 1, 2,) there remains 78.6 of pure terra ponderosa. This, when saturated with vitriolic acid, and made perfectly dry, weighed 117; consequently it had taken 38.4 of vitriolic acid.



*Observations.*—The apparent insolubility of terra ponderosa aërata in the diluted vitriolic acid 1 2, can be accounted for by remarking, that the moment the surface of the lump was acted on by the acid, an insoluble coat of marmor metallicum was formed on it, which effectually precluded any further operation of the acid. Professor Bergman, in order to obtain the earth from the terra ponderosa vitriolata, directs the latter to be roasted with fixed alkali, and the dust of charcoal; but I have always done it by charcoal-dust alone, though probably this method may require a greater degree of heat. It has been remarked, that terra ponderosa and calx of lead resemble each other in many respects; and I must add, that the vitriol of lead dissolves in the highly concentrated vitriolic acid much in the same manner that the marmor metallicum does, and like this too separates on the affusion of water; but I never observed it disposed to crystallize. The marmor metallicum may probably be useful in some cases where a powerful flux is wanted; for I once mixed some of it with the black flux, and exposing it to a pretty sharp heat, it entirely ran through the crucible. May not therefore some of the more common varieties of it be used advantageously as a flux to some of the more refractory metallic ores?

SECT. 3.—*Terra Ponderosa Vitriolata.* Variety, calk or cauk. Marmor metallicum, Cronstedt Min. § 18. B? It is plentiful in the mines in Derbyshire. Is of a white or reddish colour; crystallized in rhomboidal laminæ, but these very much intermixed and confused. Loses little or nothing of its weight by being made red-hot. Specific gravity 4.330.

*Exper.* A. Ground in a mortar, and washed over, the washing water when decanted gave no precipitation with mild vegetable alkali; but with nitre of silver and nitre of mercury the slightest cloud imaginable.

B. 100 grs. boiled in marine acid weighed, after proper washing and drying, 99.5.

C. The acid solution B let fall a Prussian blue on the addition of a single drop of phlogisticated fixed alkali; and, when saturated with mild fossil alkali, afforded an ochry-coloured precipitate.

D. This precipitate, collected and washed, weighed half a grain. It was roasted with tallow, and then was wholly attracted by a magnet.

E. A quantity of the cauk, finely powdered, was mixed with charcoal-dust, and roasted in a crucible at a white heat, for 5 hours, fresh charcoal-dust being occasionally added. It gave out a strong smell of sulphur.

F. To this roasted cauk nitrous acid was added, which dissolved the greater part of it; producing, during the solution, some effervescence, and a strong smell of hepar sulphuris.

G. Some of this solution, after proper evaporation, afforded beautiful crystals,



not deliquescent, exactly resembling those obtained from the marmor metallicum, (§ II, H.)

H. To other portions of the solution F, were added fixed vegetable and fossil alkalis, also volatile alkali, each of which precipitated the earth from the acid.

I. This earth, after exposure to a white heat for one hour, became caustic, and made lime-water, similar in properties to that mentioned at § 1st E.

K. Some of the part not acted on by the nitrous acid F, dissolved entirely by boiling in highly concentrated vitriolic acid, and wholly separated again by the affusion of water. More water was added, and the whole was boiled again; but the filtered liquor gave no signs of precipitation on the most liberal addition of mild fixed vegetable alkali.

It appears therefore, that 100 parts of Derbyshire cauk contain, of marmor metallicum 99.5, calciform iron 0.5. And it is probable, that the redder pieces contain a little more iron.

SECT. 4.—*Terra ponderosa vitriolata*. Variety, radiated cauk; gypsum crystallisatum capillare; Crönstedt Min. § 19 B. It was from Pennely by the Bog, near Minsterley, in Shropshire, 15 miles from Salop, on the road to Montgomery.

It is somewhat glossy like satin; yellowish-white, opaque; composed of slender spiculæ set close together, and pointing from a centre. In some pieces there are concentric circles of a semi-transparent horn like appearance. It is not very brittle; may be shaved with a knife; loses little or nothing of its weight by being made red-hot. Its specific gravity 4.000; but after soaking one night in the water 4.200, or more.

*Exper.* When treated in the same manner that the Derbyshire cauk was, in the preceding section, 100 parts of it appeared to contain, of marmor metallicum 97.7, calciform iron 2.3.

Suspecting that the presence of so small a proportion of iron could hardly occasion the whole of the apparent differences between the Shropshire and Derbyshire cauks and the marmor metallicum; and thinking it not improbable that they might contain lead; I mixed some of them with charcoal-dust and borax, but could not by means of the blow-pipe produce any metallic appearance, though vitriol of lead, treated in the same manner, was readily reduced. I then mixed 4 parts of cauk with 1 part of vitriol of lead; the lead could still be reduced, though not so readily as before.

*General Observations.*—The terra ponderosa seems to claim a place between the earths and the metallic calces. Like the former, it cannot be made to assume a metallic form; but, like the latter, it may be precipitated from an acid, by means of phlogisticated alkali. In many of its properties it much resembles the



calx of lead; and in others, the common calcareous earth, but still seems sufficiently different from that to constitute a new genus, as will appear from a little attention to the following circumstances.

Terra ponderosa,  
When dissolved in water, precipitates on the addition of the smallest portion of vitriolic acid. Its gypsum therefore is insoluble.  
With the nitrous and marine acid, forms crystals which do not deliquesce.

Decomposes vitriolic salts via humida.

It has been called terra ponderosa,\* or heavy earth, on account of the great specific gravity of its gypsum; its spar is likewise heavy enough to countenance such an appellation; but the earth itself does not appear to be a heavy substance, and I imagine the great weight of its compounds with the vitriolic and aërial acids is owing to the absence of water.

Terra calcarea,  
Dissolved in water, does not precipitate on the addition of vitriolic acid.  
Its gypsum therefore is soluble.  
With nitrous and marine acids, forms salts so deliquescent that they cannot be kept in a crystallized form.

Does not decompose vitriolic salts.

*XXIV. Observations on the Transit of Mercury over the Sun's Disc, Nov. 12, 1782, made at the Royal Observatory at Paris. By J. Wm. Wallot. From the French. p. 312.*

Mr. W. says he observed this transit along with M. Cassini, with a 3-feet achromatic telescope by Dollond. The transit was attended by two unfavourable circumstances, the sun's proximity to the horizon, and the near passage of Mercury to the sun's limb. But the fine weather made some compensation, and enabled them to make good observations, which are reduced to the true time of the meridian of Paris observatory. The times of the immersion and emersion are given as below: viz. at

2 <sup>h</sup>	56 <sup>m</sup>	28 <sup>s</sup> .8	the first exterior contact,
2	58	28.8	half immersed, or ☿'s centre on the sun's limb,
3	2	3.8	total immersion, or internal contact,
3	3	45.8	Mercury quite detached from the limb,
4	17	18.4	interior contact at the exit,
4	20	36.4	half emersed, or ☿'s centre on the ☉'s limb,
4	22	53.4	exterior contact, or end of the transit.

In measuring the planet's diameter when on the sun's disc, Mr. W. found it twice the same quantity, viz. 9 parts of the objective micrometer, which are equal to 9".535 of a degree. The sun's limb was so undulatory that Mercury, towards the final exit, exactly resembled a body floating on the waves of water much agitated, frequently disappearing and appearing again. Mr. W. saw no-

\* Termed by subsequent chemists, barytes.



thing of either atmosphere or nebulosity about Mercury during all the time of the transit; and yet he is persuaded that such an atmosphere really exists.

Mr. W. calculated the places of the sun and of Mercury by Halley's tables for  $2\frac{1}{2}^h$ ,  $3\frac{1}{2}^h$ , and  $4\frac{1}{2}^h$ , a space of time including the whole duration of the transit, whence he found as follows, in true time.

	at $2^h 30^m$			at $3^h 30^m$			at $4^h 30^m$		
The longitude of the sun	.....7 <sup>s</sup>	20° 22'	43".6....7 <sup>s</sup>	20° 25'	14".8....7 <sup>s</sup>	20° 27'	45".9		
His right ascension	.....7	17 55	55.3....7	17 58	28.4....7	18 1	1.5		
His declination, south	.....	17 51	49.6....	17 52	29.9....	17 53	10.1		
☿'s geocentric longitude	.....7	20 32	2.9....7	20 28	40.8....7	20 25	18.4		
His latitude, north	.....	0 14	31.0....	0 15	22.6....	0 16	13.8		
Which gives	between $2\frac{1}{2}^h$ and $3\frac{1}{2}^h$ between $3\frac{1}{2}^h$ and $4\frac{1}{2}^h$								
☿'s relative horary motion in the ecliptic	° 5' 33".3			° 5' 53".5					
Inclination of his orbit to the ecliptic	8 18 33.8			8 14 28.5					
Horary motion in his orbit	5 57.05			5 57.19					

The diameter of the sun used in the calculations was  $32' 24''.5$ , and that of Mercury  $9''.535$  as found above; and the sun's parallax  $8''.7$  at its mean distance from the earth. Whence Mr. W. concludes the difference of the horizontal parallaxes of the sun and Mercury for the day of the transit, Nov. 12, to be  $4''.088$ . With these elements he calculated the observations of the contacts, the chief results of which are as in the following table.

*Table of the Results of the Calculation of the Observations of the Contacts and of the Centre of Mercury.*

	Interior contacts.		Exterior contact.	Centre of ☿.
True time of the observed { beginning	.....	3 <sup>h</sup> 2 <sup>m</sup> 3 <sup>s</sup> . 8..	2 <sup>h</sup> 56 <sup>m</sup> 28 <sup>s</sup> . 8..	2 <sup>h</sup> 58 <sup>m</sup> 28 <sup>s</sup> . 8
True time of the observed { end	.....	4 17 18. 4..	4 22 53. 4..	4 20 36. 4
Duration given by observation	.....	1 15 14. 6..	1 26 24. 6..	1 22 7. 6
Least dist. of the cent. seen from the earth's surf.	.....	15 41. 2..	15 42. 5..	15 41. 0
True time of the middle for the earth's centre	..	3 39 47. 4..	3 39 47. 1..	3 39 38. 7
Least dist. of the cent. seen from the earth's cent.	..	15 45. 1..	15 46. 4..	15 44. 9
Reduction of the observation to the { beginning	.....	+ 2 59.45..	+ 2 34.38..	+ 2 42. 9
Reduction of the observation to the { end	.....	- 2 46.70..	- 2 22.27..	- 2 30. 7
True time of observation at the { beginning	.....	3 5 3.25..	2 59 3.11..	3 1 11. 7
True time of observation at the { end	.....	4 14 31.70..	4 20 31.13..	4 18 5. 7
Duration for the earth's centre	.....	1 9 28.45..	1 21 28.02..	1 16 54. 0
Time of the ☿ of ☉ and ☿	.....	4 2 53. 2..	4 2 54. 8..	4 2 44. 1
Latitude of ☿ in the ☿ by observation	.....	° 15' 55".1..	° 15' 56".4..	° 15' 54".8
Longitude of the ☉ or ☿ in the ☿	.....	7 20 26 37. 6..	7 20 26 37. 7..	7 20 26 37. 2
Longitude of ☿ in the ☿ allowing for aberration	.....	7 20 27 8.4..	7 20 27 8.3..	7 20 27 8. 9
Latitude of ☿ in the ☿ by the tables, north	.....	15 50. 7..	15 50. 7..	15 50. 5
Error of the tables { in longitude	.....	- 30.8	- 30.6	- 31.7
Error of the tables { in latitude	.....	+ 4.4	+ 5.7	+ 4.4
Adopting the lat. of ☿ at the ☿ given by the interior contacts $15' 55''.1$ , Mr. W. finds the place of the ☿ of ☿ to be				
1 <sup>s</sup> 15° 45' 22".8 supposing the inclin. of the orbit 7° 0' 0" with Cassini,				
1 15 44 57. 7 ..... 6 59 20 with Halley.				

It appears by this table that the interior contacts give the time of the middle of the transit within  $\frac{3}{10}$  of a second of the same by the exterior contacts; the



time of the conjunction within  $1''.6$  of the same; and the nearest distance of the centres, as well as the latitude of Mercury in the conjunction,  $1''.3$  too little. As to the two observations of the centre of Mercury on the sun's limb, they give the middle of the transit  $8''.7$  sooner than the interior contacts, and the nearest distance of the centres, as also the latitude of Mercury in the conjunction, only  $\frac{2}{10}$  of a second too little.

Though the result of the calculations agree sufficiently well, yet Mr. W. is not satisfied to find the least distance of the centres  $1''.3$  greater by the exterior contacts than by the interior; a difference which shows an error in the duration; either that the duration between the two exterior contacts is too small, or that of the interior contacts too great. The cause of this difference he expects to find in the effect of a supposed atmosphere about Mercury, or some such cause; and in effect, by examination, he sees no reason to doubt of its being the effect of a refraction or inflection of the solar rays in their passage near the surface of Mercury.

*XXV. Thoughts on the Constituent Parts of Water and of Dephlogisticated Air; with an Account of some Experiments on that Subject. By Mr. James Watt,\* Engineer. p. 329.*

1. It has been known for some time, that inflammable air contained much phlogiston; and Dr. Priestley has found, by some experiments made lately, that it "is either wholly pure phlogiston, or at least that it contains no apparent mixture of any other matter." In my opinion however, it contains a small quantity of water, and much elementary heat.† He found, that by exposing the calces of metals to the solar rays, concentrated by a lens, in a vessel containing inflammable air only, the calces of the softer metals were reduced to their metallic state; and that the inflammable air was absorbed in proportion as they became phlogisticated; and, by continually supplying the vessel with inflammable air, as it was absorbed, he found, that out of 101 oz. measures, which he had put into the vessel, 99 oz. measures were absorbed by the calces, and only 2 oz. measures remained, which, on examination, he found to be nearly of the same quality the whole quantity had been of before the experiment, and to be still capable of deflagrating in conjunction with atmospheric or with dephlogisticated air. Therefore, as so great a quantity of inflammable air had been absorbed by the metallic calces; the effect of reducing them to their metallic state had been produced;

\* The celebrated improver of the steam-engine; to the augmented power and diversified application of which, may, in a great measure, be attributed the present flourishing condition of some of the most important arts and manufacturing concerns in this country.

† Previous to Dr. Priestley's making these experiments, M. Kirwan had proved, by very ingenious deductions from other facts, that inflammable air was, in all probability, the real phlogiston, in an aerial form. These arguments were perfectly convincing to me; but it seems more proper to rest that part of the present hypothesis on the direct experiment.—Orig.



and the small remaining portion was still unchanged, at least had suffered no change which might not be attributed to its original want of purity; it was reasonable to conclude, that inflammable air must be the pure phlogiston, or the matter which reduced the calces to metals.

2. The same ingenious philosopher mixed together certain proportions of pure dry dephlogisticated air and of pure dry inflammable air in a strong glass vessel, closely shut, and then set them on fire by means of the electric spark, in the same manner as is done in the inflammable air pistol. "The first effect was the appearance of red heat or inflammation in the airs, which was soon followed by the glass vessel becoming hot. The heat gradually pervaded the glass, and was dissipated in the circumambient air, and as the glass grew cool, a mist or visible vapour appeared in it, which was condensed on the glass in the form of moisture or dew.\* When the glass was cooled to the temperature of the atmosphere, if the vessel was opened with its mouth immersed in water or mercury, so much of these liquids entered, as was sufficient to fill the glass within about  $\frac{1}{100}$  part of its whole contents; and this small residuum may safely be concluded to have been occasioned by some impurity in one or both kinds of air. The moisture adhering to the glass, after these deflagrations, being wiped off, or sucked up, by a small piece of sponge paper, first carefully weighed, was found to be exactly, or very nearly, equal in weight to the airs employed. In some experiments, but not in all, a small quantity of a sooty-like matter was found adhering to the inside of the glass," the origin of which is not yet investigated; but Dr. Priestley thinks, that it arises from some minute grains of the mercury that was used to fill the glass with the air, which being super-phlogisticated by the inflammable air, assumed that appearance: but, from whatever cause it proceeded, the whole quantity of sooty-like matter was too small to be an object of consideration, particularly as it did not occur in all the experiments.

I am obliged to Mr. De Luc for the account of the experiments which have been lately made at Paris on this subject, with large quantities of these 2 kinds of air, by which the essential point seems to be clearly proved, that the deflagration or union of dephlogisticated and inflammable air, by means of ignition, produces a quantity of water equal in weight to the airs; and that the water thus produced appeared, by every test, to be pure water. As I am not furnished with any particulars of the manner of making the experiment, I can make no observations on it, only that from the character of the gentlemen who made it, there is no reason to doubt of its being made with all necessary precautions and accuracy, which was further secured by the large quantities of the 2 airs consumed.

3. "Let us now consider what obviously happens in the case of the deflagration of the inflammable and dephlogisticated air. These 2 kinds of air unite with

\* I believe that Mr. Cavendish was the first who discovered that the combustion of dephlogisticated and inflammable air produced moisture on the sides of the glass in which they were fired.—Orig.



violence, they become red-hot, and on cooling totally disappear. When the vessel is cooled, a quantity of water is found in it equal to the weight of the air employed. This water is then the only remaining product of the process, and water, light, and heat, are all the products," unless there be some other matter set free which escapes our senses. "Are we not then authorised to conclude, that water is composed of dephlogisticated air and phlogiston, deprived of part of their latent or elementary heat; that dephlogisticated or pure air is composed of water deprived of its phlogiston, and united to elementary heat and light; and that the latter are contained in it in a latent state, so as not to be sensible to the thermometer or to the eye; and if light be only a modification of heat, or a circumstance attending it, or a component part of the inflammable air, then pure or dephlogisticated air is composed of water deprived of its phlogiston and united to elementary heat?"

4. "It appears, that dephlogisticated water," or, which may be a better name for the basis of water and air, the element Mr. De Luc calls humor, "has a more powerful attraction for phlogiston than it has for latent heat, but that it cannot unite with it, at least not to the point of saturation, or to the total expulsion of the heat, unless it be first made red-hot," or nearly so. "The electric spark heats a portion of it red-hot, the attraction between the humor and the phlogiston takes place, and the heat which is let loose from this first portion heats a second, which operates in a like manner on the adjoining particles, and so continually till the whole is heated red-hot and decomposed." Why this attraction does not take place to the same degree in the common temperature of the atmosphere, is a question I am not yet able to solve; but it appears, that, in some circumstances, "dephlogisticated air can unite, in certain degrees, with phlogiston without being changed into water." Thus Dr. Priestley has found, that by taking clean filings of iron, which alone produce only inflammable air of the purest kind, and mercurius calcinatus per se, which gives only the purest dephlogisticated air, and exposing them to heat, in the same vessel, he obtained neither dephlogisticated nor inflammable air, "but in their place fixed air." Yet it is well known, that a mixture of dephlogisticated and inflammable air will remain for years in close vessels in the common heat of the atmosphere, without suffering any change, the mixture being as capable of deflagration at the end of that time as it was when first shut up. These facts the Doctor accounts for, by supposing that the 2 kinds of air, when formed at the same time in the same vessel, can unite in their nascent state; but that, when fully formed, they are incapable of acting on each other, unless they are first set in motion by external heat. Phlogisticated air seems also to be another composition of phlogiston and dephlogisticated air; but in what proportions they are united, or by what means, is still unknown. It appears to be very probable, that fixed air contains a greater



quantity of phlogiston than phlogisticated air does, because it has a greater specific gravity, and because it has more affinity with water.

5. For many years I have entertained an opinion, that air was a modification of water, which was originally founded on the facts that in most cases, wherein air was actually made, which should be distinguished from those wherein it is only extricated from substances containing it in their pores, or otherwise united to them in the state of air, the substances were such as were known to contain water as one of their constituent parts, yet no water was obtained in the processes, except what was known to be only loosely connected with them, such as the water of the crystallization of salts. This opinion arose from a discovery, that the latent heat contained in steam diminished in proportion as the sensible heat of the water from which it was produced increased; or, in other words, that the latent heat of steam was less when it was produced under a greater pressure, or in a more dense state, and greater when it was produced under a less pressure, or in a less dense state; which led me to conclude, that when a very great degree of heat was necessary for the production of the steam, the latent heat would be wholly changed into sensible heat; and that, in such cases the steam itself might suffer some remarkable change. I now abandon this opinion in so far as relates to the change of water into air, as I think that may be accounted for on better principles.

6. In every case, wherein dephlogisticated air has been produced, substances have been employed, some of whose constituent parts have a strong attraction for phlogiston, and, as it would appear, a stronger attraction for that substance than humor has; they should therefore dephlogisticate the water or fixed air, and the humor thus set free should unite to the matter of fire and light, and become pure air. Dephlogisticated air is produced in great abundance from melted nitre. The acid of nitre has a greater attraction for phlogiston than any other substance is known to have; and it is also certain, that nitre, besides its water of crystallization, contains a quantity of water as one of its elementary parts, which water adheres to the other parts of the nitre with a force sufficient to enable it to sustain a red heat. When the nitre is melted, or made red-hot, the acid acts on the water and dephlogisticates it; and the fire supplies the humor with the due quantity of heat to constitute it air, under which form it immediately issues. It is not easy to tell what becomes of the acid of nitre and phlogiston, which are supposed to be united, as they seem to be lost in the process. Dr. Priestley has lately made some experiments, with a view to ascertain this point. He distilled dephlogisticated air from pure nitre, in an earthen retort glazed within and without. He employed 2 oz. = 960 grs. of nitre: the retort was placed in an air furnace, and, by means of an intense heat, he obtained from the nitre in one experiment 787, and in another experiment 800



oz. measures of dephlogisticated air; and he found that, on weighing the retort and nitre before and after the process, they had suffered a loss of weight equal to the weight of the air, and to the water of crystallization of the nitre, but nothing more. He remarked that the air had a pungent smell, which he could not divest it of by washing; and that the water in which the air was received had become slightly acid. I examined a portion of this water, and found by it that the whole of the receiving water had contained the acid belonging to 2 drams = 120 grs. of nitre. I also examined the residuum, and the retort in which the distillation had been performed, and found the residuum highly alkaline, yet containing a minute quantity of phlogisticated nitrous acid. It had acted considerably on the retort, and had dissolved a part of it, which was deposited in the form of a brownish powder, when the saline part was dissolved in water. This earthy powder I have not yet thoroughly examined, but have no doubt that it principally consists of the earth of the retort. This experiment, and all others tried in earthen vessels, leave us still at a loss to determine what becomes of the acid and phlogiston. They seem either to remain mixed with the air, in the form of an incoercible gas; or to unite with the alkali, or with the earth of the retort, in some manner so as not to be easily separated from them; or else they are imbibed by the retorts themselves, which are sufficiently porous to admit of such a supposition. All that appears to be conclusive from this experiment is, that above one-half of the weight of the nitre was obtained in the form of dephlogisticated air; and that the residuum still contained some nitrous acid united to phlogiston.

7. Finding that the action of the nitre on the retort tended to prevent any accurate examination of the products, I had recourse to combinations of the nitrous acid with earths from which the dephlogisticated air is obtained with less heat than from nitre itself. As these processes have been particularly described by Dr. Priestley, by Mr. Scheele, and others, I shall not enter into any detail of them; but shall mention the general phenomena which I observed, and which relate to the present subject. The earths I used were magnesia alba, calcareous earth, and minium or the red calx of lead. I dissolved them in the respective experiments in nitrous acid dephlogisticated by boiling, and diluted with proper proportions of water. I made use of glass retorts, coated with clay; and I received the air in glass vessels, whose mouths were immersed in a glazed earthen bason, containing the smallest quantity of water that could be used for the purpose. As soon as the retort was heated a little above the heat of boiling water, the solutions began to distil watery vapours containing nitrous acid. Soon after these vapours ceased, yellow fumes, and in some of the cases dark red fumes, began to appear in the neck of the retort; and at the same time there was a production of dephlogisticated air, which was greater in quantity from some of these



mixtures than from others, but continued in all of them till the substances were reduced to dryness. I found, in the receiving water &c. very nearly the whole of the nitrous acid used for their solution, but highly phlogisticated, so as to emit nitrous air by the application of heat; and there is reason to believe, that with more precaution the whole might have been obtained.

8. As the quantity of dephlogisticated air produced by these processes did not form a sufficient part of the whole weight, to enable me to judge whether any of the real acid entered into the composition of the air obtained, I ceased to pursue them further, having learned from them the fact, that however much the acid and the earths were dephlogisticated before the solution, the acid always became highly phlogisticated in the process. In order to examine whether this phlogiston was furnished by the earths, some dephlogisticated nitrous acid was distilled from minium till no more acid or air came over. More of the same acid was added to the minium as soon as it was cold, and the distillation repeated, which produced the same appearance of red fumes and dephlogisticated air. This operation was repeated a third time on the same minium, without any sensible variation in the phenomena. The process should have been still further repeated, but the retort broke about the end of the 3d distillation. The quantity of minium used was 120 grs. and the quantity of nitrous acid added each time was 240 grs. of such strength that it could dissolve half its weight of mercury, by means of heat.

It appears from this experiment, that unless minium be supposed to consist principally of phlogiston, the source of the phlogiston, thus obtained, was either the nitrous acid itself, or the water with which it was diluted; or else that it came through the retort with the light, for the retort was in this case red-hot before any air was produced; yet this latter conclusion does not appear very satisfactory, when it is considered, that in the process wherein the earth made use of was magnesia, the retort was not red-hot, or very obscurely so, in any part of the process; and by no means luminous when the yellow and red fumes first made their appearance.

9. As the principal point in view was to determine whether any part of the acid entered into the composition of the air, I resolved to employ some substance which would part with the acid in a moderate heat, and also give larger quantities of air than had been obtained in the former processes. Mercury was thought a proper substance for this purpose. 240 grs. of mercury were put into a glass retort, with 480 grs. of diluted dephlogisticated nitrous acid, which was the quantity necessary to dissolve the whole of the mercury; a gentle heat was applied, and as soon as the common air contained in the retort was dissipated, a vessel was placed to receive the nitrous air proceeding from the solution, which was 16 oz. measures. When it had ceased to give nitrous air, the neck of the



retort became hot from the watery steams of the acid. The air receiver was taken away, and a common receiver was luted on, with a little water in it, to condense the vapours, and a quantity of dilute, but highly phlogisticated, acid was caught in the receiver. When the watery vapours had nearly come over, and yellow fumes appeared in the neck of the retort, the common receiver was removed, and the air receiver replaced; about 4 oz. of very strong nitrous air passed up immediately, the fumes in the retort became red, and dephlogisticated air passed up, which, uniting with the nitrous air in the receiver, produced red fumes in the receiver; and the 2 kinds of air acting on each other, their bulk was reduced to the  $\frac{1}{2}$  oz. measure. At this period the fumes in the retort were of a dark red colour, and dephlogisticated air was produced very fast. After a short time some orange-coloured sublimate appeared in the upper part of the retort, and extended a little way along its neck, the red colour of the fumes gradually disappeared, and the neck of the retort became quite clear. At the time when this happened, small globules of mercury appeared in the neck of the retort, and accumulated there till they ran down in drops. The production of the air was now very rapid, and accompanied with much of the white cloud or powdery matter, which passed up with the air into the receiver, and mixed with the water, but did not dissolve in it. After giving about 36 oz. measures of dephlogisticated air, it suddenly ceased to give any more; and the retort being cooled, the bulb was found to be quite empty, excepting a small quantity of black powder, which, on being rubbed on the hand, proved to be mostly running mercury. The orange-coloured sublimate was washed out of the neck of the retort, and what running mercury was in it was separated, and added to that which had run down into the bason among the water. The whole fluid mercury, when dried, weighed 218 grs.; therefore 22 grs. remained in the form of sublimate, which I believe would also have been reduced if I could have applied heat in a proper manner to the neck of the retort, as some of it, to which heat could be applied, disappeared.

10. The 16 oz. measures of nitrous air, which had been produced in the solution of the mercury, and had remained confined by water in the receiver, was converted into nitrous acid by the gradual admission of common air, and was taken up by the water; this water was added to that in the bason, which had served to receive the dephlogisticated air. The whole quantity was about 2 quarts, was very acid to the taste, and sparkling with nitrous air. It was immediately put into bottles, and well corked, till it had lost the heat gained in the operation. In order to determine the quantity of acid in the receiving water and in the sublimate, I dissolved fixed alkali of tartar in water, and filtered the solution. 352 grs. of this alkaline solution saturated 120 grs. of the nitrous acid I had employed to dissolve the mercury, and 1395 grs. of the same alkaline solu-



tion saturated the orange-coloured precipitate, and all the acid liquor obtained from the process: therefore we have the proportion as  $352 : 120 :: 1395 : 475$ , from which it appears, that all the acid employed was recovered again in the form of acid, excepting only 5 grs.; a smaller quantity than what might reasonably be supposed to be lost in the process by the extreme volatility of the nitrous air. In order to ascertain the exact point of saturation, slips of paper, stained by the juice of the petals of the scarlet rose, were employed, which were the nicest test I could procure, as litmus will not show the point of saturation of any liquor containing much phlogisticated nitrous acid, or even fixed air, but will turn red, and show it to be acid, when the test of those leaves, violets, and some other of the like kind, will turn green in the same liquor, and show it to be alkaline. But the exact point of saturation of so dilute a liquor is so very difficult to ascertain, that an error might easily be committed, notwithstanding the attention bestowed on it. Supposing this experiment to be unexceptionable, the conclusions which may be drawn from it are very favourable to the hypothesis I endeavour to support. Thirty-six oz. measures of dephlogisticated air were obtained, and only 5 grs. of a weak nitrous acid were lost in the process. 218 grs. of mercury out of 240 were revived, and all the dephlogisticated nitrous acid employed is found to be highly phlogisticated in the process. It appears, that the nitrous acid does not enter into the composition of dephlogisticated air; it seems only to serve to absorb phlogiston from the watery part of the mercurial nitre.

11. As this last process proved very tedious and complicated, on account of the necessity of ascertaining the quantity of acid in the receiving water, by means of an alkali which afforded a double source of error in the point of saturation, I resolved to try the distillation of dephlogisticated air from cubic nitre in a glass vessel, and to draw from it only such a quantity of air as it would yield without acting much on the retort, which latter circumstance is essentially necessary to be attended to. An ounce of the crystals of mineral alkali were dissolved in nitrous acid, and the mixture brought to an exact saturation by the test of litmus; 30 oz. measures of air were distilled from it, which, during the latter part of the process, was accompanied with slightly yellow fumes; the receiving water was found to be acid, and the residuum alkaline. The residuum being dissolved in the receiving water, the solution was neutral, or very nearly so, by every test; for in this case litmus might be used, as the acid was very slightly phlogisticated. On adding a few drops of a very dilute nitrous acid, the tests showed the liquor to be acid.

12. Encouraged by the success of this experiment, I took an ounce = 480 grs. of pure common nitre, and put it into a flint-glass retort, coated, which was placed in a furnace. It began to give air about the time it became red-hot;



and during the latter part of the process this air was accompanied with yellowish fumes. I stopped the process when it had produced 50 oz. measures of air. The receiving water, and particularly the air, had a strong but peculiar smell of nitrous acid. The air was well washed with the receiving water, but was not freed from the smell. The receiving water, which was 50 oz. was slightly acid, and the residuum alkaline. I dissolved the latter in the former, and found the mixture alkaline. 10 grs. of weak nitrous acid were added to it, which saturated it, and 105 grs. of this spirit of nitre was found to contain the acid of 60 grs. of nitre; therefore the 10 grs. contained the acid of 5.7 grs. of nitre, which, by Mr. Kirwan's experiments is equal to 2 grs. of real nitrous acid. We have therefore 34 grs. weight of dephlogisticated air produced, and only 2 grs. of real acid missing; and it is not certain that this quantity was destroyed, because some portion of the glass of the retort was dissolved by the nitre, and some parts of the materials employed in making the glass being alkali, we may conclude that the alkali of the nitre would be augmented by the alkali of that part of the glass it had dissolved. As the glass cracked into small pieces on cooling, and some part of the coating adhered firmly to it, the quantity of the glass that was dissolved could not be ascertained. From this experiment it appears, that if any of the acid of the nitre enters into the composition of the dephlogisticated air, it is a very small part; and it rather seems that the acid, or part of it, unites itself so firmly to the phlogiston as to lose its attraction for water.

13. "The vitriolic salts also yield dephlogisticated air by heat; and in these cases the dephlogisticated air is always attended with a large quantity of vitriolic acid air or sulphureous vapour," even when the salts used are not known to contain any phlogistic matter. Mr. Scheele mentions his having obtained dephlogisticated air from manganese dissolved in acid of phosphorus, and also from the arsenical acid: whence it appears that these acids, or perhaps any acid which can bear a red heat, can concur to the production of dephlogisticated air. It is necessary to remark, that no experiments have been yet published showing that dephlogisticated air can be produced from salts formed by the muriatic acid. The acids which produce salts suitable for this purpose have all a strong affinity with phlogiston; and the marine acid has either a very small affinity with it, or else is already saturated with it, at least so far saturated as not to be able to attract it from the humour.

14. "The dephlogisticated air obtained from the pure calces of metals may be attributed to the calces themselves, attracting the phlogiston from water which they have imbibed from the atmosphere, or from their dephlogisticating the fixed air which they are known to contain." It is very probable, that the dephlogisticated air extruded from growing vegetables may be owing to their dephlogisticating the water they grow in; but it appears most probable that the plants



have a power of dephlogisticating the fixed, or phlogisticated, air of the atmosphere.

“When dephlogisticated and nitrous air are mixed, the dephlogisticated air seizes part of the phlogiston of the nitrous air.” The water contained in the nitrous air, and the other part of the phlogiston, unite with the nitrous acid, which then assumes a liquid form, or at least that of a dense vapour; “and that part of the latent heat of the 2 airs not essential to the new combination is set at liberty.”\* In the combustion of sulphur the same thing happens, but in a greater degree; for the vitriolic acid, having a much weaker attraction for phlogiston than air has, abandons it almost entirely to the latter, which is thereby converted into water, and reduces it to a liquid state. The same reasoning may be applied to the combustion of phosphorus, which is attended with similar effects.

15. I shall not make, at present, any further deductions from what I myself consider still in the light of a conjectural hypothesis, which I have perhaps dwelt on too long already. I shall only beg your attention to some general reasoning on the subject; which however may possibly serve more to show the uncertainty of other systems on the constituent parts of air, than the certainty of this. Some of those systems suppose dephlogisticated air to be composed of an acid and something else, some say phlogiston. If an acid enters into the composition of it, why does not that acid appear again when the air is decomposed, by means of inflammable air and heat? And why is the water which is the product of this process pure water? And if an acid forms one of its constituent parts, why has nobody been able to detect any difference in the dephlogisticated air, made by the help of different acids, when compared with each other, or with the air extruded by vegetables? These airs, of such different origins, appear to be exactly the same. And if phlogiston be a constituent part of air, why does it attract phlogiston with such avidity? Some have, on the other hand, contended that air is composed of earth, united to acids or phlogiston, or to some other matter. Here we must ask, what earth it is which is one of the component parts of air? All earths which will unite with the nitrous or vitriolic acids, and with some others, such as the phosphoric and the arsenical acids, will serve as bases for the formation of air, and the air produced from all

\* I cannot take on me to determine, from any facts which have come to my knowledge, whether any part of the dephlogisticated air employed in this experiment is turned into fixed air; but I am rather inclined to think that some part is, because the quantity of heat, which is separated by the union of the two airs, does not seem to be so great as that which is separated when the dephlogisticated air is wholly changed into water: yet some water appears to be formed; because when the mixture is made over mercury, the solution of the mercury in the nitrous acid assumes a crystallized form, which however may be due to the watery part of the nitrous air.—Orig.



of them appears by every test to be the same, when freed from accidental impurities. To this argument it is answered, that it is not any particular species of earth which is the basis of air, but elementary or simple earth, which is contained in all of them. If this were the matter of fact, would not that earth be found after the decomposition of the air?

Mr. Scheele has formed an hypothesis on this subject, in which he supposes heat to be composed of dephlogisticated air united to phlogiston, and that this combination is sufficiently subtle to pass through glass vessels. He affirms, that the nitrous and other acids, when in an ignited state, attract the phlogiston from the heat, and set the dephlogisticated heat at liberty; but he does not seem to have been more successful than myself in explaining what becomes of the acid of nitre and phlogiston in the case of the decomposition of nitre by heat. And since we know, from the late experiments, that water is a composition of air, or more properly humour and phlogiston, his whole theory must fall to the ground, unless that fact be otherwise accounted for, which it does not seem easy to do.

16. To return to the experiment of the deflagration of dephlogisticated and inflammable air, it appears from the two airs becoming red-hot on their union, that the quantity of heat contained in one or both of them, is much greater than that contained in steam; because, for the first moments after the explosion, the water deposited by the air remains in the form of steam, and consequently retains the latent heat due to that modification of water. This matter may be easily examined by firing the mixture of dephlogisticated and inflammable air in a vessel immersed in another vessel containing a given quantity of water of a known heat, and after the vessel in which the deflagration is performed is come to the same temperature with the water in which it is immersed, by examining how much heat that water has gained, which being divided by the quantity of water produced by the decomposition of the airs, will give the whole quantity of elementary or latent heat which that water had contained, both as air and as steam; and if from that quantity we deduct the latent heat of the steam, the remainder will be the latent or elementary heat contained more in air than in steam. This experiment may be made more completely by means of the excellent apparatus which Messrs. Lavoisier and De la Place have contrived for similar purposes.

Till direct experiments are made, we may conclude, from those which have been made by the gentlemen just named, on the decompositions of air by burning phosphorus and charcoal, that the heat extricated during the combustion of inflammable and dephlogisticated air is much greater than it appears to be; for they found that one Paris oz. (= 576 Parisian grs.) of dephlogisticated air, when decomposed by burning phosphorus, melted 68.634 oz. of ice; and as, according to another of their experiments, ice, on being melted, absorbs  $135^{\circ}$  of heat by



Fahrenheit's scale, each ounce of air gave out  $68.634 \times 135^\circ = 9265^\circ.590$ ; that is to say, a quantity of heat which would have heated 1 oz. of water, or any other matter which has the same capacity for receiving heat as water has, from  $32^\circ$  to  $9263\frac{1}{2}^\circ$ : a surprizing quantity! (It is to be understood, that all the latent heats mentioned herein are compared with the capacity of water). And when 1 oz. of dephlogisticated air was changed into fixed air, by burning charcoal, or by the breathing of animals, it melted 29.547 oz. of ice: consequently we have  $29.547 \times 135^\circ = 3988^\circ.845$ , the quantity of heat which an ounce of dephlogisticated air loses when it is changed into fixed air. By the heat extricated during the detonation of 1 oz. of nitre with 1 oz. of sulphur, 32 oz. of ice were melted; and, by the experiment I have mentioned of Dr. Priestley's (6), it appears that nitre can produce one half of its weight of dephlogisticated air. When the nitre and sulphur are kindled, the dephlogisticated air of the nitre unites with the phlogiston of the sulphur, and sets its acid free, which immediately unites to the alkali of the nitre, and produces vitriolated tartar. The dephlogisticated air, united to the phlogiston, is turned into water, part of which is absorbed by the vitriolated tartar, and part is dissipated in the form of vapours, or unites to the nitrous air, or other air, produced in the process.

As  $\frac{1}{2}$  an oz. of dephlogisticated air is, in this process, united by inflammation to a quantity of phlogiston sufficient to saturate it, and no fixed air is produced, it should melt a quantity of ice equal to the half of what was melted by the combination of 1 oz. of air with phlogiston in burning phosphorus; that is, it should melt 34.317 oz. of ice; and we find, by Messrs. Lavoisier and De la Place's experiment, that it actually melted 32 oz. of ice: the small difference may be accounted for by supposing, that the heat produced by the combustion might not be quite so great as that Dr. Priestley employed in his experiment; or that the nitre might be less pure, and consequently not so much air formed. The two facts, however, agree near enough to permit us to conclude, that dephlogisticated air, in uniting to the phlogiston of sulphur, produces as much heat as it does in uniting with the phlogiston of phosphorus.

17. According to Dr. Priestley's experiments, dephlogisticated air unites completely with about twice its bulk of the inflammable air from metals. The inflammable air being supposed to be wholly phlogiston, and being  $\frac{1}{9.18}$  of the weight of an equal bulk of dephlogisticated air, and being double in the quantity, will be  $\frac{1}{4.59}$  of the weight of the dephlogisticated air it unites with. Therefore 1 oz. (576 grains) of dephlogisticated air will require 120 grs. of inflammable air, or phlogiston, to convert it into water. And supposing the heat extricated by the union of dephlogisticated and inflammable air to be equal to that extricated by the burning of phosphorus, we shall find, that the union of



120 grs. of inflammable air with 576 grs. of dephlogisticated air, extricates  $9265^{\circ}$  of heat.

18. In the experiment on the deflagration of nitre with charcoal, by Messrs. Lavoisier and De la Place, 1 oz. of nitre and  $\frac{1}{3}$  of 1 oz. of charcoal melted 12 oz. of ice. Supposing the ounce of nitre to have produced  $\frac{1}{4}$  an oz. of dephlogisticated air, it ought to have consumed 0.1507 oz. of ice; and I suppose it fell short of its effect by the heat not being sufficiently intense to decompose the nitre perfectly.

19. By the above gentlemen's experiment 1 oz. of charcoal required for its combustion 3.3167 oz. of dephlogisticated air, and produced 3.6715 oz. of fixed air; therefore there was united to each oz. of air, when changed into fixed air, 61.5 grs. of phlogiston, and  $3988^{\circ}$  of heat were extracted. It appears by these facts, that the union of phlogiston, in different proportions, with dephlogisticated air, does not extricate proportional quantities of heat. For the addition of 61.5 grs. produces  $3988^{\circ}$ , and the union of 120 grs. produces  $9265^{\circ}$ . This difference may arise from a mistake in supposing the specific gravity of the inflammable air Dr. Priestley employed to have been only  $\frac{1}{9\frac{1}{8}}$  of that of dephlogisticated air; for if it be supposed that its specific gravity was a little more than  $\frac{1}{9}$  of that of the dephlogisticated air, then equal additions of phlogiston would have produced equal quantities of heat: \* this matter should therefore be put to the test of experiment, by deflagrating dephlogisticated air with inflammable air of a known specific gravity, or by finding how much dephlogisticated air is necessary for the combustion of 1 oz. of sulphur, the quantity of phlogiston in which has been accurately determined by Mr. Kirwan; or by finding the quantity of phlogiston in phosphorus, the quantity of dephlogisticated air necessary for its decomposition being known from Messrs. Lavoisier and De la Place's experiments.

On considering these latter gentlemen's experiments on the combustion of charcoal, a difficulty arises, to know what became of the remainder of the oz. of charcoal; for the dephlogisticated air, in becoming fixed air, gained only the weight of 0.3548, or about  $\frac{1}{3}$  of 1 oz.; about  $\frac{2}{3}$  of 1 oz. are therefore unaccounted for. The weight of the ashes of 1 oz. of charcoal is very inconsiderable; and, by some experiments of Dr. Priestley's, charcoal, when freed from fixed air, and other air which it imbibes from the atmosphere, is almost wholly convertible into phlogiston. The cause of this apparent loss of matter, I doubt

\* Or it may arise from my being mistaken, in supposing that the same quantity of heat is disengaged by the union of dephlogisticated air with phlogiston, in the form of inflammable air, as is by its union with the phlogiston of phosphorus or sulphur; and there appears to be some reason why there should not; because in these latter cases the water, being united to the acids, cannot retain so much elementary heat as it can do when left in the form of pure water, which is the case when the inflammable air is used.—Orig.



not, these gentlemen can explain satisfactorily, and very probably in such a manner as will throw other lights on the subject.

It is also worthy of inquiry, whether all the amazing quantity of heat let loose in these experiments was contained in the dephlogisticated air; or whether the greatest portion of it was not contained in the phlogiston or inflammable air. If it was all contained in the dephlogisticated air, the general rule is not fact, that elastic fluids are enlarged in their dimensions in proportion to the quantity of heat they contain, because then, inflammable air, which is 10 times the bulk of dephlogisticated air, must be supposed to contain no heat at all; and it is known, from some experiments of my friend Dr. Black's, and some of my own, that the steam of boiling water, whose latent and sensible heat are only  $1100^{\circ}$ , reckoning from  $60^{\circ}$ , or temperate, is more than twice the bulk of an equal weight of dephlogisticated air. It seems however reasonable to suppose that the great quantity of heat should be contained in the rarer fluid.

It may be alleged, that in proportion to the quantity of phlogiston that is contained in any fluid, the quantity of heat is lessened. But if we reason by analogy, the attraction of the particles of matter to each other in other cases is increased by phlogiston, and bodies are thereby rendered specifically heavier; and we know of no other substance besides heat which can be supposed to separate the particles of inflammable air, and to endow it with so very great an elastic power, and so small a specific gravity. On the other hand, if a great quantity of elementary heat be allowed to be contained in inflammable air, on account of its bulk, the same reasoning cannot hold good in respect to the phlogiston of phosphorus, sulphur, charcoal, &c. But all these substances contain other matter besides phlogiston and heat. The acids in the sulphur and phosphorus, and the alkali and earth in charcoal, may attract the phlogiston so powerfully that the heat they contain may not be able to overcome the adhesion of their particles, till, by the effect of external heat, they are once removed to such a distance from each other, as to be out of the sphere of that kind of attraction.\* If it be found to be a constant fact, that equal additions of phlogiston to dephlogisticated air do not extricate equal quantities of heat, that may afford the means of finding the quantities of heat contained in phlogiston and dephlogisticated air respectively, and solve the problem.

Many other ideas on these subjects present themselves; but I am not bold enough to trouble you, or the public, with any speculations, but such as I think are supported by uncontroverted facts. I must therefore bring this long letter to a conclusion, and leave to others the future prosecution of a subject which, however engaging, my necessary avocations prevent me from pursuing. I cannot

\* On the whole, this question seems to involve so many difficulties, that it cannot be cleared up without many new experiments.—Orig.



however conclude, without acknowledging my obligations to Dr. Priestley, who has given me every information and assistance in his power, in the course of my inquiries, with that candour and liberality of sentiment which distinguish his character.

*XXVI. Sequel to the Thoughts on the Constituent Parts of Water and Dephlogisticated Air. In a subsequent Letter from Mr. Watt to Mr. De Luc, F. R. S. p. 354.*

On re-considering the subject of my letter of the 26th of Nov. last, I think it necessary to resume the subject, in order to mention some necessary cautions to those who may chuse to repeat the experiments mentioned there, and to point out some circumstances that may cause variations in the results. In experiments where the dephlogisticated air is to be distilled from common or cubic nitre, these salts should be purified as perfectly as possible, both from other salts and from phlogistic matter of any kind; otherwise they will produce some nitrous air, or yellow fumes, which will lessen the quantity, and perhaps debase the quality of the dephlogisticated air. If the nitre is perfectly pure, no yellow fumes are perceptible, until the alkaline part begins to act on the glass of the retort, and even then they are very slightly yellow. When earthen retorts are used, and a large quantity of air is drawn from the nitre, it acts very much on the retort, dissolves a great part of it, and becomes very alkaline, retaining only a small part of its acid, at least only a small part which can be made appear in any of the known forms of that acid; and unless retorts can be obtained of a true apyrous and compact porcelain, I should prefer glass retorts, properly coated for making experiments for the present purpose.

In some of my experiments the nitre was left in the retort placed in a furnace, so that it took an hour or more to cool. In these cases there was always a deficiency of the acid part, which seemed, from some appearances on the coating, either to have penetrated the hot and soft glass, by passing from particle to particle, or to have escaped by small cracks which happened in the retort during the cooling. There was the least deficiency of the acid when the distillation was performed as quickly as was practicable, and the retort was removed from the fire immediately after the operation was finished. In order to shorten the duration of the experiment, and consequently to lessen the action of the nitre on the retort, it is advisable not to distil above 50 oz. measures of dephlogisticated air from an ounce of nitre. The experiment has succeeded best when the retort was placed in a charcoal fire in a chafing-dish or open furnace; because it is easy in that case to stop the operation, and to withdraw the retort at the proper period.

When the dephlogisticated air is distilled from the nitre of mercury, the solution should be performed in the retort itself, and the nitrous air produced by the



solution should be caught in a proper receiver, and decomposed by the gradual admission of common air through water; and the water, which thus becomes impregnated with the acid of the nitrous air, should be added after the process to the water through which the dephlogisticated air has passed. When the solution ceases to give any more nitrous air, the point of the tube of the retort should be raised out of the water; otherwise, by the condensation of the watery and acid vapours which follow, a partial exhaustion will take place, and the receiving water will rise up into the retort and break it, or at least spoil the experiment. A common receiver, such as is used in distilling spirit of nitre, should be applied, with a little water in it, to receive the acid steam; and it should be kept as cool as can conveniently be done, as these fumes are very volatile. This receiver should remain as long as the fumes are colourless; but when they appear, in the neck of the retort, of a yellow colour, it is a mark that the mercurial nitre will immediately produce dephlogisticated air; the receiver should then be withdrawn, and an apparatus placed to receive the air. The rest of the process has been sufficiently explained in my former letter.

The phlogisticated nitrous acid, saturated by an alkali, will not crystallize; and if exposed to evaporation, even in the heat of the air, will become alkaline again; which shows the weakness of its affinity with alkalis when dissolved in water;\* a further proof of which is, that it is expelled from them by all the acids, even by vinegar (which fact has been observed by Mr. Scheele.) I have observed that litmus is no test of the saturation of this acid by alkalis; for the infusion of litmus added to such a mixture will turn red, when the liquor appears to be highly alkaline, by its turning the infusions of violets, rose leaves, and most other red juices, green. This does not proceed from the infusion of litmus being more sensible to the presence of acids than other tests; for I have lately discovered a test liquor (the preparation of which I mean to publish soon) which is more sensible to the presence of acids than litmus is; but which turns green in the same solution of phlogisticated nitre that turns litmus red.

The unavoidable little accidents which have attended these experiments, and which tend to render their results dubious, have prevented me from relying on them as full proofs of the position that no acid enters into the composition of dephlogisticated air; though they give great probability to the supposition. I have therefore explained the whole of the hypothesis and experiments with the diffidence which ought to accompany every attempt to account for the phenomena of nature on other principles than those which are commonly received by philosophers in general. And in pursuance of the same motives it is proper to men-

\* I have been informed; that Mr. Cavendish has also observed this fact; and that he has mentioned it in a paper lately read before the R. S.; but I had observed the fact previous to my knowledge of his paper.—Orig.



tion, that the alkali employed to saturate the phlogisticated nitrous acid, was always that of tartar which is partly mild; and I have not examined whether highly phlogisticated nitrous acid can perfectly expel fixed air from an alkali, though I know no fact which proves the contrary. It should also be examined, whether the same quantity of real nitrous acid is requisite to saturate a given quantity of alkali, when the acid is phlogisticated, as is necessary when it is dephlogisticated.

*XXVII. An Attempt to Compare and Connect the Thermometer for Strong Fire, described in Vol. 72 of the Philos. Trans. with the Common Mercurial Ones. By Mr. J. Wedgwood, F. R. S. p. 358.*

This thermometer has now been found, from extensive experience, both in my manufactories and experimental inquiries, to answer the expectations I had conceived of it as a measure of all degrees of common fire above ignition: but at present it stands in a detached state, not connected with any other, as it does not begin to take place till the heat is too great to be measured or supported by mercurial ones. What is now therefore wanting, to give us clear ideas of the value of its degrees, is, to connect it with one which long use has rendered familiar to us; so that if the scale of the common thermometer be continued indefinitely upwards as a standard, the divisions of mine may be reduced to that scale, and we may thus have the whole range of the degrees of heat brought into one uniform series, expressed in one language, and comparable in every part, from the lowest that have hitherto been produced by any artificial freezing mixtures, up to the highest that can be obtained in our furnaces, or that the materials of our furnaces and vessels can support.

The hope of attaining this desirable and important object gave rise to the present experiments. This attempt is founded on the construction and application of an intermediate measure, which takes in both the heats that are measurable by the mercurial thermometer, and a sufficient number of those that come within the province of mine to connect the two together; the manner of doing which will be apparent from the first 3 figures 11, 12, 13, pl. 7; where F represents Fahrenheit's thermometer, with a continuation of the scale; w my thermometer; and m the intermediate measure divided into any number of equal parts at pleasure.

For if the heat of boiling water, or 212 degrees of Fahrenheit, be communicated to m, and its measure on m marked as at a; and if the heat of boiling mercury, or 600° of Fahrenheit, be also communicated to m, and marked as at b; it is plain, that the number of degrees on m between a and b will be equal to the interval between 212 and 600, that is, to 388° on Fahrenheit. In like manner, by exposing m to 2 different heats above ignition along with my ther-



mometer-pieces, if a certain degree of my scale be found to correspond with the point d, and another degree of mine with the point c; then the interval between those 2 degrees on mine must be equal to the interval dc; and how many of Fahrenheit's that interval is equivalent to will be known from the preceding comparison. Thus we can find the number of Fahrenheit's degrees contained in any given extent of mine, and the degree of Fahrenheit's with which a given point of mine coincides; whence either scale is easily reducible to the other through their whole range, whether we suppose Fahrenheit's continued upwards, or mine downwards.

For obtaining the intermediate thermometer different means were thought of; but the only principle which, on attentive consideration, afforded any prospect of success, was the expansion of metals. This therefore was adopted, and among different methods of measuring that expansion, which either occurred to myself, or which I can find to have been practised by others, there is no one which promises either so great accuracy, or convenience in use, as a gage like that by which the thermometer-pieces are measured: the utility of this gage had now been confirmed by experience, and the machines and long rods, which have been employed for measuring expansions on other occasions, were absolutely inadmissible here, on account of the insuperable-difficulties of performing nice operations of this kind in a red heat, and of communicating a perfectly equal heat through any considerable extent.

To give a clearer idea of this species of gage, which, simple as it is, I am informed has been misunderstood by some of the readers of my former paper, a representation of one used on the present occasion is annexed in fig. 14, where ABCD is a smooth flat plate; and EF and GH two rulers or flat pieces, a quarter of an inch thick, fixed flat upon the plate, with the sides that are towards each other made perfectly true, a little farther asunder at one end EG than at the other end FH; thus they include between them a long converging canal, which is divided on one side into a number of small equal parts, and which may be considered as performing the offices both of the tube and scale of the common thermometer. It is obvious, that if a body, so adjusted as to fit exactly at the wider end of this canal, be afterwards diminished in its bulk by fire, as the thermometer-pieces are, it will then pass farther in the canal, and more and more so according as the diminution is greater; and conversely, that if a body, so adjusted as to pass on to the narrow end, be afterwards expanded by fire, as is the case with metals, and applied in that expanded state to the scale, it will not pass so far; and that the divisions on the side will be the measures of the expansions of the one, as of the contractions of the other, reckoning in both cases from that point to which the body was adjusted at first. I is the body whose alteration of bulk is thus to be measured, which, in the present instance,



is a piece of fine silver : this is to be gently pushed or slid along, towards the end FH, till it is stopped by the converging sides of the canal. K is a little vessel formed in the gage for this particular series of experiments, the use of which will appear hereafter.

The contraction, which the thermometer pieces receive from fire, is a permanent effect, not variable by an abatement of the heat, and which accordingly is measured commodiously and at leisure, when the pieces are become cold. But the expansion of bodies is only temporary, continuing no longer than the heat does that produced it; and therefore its quantity, at any particular degree of heat, must be measured in the moment while that heat subsists. And further, if the heated piece was applied to the cold gage, the piece would be deprived of a part of its heat on the first contact; and as the gage receives some degree of expansion from heat as well as the piece, it is plain that in this case the piece would be diminished in its bulk, and the gage enlarged, before the measurement could be taken. It is therefore necessary that both of them be heated to an exact equality; and in that state we can measure, not indeed the true expansion of either, but the excess of the expansion of one above that of the other; which is sufficient for the present purpose, as we want only a uniform and graduated effect of fire, and it is totally immaterial whether that effect be the absolute expansion of one or the other body, or the difference of the two, provided only that its quantity be sufficient to admit of nice measurement.

Some difficulties occurred with respect to the choice of a proper matter for the gage; the essential requisites of which are, to have but little expansibility, and to bear the necessary fires without injury. All the metals, except gold and silver, would calcine in the fire: those two are indeed free from that objection, and accordingly it is of the most expansible of them that the piece is made; but if the gage also was made of the same, the measure itself would expand just as much as the body to be measured, and no expansion at all would be sensible; and though the gage was made of one of those metals, and the piece of the other, the difference between their expansions would be too small to give any satisfactory results, as more than  $\frac{2}{3}$  of the real expansion of either would be lost or taken off by the other.

For these reasons I had recourse to earthy compositions, which expand by heat much less than metallic bodies, and bear the necessary degrees of fire without the least injury. I made choice of tobacco-pipe clay, mixed with charcoal in fine powder, in the proportion of 3 parts of the charcoal to 5 of the clay by weight. By a free access of air, in the burning by which the gage is prepared for use, the charcoal is consumed, and leaves the clay extremely light and porous; from which circumstance it bears sudden alternations of cold and heat, often requisite in these operations, much better than the clay alone. Another



and more important motive for the use of charcoal was, that in consequence of the remarkable porosity which it produces in the clay, it would probably diminish the expansibility, by occasioning the mass to contain, under an equal surface a much less quantity of solid or expansible matter. It may be objected to this idea, that the expansions of metals, in Mr. Ellicott's\* and Mr. Smeaton's† experiments, do not appear to have any connection at all with their densities: but the cases are by no means parallel; for there the comparison lies between different species of matter; but here, between one and the same matter in different states of compactness. If a metal could be treated as clay is in this instance, that is, if a large bulk of any foreign matter could be blended with it, and this matter afterwards burnt out, so as to leave the metallic particles at the same distances to which they had been separated by the mixture of it, we may presume that the metal thus enlarged would not expand so much as an equal volume of the solid metal. Such at least were the ideas which determined my choice to a composition of clay and charcoal powder; and being afterwards desirous of satisfying myself whether they had any foundation in fact, I have, since the experiments were made, prepared some pieces of clay with and without charcoal, and having burnt them in the same fire, I ground them at the sides, to make them both fit exactly to the same division near the narrow end of the gage; then, examining their expansions by equal heats, I found the piece with charcoal to expand only  $\frac{1}{3}$  part so much as that without; and thus was fully satisfied with the composition of the gage.

To ascertain a fixed point on the scale for the divisions to be counted from, the silver piece and gage were laid together for some time in spring water, of the temperature of  $50^{\circ}$  of Fahrenheit: the point which the piece went to in this cold state is that marked O near the narrow end of the gage. The adjustment is re-examined at the beginning and end of every succeeding experiment, lest the repeated attrition, in sliding the pieces backwards and forwards, should wear off so much from the surface of this soft metal as to occasion an error in the minute quantities here measured. The apparatus is then exposed successively to different degrees of heat, with the piece lying always in a part of the canal at least as wide as it is expected to fill when expanded, otherwise the sides of the gage would be burst asunder by its expansion, as I experienced in some of my first trials. When the whole has received any particular degree of heat desired, the piece is cautiously and equably pushed along, till it is stopped by the convergency of the sides, of which I always find notice given by the gage itself (which is small and light) beginning to move on the continuance of the impulse. A flat slip of iron, a little narrower than the piece, bent down to a right angle at

\* Phil. Trans. vol. 47, p. 485.

† Ibid. vol. 48, p. 612.



one end, and fixed in a long handle at the other, makes a convenient instrument for pushing the piece forward, or drawing it back again, while red-hot: this instrument, at every time of using, is heated to the same degree as the piece itself.

The heat of boiling water is taken without difficulty, by keeping the apparatus in boiling water itself during a sufficient time for the full heat to be communicated to it. The water I made use of was a very fine spring water, which on chemical trials appeared very nearly equal in purity to that of rain or snow; and I had previously satisfied myself, by trials in the cold, that the gage and piece being wet, or under water, made no difference in the measurement. The expansion of the silver by this heat, that is, by an increase of the heat from  $50^{\circ}$  to  $212^{\circ}$ , or a period containing  $162^{\circ}$  of Fahrenheit, was just  $8^{\circ}$  of the gage or intermediate thermometer M; whence 1 of these degrees, according to this experiment, contains just  $20^{\circ}\frac{1}{4}$  of Fahrenheit's. The operation was many times repeated, and the result was always precisely the same.

For the boiling heat of mercury, it was necessary to proceed in a different manner; not to convey the heat from the mercury to the instrument, but to convey it equally to them both from another body. I made a small vessel for holding the mercury in the gage itself, seen at 1 fig. 14, and more distinctly in fig. 15, which is a transverse section of the gage through this vessel. The plate CD, which forms the bottom of the canal, serves also for the bottom of the vessel, which is situated close to the side of the canal, and as near as could be to that part of it, in which both the silver piece, and the divisions required for this particular experiment, are contained. By this arrangement it is presumed, that all the parts concerned in the operation will receive very nearly an equal heat. The gage, with some mercury in the vessel, was laid on a smooth and level bed of sand, on the bottom of an iron muffle kept open at one end; the fire increased very gradually till the mercury boiled, and then continued steady, so as just to keep it boiling, for a considerable time. The boiling heat of mercury was thus found to be  $27^{\circ}\frac{1}{4}$  of the intermediate thermometer, which answering to an interval of  $550^{\circ}$  of Fahrenheit, makes 1 degree of this equal to just  $20^{\circ}$  of his; a result corresponding even beyond my expectations with that which boiling water had given.

These standard heats of Fahrenheit's thermometer are obtained with little difficulty on a common fire; but it is far otherwise with the higher ones in which mine begins to apply; and all the precautions I could take, by using a close muffle, surrounding it as equally as possible with the fuel, varying its position with respect to the draught of air, &c. proved insufficient for securing the necessary equality of heat even through the small space concerned in these experiments. Nor had I any idea, before the discovery of this thermometer, of



the extreme difficulty, not to say impracticability, of obtaining, in common fires, or in common furnaces, a uniform heat through the extent even of a few inches. Incredible as this may appear at first sight, whoever will follow me in the operations I have gone through, placing accurate measures of the heat in different parts of one and the same vessel, will soon be convinced of its truth, and that he can no otherwise expect to communicate with certainty an equal heat to different pieces, than by using a fire of such magnitude as to exceed perhaps some hundreds of times the bulk of the matters required to be heated. To such large body of fire, therefore, after many fruitless attempts in small furnaces, not a little discouraging by the irregularity of their results, I at length had recourse, fitting up for this purpose an iron oven, used for the burning-on of enamel colours on earthen-ware, about 4 feet long, by  $2\frac{1}{2}$  wide, and 3 feet high, which is heated by the flame of wood conducted all round it. An iron muffle, 4 inches wide,  $2\frac{3}{4}$  inches high, and 10 inches long, containing the gage and piece, was placed in the middle of this oven, and the vacancy between them filled up with earthen-ware, to increase the quantity of ignited matter, and thereby communicate the heat more equably from the oven to the muffle. In such a situation of the muffle, in the centre of an oven more than 500 times its own capacity, it could not well fail of being heated pretty uniformly, at least through the small space which these experiments required; nor have I found any reason to suspect that it was not so.

The gage being laid flat on the bottom of the muffle, with the silver piece in the canal as before, some of the clay thermometer pieces were set on end on the silver piece, with that end of each downwards which is marked to go foremost in measuring it; that is, they were in contact with the silver in that part of their surface by which their measure is afterwards ascertained. I was led to this precaution by an experiment I had made on another occasion, in which a number of thermometer pieces having been set upright on an earthen-ware plate, over a small fire, till the plate became red-hot, all the pieces were found diminished, some of them more than 2 degrees, at the lower ends which rested on the plate, while the upper ends were as much enlarged, not having yet passed the stage of extension which, as observed in the former paper, always precedes the thermometric diminution: thus we see how punctually every part of the piece obeys the heat that acts on it.

The fire about the oven was slowly increased for some hours, and kept as even and steady as possible, by an experienced fireman, under my own inspection. On opening a small door, which had been made for introducing the apparatus, and looking in from time to time, it was observed, that the muffle, with the adjacent parts of the oven and ware, acquired a visible redness at the same time; and in the progress of the operation, the eye could not distinguish the least dis-



similarity in the aspect of the different parts; whereas in small fires, the difference not only between the two ends of the muffle, but in much less distances, is such as to strike the eye at once. When the muffle appeared of a low red heat, such as was judged to come fully within the province of my thermometer, it was drawn forward, towards the door of the oven; and its own door being then nimbly opened by an assistant, I immediately pushed the silver piece as far as it would go. But as the division which it went to could not be distinguished in that ignited state, the muffle was lifted out, by means of an iron rod passed through two rings made for that purpose, with care to keep it steady, and avoid any shake that might endanger the displacing of the silver piece.

When sufficiently cooled to be examined, I noted the degree of expansion which the silver piece stood at, and the degree of heat shown by the thermometer pieces measured in their own gage; then returned the whole into the oven as before, and repeated the operation with a stronger heat, to obtain another point of correspondence on the two scales. The first was at  $2\frac{1}{4}^{\circ}$  of my thermometer, which coincided with  $66^{\circ}$  of the intermediate one; and as each of these last has been before found to contain 20 of Fahrenheit's, the 66 will contain 1320; to which add 50, the degree of his scale to which the 0 of the intermediate thermometer was adjusted, and the sum, 1370, will be the degree of Fahrenheit's corresponding to my  $2\frac{1}{4}^{\circ}$ .

The 2d point of coincidence was at  $6\frac{1}{4}^{\circ}$  of mine, and  $92^{\circ}$  of the intermediate; which 92 being, according to the above proportion, equivalent to 1840 of Fahrenheit, add 50 as before to this number, and my  $6\frac{1}{4}^{\circ}$  is found to fall on the 1890th degree of Fahrenheit. It hence appears, that an interval of  $4^{\circ}$  on mine is equivalent to an interval of  $520^{\circ}$  on his; consequently 1 of mine to  $130^{\circ}$  of his; and that the 0 of mine corresponds to his  $1077\frac{1}{2}^{\circ}$ . Several other trials were made, which gave results so nearly alike, that I have little apprehension of any material error.

From these data it is easy to reduce either scale to the other through their whole range; and from such reduction it will appear, that an interval of near  $480^{\circ}$  remains between them, which the intermediate thermometer serves as a measure for; that mine includes an extent of about 32000 of Fahrenheit's degrees, or about 54 times as much as that between the freezing and boiling points of mercury, by which mercurial ones are naturally limited; that if the scale of mine be produced downwards, in the same manner as we have supposed Fahrenheit's to be produced upwards, for an ideal standard, the freezing point of water would fall nearly on  $8^{\circ}$  below 0 of mine, and the freezing point of mercury a little below  $8\frac{1}{3}^{\circ}$ ; and that therefore, of the extent of now measureable heat, there are about  $\frac{5}{10}$  of a degree of my scale from the freezing of mercury to the



freezing of water;  $8^{\circ}$  from the freezing of water to full ignition; and  $160^{\circ}$  above this to the highest degree I have hitherto attained.

As we are now enabled to compare not only the higher degrees among themselves, and the lower among themselves, on their respective scales, but likewise the higher and lower with each other in every stage, it may be proper to take a general view of the whole range of measurable heat, as expressed both in Fahrenheit's denominations and in mine; and for this purpose I have drawn up the following little table of a few of the principal points that have been ascertained, to show their mutual relations or proportions to each other; any other points that have been, or hereafter may be observed, by these or any other known thermometers, may be inserted at pleasure.

	Fahr.	Wedg.		Fahr.	Wedg.
Extremity of the scale of my thermometer .....	32277 $^{\circ}$	240 $^{\circ}$	Heat by which my enamel colours are burnt on .....	1857 $^{\circ}$	6 $^{\circ}$
Greatest heat of my small air-furnace .....	21877	160	Red-heat fully visible in day-light	1077	0
Cast iron melts .....	17977	130	Red-heat fully visible in the dark	947	1
Greatest heat of a common smith's forge .....	17327	125	Mercury boils .....	600	3 $\frac{673}{1000}$
Welding heat of iron, greatest..	13427	95	Water boils .....	212	6 $\frac{058}{1000}$
least .....	12777	90	Vital heat .....	97	7 $\frac{542}{1000}$
Fine gold melts .....	5237	32	Water freezes .....	32	8 $\frac{42}{1000}$
Fine silver melts .....	4717	28	Proof spirit freezes .....	0	8 $\frac{289}{1000}$
Swedish copper melts .....	4587	27	The point at which mercury congeals, consequently the limit of mercurial thermometers.	about 40	8 $\frac{526}{1000}$
Brass melts .....	3807	21			

To assist our conceptions of this subject, it may be proper to view it in another light, and endeavour to present it to the eye; for numbers, on a high scale, are with difficulty estimated and compared by the mind. I have therefore completed the scales of which a part is represented in fig. 41 and 13, by continuing the same equal divisions, both upwards and downwards, as far as the utmost limits of heat that have hitherto been attained and measured. In a scale of heat drawn up in this manner, the comparative extents of the different departments of this grand and universal agent are rendered conspicuous at a single glance of the eye. We see at once, for instance, how small a portion of it is concerned in animal and vegetable life, and in the ordinary operations of nature. From freezing to vital heat is barely a 500th part of the scale; a quantity so inconsiderable, relatively to the whole, that in the higher stages of ignition, 10 times as much might be added or taken away, without the least difference being discernible in any of the appearances from which the intensity of fire has hitherto been judged of. Hence, at the same time, we may be convinced of the utility and importance of a physical measure for these higher degrees of heat, and the utter insufficiency of the common means of discriminating and estimating their force. I have too often found differences, astonishing when considered as a part of this scale, in



the heats of my own kilns and ovens, without being perceivable by the workmen at the time, or till the ware was taken out of the kiln.

P. S. Since the foregoing experiments were made, I have seen a curious Memoir by Messrs. Lavoisier and De la Place, containing a method of measuring heat by the quantity of ice which the heated body is capable of liquefying. The application of this important discovery, as an intermediate standard measure between Fahrenheit's thermometer and mine, could not escape me, and I immediately set about preparing an apparatus, and making the experiments necessary for that purpose; in hopes either of attaining by this method a greater degree of accuracy than I could expect from any other means, or of having what I had already done confirmed by a series of experiments on a different principle. But in the prosecution of these experiments I have, to my great mortification, hitherto failed of success; and I should have contented myself for the present with saying little more than this, if some phenomena had not occurred, which appear not unworthy of further investigation.

The authors observe, that if ice, cooled to whatever degree below the freezing point, be exposed to a warmer atmosphere, it will be brought up to the freezing point through its whole mass before any part of its surface begins to liquefy; and that consequently ice, beginning to melt on the surface, will be always exactly of the same temperature, viz. at the freezing point; and that if a heated body be inclosed in a hollow sphere of such ice, the whole of its heat will be taken up in liquefying the ice; so that if the ice be defended from external warmth, by surrounding it with other ice in a separate vessel, the weight of the water produced from it will be exactly proportional to the heat which the heated body has lost; or, in other words, will be a true physical measure of the heat.

For applying these principles in practice, they employ a tin vessel, divided, by upright concentric partitions, into 3 compartments, one within another. The innermost compartment is a wire cage, for receiving the heated body. The 2d, surrounding this cage, is filled with pounded ice, to be melted by the heat; and the outermost is filled also with pounded ice, to defend the former from the warmth of the atmosphere. The first of these ice compartments terminates at bottom in a stem like a funnel, through which the water is conveyed off; and the other ice compartment terminates in a separate canal, for discharging the water into which that ice is reduced. As soon as the heated body is dropped into the cage, a cover is put on, which goes over both that and the first ice compartment; which cover is itself a kind of shallow vessel, filled with pounded ice, with holes in the bottom for permitting the water from this ice to pass into the 2d compartment, all the liquefaction that happens here, as well as there, being the effect of the heated body only. Over the whole is placed another cover with pounded ice, as a defence from external warmth.



As soon as this discovery came to my knowledge, on the 23d of February, a thaw having begun 3 days before, after a frost which had continued with very little intermission from the 24th of December, I collected a quantity of ice, and stored it up in a large cask in a cellar. I thought it necessary to satisfy myself in the first place, by actual experiment, that ice, how cold soever it may be, comes up to the freezing point through its whole mass before it begins to liquefy on the surface. For this purpose I cooled a large fragment of ice, by a freezing mixture, to  $17^{\circ}$  of Fahrenheit's thermometer, and then hung it up in a room whose temperature was  $50^{\circ}$ . When it began to drop, it was broken, and some of the internal part quickly pounded and applied to the bulb of a thermometer that was cooled by a freezing mixture below  $30^{\circ}$ . The thermometer rose to, and continued at  $32^{\circ}$ ; being then taken out, and raised by warmth to  $40^{\circ}$ , some more of the same ice, applied as before to the bulb, sunk it again to  $32^{\circ}$ ; so that no doubt could remain on this subject.

Apprehensive that pounded ice, directed by the authors, might imbibe and retain more or less of the water by capillary attraction, according to circumstances, and thus occasion some error in the results, I thought it necessary to satisfy myself in this respect also by experiment. I therefore pounded some ice, and laid it in a conical heap on a plate; and having at hand some water, coloured with cochineal, I poured it gently into the plate, at some distance from the heap; as soon as it came in contact with the ice, it rose hastily up to the top; and on lifting up the lump, I found that it held the water, so taken up, as a sponge does, and did not drop any part of it till the heat of my hand, as I suppose, began to liquefy the mass. On further trials I found, that in pounded ice pressed into a conical heap, the coloured water rose, in the space of 3 minutes to the height of  $2\frac{1}{2}$  inches; and by weighing the water employed, and what remained on the plate unabsorbed, it appeared, that 4 ounces of ice had thus taken up, and retained, 1 ounce of water.

To further ascertain this absorbing power, in different circumstances, more analogous to those of the process itself, I pressed 6 oz. of pounded ice pretty hard into the funnel, having first introduced a wooden core in order to leave a proper cavity in the middle: then, taking out the core, and pouring 1 oz. of water on the ice, I left the whole for half an hour; at the end of which time the quantity that ran off was only 12 pennyweights and 4 grains, so that the ice had retained 7 pennyweights and 20 grains, which is nearly  $\frac{1}{12}$  of its own weight, and  $\frac{2}{3}$  of the weight of the water.

These previous trials determined me, instead of using pounded ice, to fill a proper vessel with a solid mass of ice, by means of a freezing mixture, as the frost was now gone, and then expose it to the atmosphere till the surface began to liquefy. The apparatus I fitted up for this purpose was made of earthen-ware



well glazed, and is represented in fig. 1, pl. 8. A, is a large funnel, filled with a solid mass of ice. B, a cavity in the middle of this ice, formed, part of the way, by scraping with a knife, and for the remaining part, by boring with a hot iron wire. C, one of my thermometer pieces, which serves for the heated body, and rests on a coil of brass wire: it had previously been burnt with strong fire, that there might be no danger of its suffering any further diminution of its bulk by being heated again for these experiments. D, a cork stopper in the orifice of the funnel. E, the exterior vessel, having the space between its sides and the included funnel A, filled with pounded ice, as a defence to the ice in the funnel. F, a cover for this exterior vessel, filled with pounded ice for the same purpose. G, a cover for the funnel, filled also with pounded ice, with perforations in the bottom for allowing the water from this ice to pass down into the funnel.

The thermometer piece was heated in boiling water, taken up with a pair of small tongs equally heated, dropped instantly into the cavity B, and the covers put on as expeditiously as possible; the bottom of the funnel being previously corked, that the water might be detained till it should part with all its heat, and to prevent the water from the other ice, which ran down on the outside of the funnel, from mingling with it. After standing about 10 minutes, the funnel was taken out, wiped dry, and uncorked over a weighed cup: the water that ran out weighed 22 grains. Thinking this quantity too small, as the piece weighed 72 grains, I repeated the experiment, and kept the piece longer in the funnel; but the water this time weighed only 12 grains. Being much dissatisfied with this result, I made a 3d trial; continuing the piece much longer in the cavity; but the quantity of water was now still less, not amounting to quite 3 drops; and to my great surprize I found the piece frozen to the ice, so as not to be easily got off, though all the ice employed was, at the beginning of the experiment, in a thawing state.

I had prepared the apparatus for taking the boiling heat of mercury; but being entirely discouraged by these very unequal results, I gave that up, for the present at least, and heating the piece to  $6^{\circ}$  of my thermometer, turned it quickly out of the case in which it was heated into the cavity, throwing some fragments of ice over it. In about half an hour, I drew off the water, which amounted to 11 pennyweights; then stopping the funnel again, and replacing the covers, I left the whole about 7 hours. At the end of that time, I found a considerable quantity of water in the funnel: the melting of the ice had produced a cavity between it and the sides, great part of the way down, which, as well as that in the middle, was nearly full. Yet the water ran out so slowly, that I apprehended something had stopped the narrow end of the funnel, but the true cause became afterwards apparent on examining the state of the ice. The fragments which I had thrown over the thermometer piece were frozen entirely together, and in



such a form as they could not have assumed without fresh water superadded and frozen on them, for the cavities between them were partly filled with new ice. I endeavoured to take the ice out with my fingers, but in vain; and it was with some difficulty I could force it asunder even with a pointed knife, to get at the thermometer piece. When that was got out, great part of the coiled wire was found enveloped in new ice. The passage through the ice to the stem of the funnel, which I had made pretty wide with a thick iron wire red-hot, was so nearly closed up, that the slow draining off of the water was now sufficiently accounted for, and indeed this draining was the only apparent mark of any passage at all. On taking the ice out of the funnel, and breaking it to examine this canal, I found it almost entirely filled up with ice projecting from the solid mass in crystalline forms, similar in appearance to the crystals we often meet with in the cavities of flints and quarzose stones.

If, after all these circumstances, any doubt could have remained of the ice in question being a new production; a fact which I now observed must have removed all suspicion. I found a coating of ice, of considerable extent and perfectly transparent, about a 10th of an inch in thickness, on the outside of the funnel, and on a part of it which was not in contact with the surrounding ice, for that was melted to the distance of an inch from it. Some of the ice being scraped off from the inside of the funnel, and applied to the bulb of the thermometer, the mercury sunk from  $50^{\circ}$  to  $32^{\circ}$ , and continued at that point till the ice was melted; after which, the water being poured off, it rose in a little time to  $47^{\circ}$ .

Astonished at these appearances, of the water freezing after it had been melted, though surrounded with ice in a melting state, and in an atmosphere about  $50^{\circ}$ , where no part of the apparatus or materials could be supposed to be lower than the freezing point, I suspected at first that some of the salt of the freezing mixture might have got into the water, and that this, in dissolving, might perhaps absorb, from the parts contiguous to it, a greater proportion of heat than the ice of pure water does. But the water betrayed nothing saline to the taste, and I had applied the freezing mixture with my own hands, with great care, to prevent any of it being mixed with the water. To remove all doubts however on this point, I purposed repeating the experiment with some pieces of the ice I had stored up in the cellar, to see if this would congeal, after thawing, in the same manner. But going to fetch the ice, and examining it in the cask in which it was kept, I was perfectly satisfied with the appearances I found there; for though much of it was melted, yet the fragments were frozen together, so that it was with difficulty I could break or get out any pieces of it with an iron spade; and, when so broken, it had the appearance of breccia marble or plum-pudding stone, for the fragments had been broken and rammed into the cask with an iron mallet.



A porcelain cup being laid on some of this ice about half an hour, in a room whose temperature was  $50^{\circ}$ , it was found pretty firmly adhering, and when pulled off, the ice exhibited an exact impression of the fluted part of the cup which it had been in contact with; so that the ice must necessarily have liquefied first, and afterwards congealed again. This was repeated several times, with the same event. Fragments of the ice were also applied to each other, to sponges, to pieces of flannel and of linen cloth, both moist and dry: all these, in a few seconds, began to cohere, and in about a minute were frozen so as to require some force to separate them. After standing an hour, the cohesion was so firm, that on pulling away the fragments of ice from the woollen and sponge, they tore off with them that part of the surface which they were in contact with, though at the same time both the sponge and flannel were filled with water which that very ice had produced.

To make some estimate of the force of the congelation, which was stronger on the two bodies last mentioned than on linen, I applied a piece of ice to a piece of dry flannel which weighed  $2\frac{1}{2}$  dwts. and surrounded them with other ice. After lying together  $\frac{3}{4}$  of an hour, taking the piece of ice in my hand and hooking the flannel to a scale, I found a weight of 5 oz. to be necessary for pulling it off, and yet so much of the ice had liquefied as to increase the weight of the flannel above 12 dwts. I then weighed the piece of ice, put them together again, and 4 hours after found them frozen so firmly as to require 78 oz. for their separation, though from 42 dwts. of the ice, 15 more had melted off: the surface of contact was at this time nearly a square inch. I continued them again together for 7 hours; but they now bore only 62 oz. the ice being diminished to 14 dwts. and the surface of contact reduced to about  $\frac{6}{10}$  of a square inch.

Having seen before that pounded ice absorbs water in very considerable quantity, I suspected that something of the same kind might take place even with entire masses; and experiment soon convinced me, that even apparently solid pieces of ice will imbibe water, slower or quicker according to its stage of decay. I have repeatedly heated some of my thermometer pieces, and laid them on ice, in which they made cavities of considerable depth, but the water was always absorbed, sometimes as fast as it was produced, leaving both the piece and the cavity dry. Thus, though I cannot sufficiently express how much I admire the discovery that gave rise to these experiments, I have nevertheless to lament my not being able to avail myself of it at present for the purpose I wished to apply it to.

That in my experiments the two seemingly opposite processes of nature, congelation and liquefaction, went on together, at the same instant, in the same vessel, and even in the same fragment of ice, is a fact of which I have the



fullest evidence that my senses can give me; and I shall take the liberty of suggesting a few hints, which may tend perhaps to elucidate their cause, and to show that they are not so incompatible as at first sight they appear to be. It occurred to me at first, that water highly attenuated and divided, as when reduced into vapour, may freeze with a less degree of cold than water in its aggregate or grosser form; hence hoar-frost is observed on grass, trees, &c. at times when there is no appearance of ice on water, and when the thermometer is above the freezing point.\* Boerhaave, I find, in his elaborate theory of fire, assigns  $33^{\circ}$  as the freezing point of vapour, and even of water when divided only by being imbibed in a linen cloth:

Now, as the atmosphere abounds with watery vapour, or water dissolved and chemically combined, and must be particularly loaded with it in the neighbourhood of melting ice; as the heated body introduced into the funnel must necessarily convert a portion of the ice or water there into vapour; and as ice is known to melt as soon as the heat begins to exceed  $32^{\circ}$ , or nearly  $1^{\circ}$  lower than the freezing point of vapour; I think we may hence deduce, pretty satisfactorily, all the phenomena I have observed. For it naturally follows from these principles, that vapour may freeze where ice is melting; that the vapour may congeal even on the surface of the melting ice itself; and that the heat which, agreeably to the ingenious theory of Dr. Black, it emits in freezing, may contribute to the further liquefaction of that very ice on which the new congelation is formed.

I would further observe, that the freezing of water is attended with a plentiful evaporation in a close as well as an open vessel, the vapour in the former condensing into drops on the under side of the cover, which either continue in the form of water, or assume that of ice or a kind of snow, according to circumstances;† which evaporation may perhaps be attributed to the heat that was combined with the water, at this moment rapidly making its escape, and carry-

\* I am aware that experiments and observations of this kind are not fully decisive; that the atmosphere may, in certain circumstances, be much warmer or colder than the earth and waters, which, in virtue of their density, are far more retentive of the temperature they have once received, and less susceptible of transient impressions; that even insensible undulations of water, from the slightest motion of the air, by bringing up warmer surfaces from below, may prove a further impediment to the freezing; and therefore, that the degree of cold, which is sufficient to produce hoar-frost, may possibly, if continued long enough, be sufficient also to produce ice. I am not acquainted with any satisfactory experiments or observations yet made on the subject; nor do I advance the principle as a certain, but as a probable one, which occurred to me at the moment, which is countenanced by general observation, and consentaneous to many known facts; for there are numerous instances of bodies, in an extreme state of division, yielding easily to chemical agents which, before such division, they entirely resist: thus some precipitates, in the very subtile state in which they are at first extricated from their dissolvents, are re-dissolved by other menstrua, which, after their concretion into sensible molecule, have no action on them at all.—Orig.

† See Mr. Baron's paper on this subject, in the Paris Memoires for the year 1773.—Orig.



ing part of the aqueous fluid off with it. We are hence furnished with a fresh and continual source of vapour as well as of heat; so that the processes of liquefaction and congelation may go on uninterruptedly together, and even necessarily accompany each other, though, as the freezing must be in an under proportion to the melting, the whole of the ice must ultimately be consumed.

In the remarkable instance of the coating of ice on the outside of the throat of the funnel, there are some other circumstances which it may be proper to take notice of. Neither the cover of the outer vessel, nor the aperture in its bottom which the stem of the funnel passed through, were air-tight, and the melting of the surrounding ice had left a vacancy of about an inch round that part of the funnel on which the crust had formed. As there was therefore a passage for air through the vessel, a circulation of it would probably take place: the cold and dense air in the vessel would descend into the rarer air of the room then about  $50^{\circ}$ , and be replaced by air from above. The effect of this circulation and sudden refrigeration of the air will be a condensation of part of the moisture it contains on the bodies it is in contact with: the throat of the funnel, being one of those bodies, must receive its share; and the degree of cold in which the ice thaws being supposed sufficient for the freezing of this moist vapour, the contact, condensation, and freezing, may happen at the same instant. The same principles apply to every instance of congelation that took place in these experiments; and a recollection of particulars which passed under my own eye convinces me, that the congelation was strongest in those circumstances where vapour was most abundant, and on those bodies which, from their natural or mechanic structure, were capacious of the greatest quantity of it; stronger, for instance, on sponge than on woollen, stronger on this than on the closer texture of linen, and far stronger on all these than on the compact surface of porcelain.

However, if the principle I have assumed (that water highly attenuated will congeal with a less degree of cold than water in the mass) should not be admitted; another has above been hinted at, which experiments have decidedly established, from which the phenomena may perhaps be equally accounted for, and which, even though the other also is received, must be supposed to concur for some part of the effect; I mean, that evaporation produces cold; both vapour and steam carrying off some proportion of heat from the body which produces them. If therefore evaporation be made to take place on the surface of ice, the contiguous ice will thus be rendered colder; and as it is already at the freezing point, the smallest increase of cold will be sufficient for fresh congelation. It seems to be on this principle that the formation of ice is effected in the East Indies, by exposing water to a serene air, at the coldest season of the year, in shallow porous earthen vessels: part of the water transudes through the vessel,



and evaporating from the outside, the remainder in the vessel becomes cold enough to freeze; the warmth of the earth being at the same time intercepted by the vessels being placed on bodies little disposed to conduct heat.\* If ice is thus producible in a climate where natural ice is never seen, we need not wonder that congelation should take place where the same principle operates amidst actual ice.

It has been observed above, that the heat emitted by the congealing vapour probably unites with and liquefies contiguous portions of ice; but whether the whole, either of the heat so emitted, or of that originally introduced into the funnel, is thus taken up; how often it may unite with other portions of ice, and be driven out from other new congelations; whether there exists any difference in its chemical affinity or elective attraction to water in different states and the contiguous bodies; whether part of it may not ultimately escape, without performing the office expected from it on the ice; and to what distance from the evaporating surface the refrigerating effect of the evaporation may extend; must be left for further experiments to determine.

*XXVIII. On the Summation of Series, whose General Term is a Determinate Function of  $z$  the Distance from the first Term of the Series. By Edw. Waring, M. D., F. R. S. p. 385.*

PROB. The sum  $s$  being given, to find a series of which it is the sum.

1. Reduce the sum  $s$  into a converging series, proceeding according to the dimensions of any small quantities, and it is done. For example: let any algebraical function  $s$  of an unknown or small quantity  $x$  be assumed, reduce it into a converging series proceeding according to the dimensions of  $x$ , and there results a series whose sum is  $s$ . 2. Let  $A, B, C, \&c.$  be algebraical functions of  $x$ ; reduce the  $\int A \dot{x}, \int B \dot{x}, \int C \dot{x}, \&c.$  into a converging series, proceeding according to the dimensions of  $x$ , and the problem is done.

It is always necessary to find the values of  $x$ , between which the above-mentioned series converge. Reduce the algebraical function  $s$  in the first example, and the algebraical functions  $A, B, C, \&c.$  in the 2d into their lowest terms; and in such a manner that the quantities contained in the numerator and denominator may have no denominator: make the denominator in the first example, and the denominator in the 2d, and every distinct irrational quantity contained in them, respectively  $= 0$ ; and also every distinct irrational quantity contained in the numerators  $= 0$ . Suppose  $\alpha$  the least root, affirmative or negative, (but not  $= 0$ ) of the above-mentioned resulting equations; then a series ascending according to the dimensions of  $x$  will always converge, if the value of  $x$  be contained be-

\* See a description of this process in the Phil. Trans. vol. 65, p. 253.—Orig.



tween  $\alpha$  and  $-\alpha$ ; but if  $x$  be greater than  $\alpha$  or  $-\alpha$ , the above-mentioned series will diverge. Let  $\pi$  be the greatest root of the above-mentioned resulting equations; then a series descending according to the reciprocal dimensions of  $x$  will converge, if  $x$  be greater than  $\pm \pi$ ; but, if less, not. When impossible roots  $a \pm b\sqrt{-1}$  are contained in the equations, an ascending series will converge, if  $x$  be less than the least root  $\pm \alpha$ , and  $\pm (a - b)$  and  $\pm (a + b)$ ; or more generally, if  $x$  be less than the least root  $\pm \alpha$ , and  $x^{n+r}$  at an infinite distance  $n$ , be infinitely less than

$$\frac{2a^n - 2 \cdot n \cdot \frac{n-1}{2} a^{n-2} b^2 + 2 \cdot n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} a^{n-4} b^4 - \&c.}{(a^2 + b^2)^n},$$

a descending series will always converge, when  $x$  is greater than the greatest root of the resulting equations; and  $x^{n-1}$ , when  $n$  is infinite, is infinitely greater than  $(a + b)^n$  and  $(a - b)^n$ ; or more generally than

$$2a^n - 2n \cdot \frac{n-1}{2} a^{n-2} b^2 + 2n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} a^{n-4} b^4 - \&c.$$

This follows from Caput 3 of the *Meditationes Algebraicæ*.

*Cor.* It hence appears, that if the ascending series converges, the descending will diverge; and, vice versâ, if the descending converges, the ascending will diverge, unless all the roots of the above-mentioned resulting equations may be deemed of equal magnitude, as  $+\alpha$  and  $-\alpha$ ,  $\alpha\sqrt{-1}$ , &c. and  $x = \alpha$ ; in which case sometimes both series may become the same converging series, &c. —When  $x$ , in the preceding cases, is equal to the least or greatest root, the series will sometimes converge, and sometimes not, as is shown in the above-mentioned chapter. Whether the sum of a series, whose general term is given, can be found or not, will in many cases appear, from the law of the multinomial and other more general serieses.

2. There are serieses which always converge, whatever be the value of  $x$ ; as, for example, the series  $1 + \frac{1}{2 \cdot 3} x + \frac{1}{2 \cdot 3 \cdot 4 \cdot 5} x^2 + \&c.$  or  $1 + \frac{x}{2^2} + \frac{x^2}{3^3} + \frac{x^3}{4^4} + \&c. \&c.$  always converge, whatever may be the value of  $x$ ; but it may be observed, that these serieses never arise from the expansion of algebraical functions of  $x$ , or the before-mentioned fluents; but, in a few cases, they may from fluxional equations. There are also serieses which never converge as  $1 + 1 \cdot 2x + 1 \cdot 2 \cdot 3x^2 + 1 \cdot 2 \cdot 3 \cdot 4x^3 + \&c.$  to which the preceding remark may be applied.

3. In the year 1757 some papers, which contained the first edition of my *Meditationes Algebraicæ*, were sent to the R. S., in which was contained the following rule, viz. let  $s$  be a given function of the quantity  $x$ , which expand into a series  $(a + bx^m + cx^{2m} + \&c.)$  proceeding according to the dimensions of  $x$ ; in the quantity  $s$ , for  $x^m$  write  $\alpha x^m$ ,  $\beta x^m$ ,  $\gamma x^m$ , &c. where  $\alpha$ ,  $\beta$ ,  $\gamma$ , &c. are roots of the equation  $x^n - 1 = 0$ ; and let the resulting quantities be  $A$ ,  $B$ ,  $C$ ,  $D$ , &c.



then will  $\frac{A + B + C + D + \&c.}{n}$  be equal to the sum of the first  $2n + 1$ ,  $3n + 1$ ,  $\&c.$  terms in infinitum. This method, in the preface to the last edition of the *Meditationes Algebraicæ*, is rendered more correct and general.

4. Let the sum of a series required be expressed by a function of a quantity  $z$ , the distance from the first term of the series, then will the general term be the difference between the two successive sums generally expressed.

5. Let the general term be an algebraical function of  $z$ : 1st, let it be  $\frac{az^m + bz^{m-1} + cz^{m-2} + \&c.}{z + e.z + e + 1.z + e + 2 \dots z + e + n - 1} = \tau$ , where  $m$  and  $n$  are whole numbers; and  $m$  (if the sum of an infinite series of terms is required) less than  $n$  by two or more: then the general term  $\frac{az^m + bz^{m-1} + \&c.}{z + e.z + e + 1.z + e + 2 \dots z + e + n - 1}$

$$= \frac{\gamma}{z + e.z + e + 1} + \frac{\delta}{z + e.z + e + 1.z + e + 2} + \frac{\varepsilon}{z + e.z + e + 1.z + e + 2.z + e + 3} + \&c. \dots \frac{\theta}{z + e.z + e + 1 \dots z + e + n - 1} : \text{whence, if}$$

$$z + e + 2.z + e + 3.z + e + 4 \dots z + e + n - 1 = z^{n-2} + Az^{n-3} + Bz^{n-4} + \&c.;$$

$$z + e + 3.z + e + 4.z + e + 5 \dots z + e + n - 1 = z^{n-3} + A'z^{n-4} + B'z^{n-5} + \&c.;$$

$$z + e + 4.z + e + 5 \dots z + e + n - 1 = z^{n-4} + A''z^{n-5} + B''z^{n-6} + \&c.$$

and so on; then, if  $m = n - 2$ , will  $\gamma = a$ ,  $\delta = b - \gamma A$ ,  $\varepsilon = c - \delta A' - \gamma B$ ,  $\zeta = d - \varepsilon A'' - \delta B' - \gamma C$ ,  $\&c.$ ; the integral in infinitum, or sum of the infinite series, will be  $\frac{\gamma}{z + e} + \frac{\delta}{2.z + e.z + e + 1} + \frac{\varepsilon}{3.z + e.z + e + 1.z + e + 2} + \&c.$

The reduction of the general term  $\tau$  into quantities of the before given formulæ was published in the *Meditationes*, printed in the year 1774. It was before reduced into formulæ of the same kind nearly by Mr. Nichole in the *Paris Acts*.

2d. Let the general term be  $\tau' =$

$\frac{az^h + bz^{h-1} + cz^{h-2} + \&c.}{z + e.z + e + 1.z + e + 2 \dots z + e + n - 1 \times z + f.z + f + 1 \dots z + f + m - 1 \times z + g.z + g + 1 \dots z + g + l - 1 \times \&c.}$  where  $h$  is a whole number less than  $n + m + l + \&c.$  (if it be greater, then the fraction can easily be reduced into a rational quantity  $az^{b-n-m-l-\&c.} + \&c.$  and a fraction of the before-mentioned kind); then will  $\tau' =$

$$\left( \frac{\alpha}{z + e} + \frac{\alpha'}{z + f} + \frac{\alpha''}{z + g} + \&c. \right) + \left( \frac{\beta}{z + e.z + e + 1} + \frac{\beta'}{z + f.z + f + 1} + \frac{\beta''}{z + g.z + g + 1} + \&c. \right) + \left( \frac{\gamma}{z + e.z + e + 1.z + e + 2} + \frac{\gamma'}{z + f.z + f + 1.z + f + 2} + \frac{\gamma''}{z + g.z + g + 1.z + g + 2} + \&c. \right) \dots$$

$$\left( \frac{x}{z + e.z + e + 1 \dots z + e + n - 1} + \frac{x'}{z + f.z + f + 1 \dots z + f + m - 1} + \frac{x''}{z + g.z + g + 1 \dots z + g + l - 1} + \&c. \right);$$

whence its integral in infinitum, that is, the sum of the infinite series can be found when  $\alpha = 0$ ,  $\alpha' = 0$ ,  $\alpha'' = 0$ ,  $\&c.$ ; and consequently  $h$  not greater than  $n + m + l + \&c. - 2$ ; otherwise not. If  $h$  is not greater than  $n + m + l + \&c. - 2$ , then will  $\alpha + \alpha' + \alpha'' + \&c. = 0$ , for else the sum would be infinite.

Let the number of quantities ( $e, f, g, \&c.$ ) be  $r$ , then from  $r$  independent integrals of a series, whose term is  $\tau'$ ; or from  $(r - 1)$  independent sums of in-



finite serieses, whose term is  $r'$ ; that is, where  $h$  is not greater than  $n + m + 1 + \&c. - 2$ ; can be deduced the sum of all infinite serieses of the before-mentioned formulæ, whose general term is  $r'$ .

If any factors are deficient in the denominator, as suppose the term to be  $z + e \times z + e + 3 \times z + e + n - 1$ ; multiplying the numerator and denominator by the deficient factors, viz. by  $z + e + 1 \cdot z + e + 2 \times z + e + 4 \cdot z + e + 5 \cdot z + e + n - 2$ , and it acquires the preceding formula; and so in the following examples.

3d. Let the denominator be  $(x + e)^\pi \times (x + e + 1)^\pi \times (x + e + 2)^\pi \dots (x + e + n - 1)^\pi \times (x + e + \mu)^{\pi'} \times (x + e + \mu + 1)^{\pi'} \times (x + e + \mu + 2)^{\pi'} \times \&c. \times (x + f)^\rho \times (x + f + 1)^\rho \times (x + f + 2)^\rho \dots \times (x + f + m - 1)^\rho \times \&c. = D$ , where  $\pi, \pi', \rho, \&c.; \mu, \&c.$  are whole numbers; and the general term is  $\frac{az^h + bz^{h-1} + cz^{h-2} + \&c.}{D} = T''$ ; then, if the dimensions of  $z$  in the numerator be less than its dimensions in the denominator, will  $T'' =$

$(\frac{\alpha}{z + e} + \frac{\alpha'}{(z + e)^2} + \frac{\alpha''}{(z + e)^3} \dots \frac{\alpha^{p-1}}{(z + e)^{p-1}} + \frac{\beta}{z + f} + \frac{\beta'}{(z + f)^2} + \frac{\beta''}{(z + f)^3} \dots \frac{\beta^{q-1}}{(z + f)^{q-1}} + \&c.) + (\frac{\gamma}{z + e \cdot z + e + 1} + \frac{\theta}{z + f \cdot z + f + 1} + \&c.) + \&c.$ ; and in general there will be included all terms of the formulæ,

$\frac{(z + e + i)^p - (z + e)^p}{(z + e)^p \cdot (z + e + 1)^p \dots (z + e + i)^p} A, \frac{(z + f + i')^{p'} - (z + f)^{p'}}{(z + f)^{p'} \cdot (z + f + 1)^{p'} \dots (z + f + i')^{p'}} B, \&c. \frac{(z + e + \mu + i'')^{p''} - (z + e + \mu)^{p''}}{(z + e + \mu)^{p''} \cdot (z + e + \mu + 1)^{p''} \dots (z + e + \mu + i'')^{p''}} C, \&c.$  where  $A, B, C, \&c. \alpha, \alpha', \alpha'', \&c. \beta, \beta', \beta'', \&c. \gamma, \theta, \&c.$  denote invariable quantities; and  $p, p', p'', \&c.$  are whole numbers not greater than  $\pi, \rho, \pi', \&c.$  respectively; and  $i, i', i'', \&c.$  are whole numbers not greater than  $n - 1, m - 1, \&c.$

If all the quantities  $\alpha, \alpha', \alpha'', \&c. \beta, \beta', \beta'', \&c. \&c.$  are  $= 0$ , the sum of the series can be expressed in finite terms of the quantity  $z$ , otherwise not; and also if  $h$  be less than the dimensions of  $z$  in the denominator by 2 or more, then will  $\alpha + \beta + \&c. = 0$ , otherwise the sum would be infinite.

From  $\pi + \pi' + \rho + \&c. - 1$  independent sums of infinite serieses of this kind can be deduced the sums of all infinite serieses of the same kind. This method may be extended to infinite series, in which exponentials, as  $e^z$ , are contained, which will easily be seen from some subsequent propositions; but in my opinion the subsequent method of finding the sum of serieses is to be preferred to the preceding one, both for its generality and facility.

6. 1. Let the general term be  $(az^h + bz^{h-1} + cz^{h-2} + \&c. \times (z + e)^{-1} \cdot (z + e + 1)^{-1} \cdot (z + e + 2)^{-1} \dots (z + e + n - 1)^{-1}$ ; where  $h$  is a whole number less than  $n$  by 2 or more, when the sum of an infinite series is required.

Assume for the sum the quantity  $(z + e)^{-1} \cdot (z + e + 1)^{-1} \cdot (z + e + 2)^{-1} \dots (z + e + n - 2)^{-1} \times (\alpha z^{b'} + \beta z^{b'-1} + \gamma z^{b'-2} + \&c.)$ ; find the difference



between this sum and its successive one  $(z + e + 1)^{-1} \cdot (z + e + 2)^{-1} \cdot (z + e + 3)^{-1} \dots (z + e + n - 1)^{-1} \times (\alpha(z + 1)^{b'} + \beta(z + 1)^{b'-1} + \&c.)$ , which will be  $-(z + e)^{-1} \cdot (z + e + 1)^{-1} \cdot (z + e + 2)^{-1} \dots (z + e + n - 1)^{-1} \times ((z + e) \times (\alpha(z + 1)^{b'} + \beta(z + 1)^{b'-1} + \&c.) - (z + e + n - 1) \times (\alpha z^{b'} + \beta z^{b'-1} + \&c.) = (h' - n + 1)\alpha z^{b'} + \&c.)$ ; then make the terms of this difference equal to the correspondent terms of the given quantity  $az^b + bz^{b-1} + \&c.$  and there result  $h' = h$ ,  $-(h - n + 1) \times \alpha = a$ , and consequently  $\alpha = \frac{-a}{h - n + 1}$ ,  $\&c.$

2. Let the general term be  $(z + e)^{-1} \cdot (z + e + 1)^{-1} \cdot (z + e + 2)^{-1} \dots (z + e + n - 1)^{-1} \times (z + f)^{-1} \cdot (z + f + 1)^{-1} \cdot (z + f + 2)^{-1} \dots (z + f + m - 1)^{-1} \times (az^b + bz^{b-1} + cz^{b-2} + \&c.)$ . Assume the quantity  $(z + e)^{-1} \cdot (z + e + 1)^{-1} \dots (z + e + n - 2)^{-1} \times (z + f)^{-1} \cdot (z + f + 1)^{-1} \cdot (z + f + 2)^{-1} \dots (z + f + m - 2)^{-1} \times (\alpha z^{b'} + \beta z^{b'-1} + \gamma z^{b'-2} + \&c.)$  for the sum of the series sought; and thence deduce the general term, which suppose equal to the given general term, and from equating their corresponding parts easily can be deduced the index  $h'$  and co-efficients  $\alpha$ ,  $\beta$ ,  $\gamma$ ,  $\&c.$  and consequently the sum of the series sought.

3. Let the general term reduced to its lowest dimensions be  $(z + e)^\pi \times (z + e + 1)^\pi \dots (z + e + n - 1)^\pi \times (rz + f)^{-\epsilon} \times (rz + f + r)^{-\epsilon} \times (rz + f + 2r)^{-\epsilon} \dots (rz + f + (m - 1)r)^{-\epsilon} \times (z + g)^{-\sigma} \times (z + g + 1)^{-\sigma} \times \dots (z + g + l - 1)^{-\sigma} \times \&c. \times (az^b + bz^{b-1} + cz^{b-2} + \&c.)$ . If it be required to reduce the term  $(rz + f)^{-\epsilon}$ ,  $\&c.$  to a conformity with the rest, for  $(rz + f)^{-\epsilon}$ ,  $\&c.$  substitute  $(z + \frac{f}{r})^{-\epsilon} \times r^{-\epsilon}$ ,  $\&c.$  and it is done. Assume for the integral or sum the quantity  $s = (z + e)^\pi \cdot (z + e + 1)^\pi \dots (z + e + n - 2)^\pi \times (rz + f)^{-\epsilon} \cdot (rz + r + f)^{-\epsilon} \dots (rz + (m - 2)r + f)^{-\epsilon} \times (z + g)^{-\sigma} \times (z + g + 1)^{-\sigma} \dots (z + g + l - 2)^{-\sigma} \times \&c. \times (\alpha z^{b'} + \beta z^{b'-1} + \&c.) = s$ , find its successive sum by writing  $z + 1$  for  $z$  in the sum  $s$ , and let the quantity resulting be  $s'$ ; then will the general term be  $s - s'$ , which equate to the given general term, that is, their correspondent quantities; and thence may be deduced the index  $h'$  and co-efficients  $\alpha$ ,  $\beta$ ,  $\&c.$ ; and consequently the sum sought. If the series does not terminate, then the sum will be expressed by a series proceeding in infinitum, according to the reciprocal dimensions of  $z$ .

From  $\pi + \epsilon + \sigma + \&c. - 1$  independent integrals of the above-mentioned kind can be deduced the integrals of all quantities of the same kind; that is, where  $h$  is any whole affirmative number whatever, and the co-efficients  $a$ ,  $b$ ,  $c$ ,  $\&c.$  are any how varied. If any factor  $z + g$  in the denominator,  $\&c.$  has no other  $z + g + l - 1$ , which differs from it by a whole number  $l - 1$ ; or the factor  $rz + f$  has no correspondent factor  $rz + f + mr$ , where  $m$  is a whole number; then the integral of the above-mentioned series cannot be expressed in



finite terms of the quantity  $z$ . In like manner, if the dimensions of  $z$  in the numerator are less than its dimensions in the denominator by unity, then the integral of the general term cannot be expressed by a finite algebraical function of  $z$ . If the number of terms to be added be infinite, it is well known that the sum in this case will be infinite.

It may be observed, that in finding the sum of a series, whose general term is given, all common divisors of the numerator and denominator must be rejected, otherwise serieses may appear difficult to be summed, which are very easy :

for example, let the series be  $\frac{1}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} + \frac{4}{4 \cdot 5 \cdot 6 \cdot 7 \cdot 8} + \frac{9}{7 \cdot 8 \cdot 9 \cdot 10 \cdot 11} + \&c. = \frac{1}{3} \left( \frac{1}{1 \cdot 2 \cdot 4 \cdot 5} + \frac{2}{4 \cdot 5 \cdot 7 \cdot 8} + \frac{3}{7 \cdot 8 \cdot 10 \cdot 11} + \&c. \right)$ , whose general term is  $\frac{z+1}{3z+1 \cdot 3z+4 \times 3z+2 \cdot 3z+5}$ ; and by assuming, as is before taught,  $(3z+1)^{-1} \times (3z+2)^{-1} \times z$  for the sum sought; and finding its general term  $(3z+1)^{-1} \times (3z+4)^{-1} \times (3z+2)^{-1} \times (3z+5)^{-1} \times 18(z+1) \times \alpha$ , which equating to the general term given, there results  $18\alpha = 1$ , and the sum sought  $= \frac{1}{18} \times \frac{1}{3z+1 \cdot 3z+2}$ .

*Ex. 2.* Let the series be  $\frac{1}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} + \frac{14}{5 \cdot 6 \cdot 7 \cdot 8 \cdot 9} + \frac{55}{9 \cdot 10 \cdot 11 \cdot 12 \cdot 13} + \frac{140}{13 \cdot 14 \cdot 15 \cdot 16 \cdot 17} + \&c. = \frac{1}{24} \left( \frac{1}{1 \cdot 5} + \frac{1}{5 \cdot 9} + \frac{1}{9 \cdot 13} + \frac{1}{13 \cdot 17} + \&c. \right)$ , of which the general term is  $\frac{1}{24} \times \frac{1}{4z+1 \cdot 4z+5}$ ; and consequently the sum deduced is  $\frac{1}{24} \times \frac{1}{4} \times \frac{1}{4z+1}$ .

These are serieses given by Mr. De Moivre, and esteemed by Dr. Taylor *altioris indaginis*. Some other writers have made some serieses to appear more difficult to be summed, by not reducing them to their lowest terms.

7. Having given the principles of a general method of finding the sum of a series, when its general term can be expressed by algebraical, and not exponential, functions of  $z$ , the distance from the first term of the series; it remains to perform the same when exponentials are included.

1. Let  $s$  the sum be any algebraical function of  $z$  multiplied into  $e^z = x^z$ ; then will the general term be  $se^z - es'e^z = (s - es')e^z$ ; whence, from the general term  $te^z$  being given, assume quantities in the same manner (with the same denominator, &c.) as when no exponential was involved, which multiplied into  $e^z$ , suppose to be the sum; from the sum find its general term, and equate it to the given one by equating their correspondent co-efficients, and it is done.

*Ex.* Let the general term be  $\frac{z+2}{2z+1 \cdot 2z+3} \times e^{z+1}$ : assume for the sum sought  $\frac{\alpha}{2z+1} \times e^{z+1}$ , whence the general term is  $\left( \frac{\alpha}{2z+1} - \frac{\alpha e}{2z+3} \right) e^{z-1} =$



$\frac{2\alpha(1-e)z + 3\alpha - \alpha e}{2z + 1 \cdot 2z + 3} \times e^{z+1}$ ; equate it to the given term, and there results  $2\alpha(1-e) = 1$  and  $3\alpha - \alpha e = 2$ , and consequently  $e = \frac{1}{3}$  and  $\alpha = \frac{3}{4}$ , if the series can be summed.

The same observation, viz. that if any factor in the denominator or irrational quantity have no other correspondent to it; for example, if the factor be  $z + g$ , and there is no correspondent one  $x + g + n$ , where  $n$  is a whole number, then its integral cannot be expressed by a finite algebraical function of  $z$ . In the same manner may the sums be found, when the terms are exponentials of superior orders; for the exponential, irrational, &c. quantities in the denominators of the sums may be easily deduced from the preceding principles; and thence, by proceeding as is before taught, the sum required. The principles of all these cases have been given in the *Meditationes*.

8. Mr. James Bernoulli found summable serieses by assuming a series  $v$ , whose terms at an infinite distance are infinitely little, and subtracting the series diminished by any number ( $l$ ) of terms from the series itself, &c.

It is observed in the *Meditationes*, that if  $\tau(m)$ ,  $\tau(m+n)$ ,  $\tau(m+n+n')$ ,  $\tau(m+n+n'+n'')$ , &c. be the terms at  $m$ ,  $m+n$ ,  $m+n+n'$ ,  $m+n+n'+n''$ , &c. distances from the first, and  $a\tau(m) + b\tau(m+n) + c\tau(m+n+n') + d\tau(m+n+n'+n'') + \&c.$  be the general term, it will be summable, when  $a + b + c + d + \&c. = 0$ ; the sum of the series will be  $a(\tau(m) + \tau(m+1) + \tau(m+2) + \dots + \tau(m+n+n'+n'' + \&c. - 1)) + b(\tau(m+n) + \tau(m+n+1) + \tau(m+n+2) + \dots + \tau(m+n+n'+n'' + \&c. - 1)) + c(\tau(m+n+n') + \tau(m+n+n'+1) + \dots + \tau(m+n+n'+n'' + \&c. - 1)) + \&c. = H$ . If the sum  $a + b + c + d + \&c.$  be not  $= 0$ , and the series  $\tau(m) + \tau(m+1) + \tau(m+2) + \&c.$  in infinitum be a converging one  $= s$ , then will the sum of the resulting series be  $(a + b + c + d + \&c.)s - (b + c + d + \&c.) \cdot (\tau^m \dots + \tau^{m+n-1}) - (c + d + \&c.) (\tau^{m+n} \dots + \tau^{m+n+n'-1}) - (d + \&c.) \cdot (\tau^{m+n+n'} + \dots + \tau^{m+n+n'+n''-1}) - \&c.$

8. 2. Let the series  $v$  consist of terms, which have only one factor in the denominator, and its numerator  $= 1$ ; that is, let the general term be

$\frac{1}{rz + e}$  and the series consequently  $\frac{1}{e} + \frac{1}{r+e} + \frac{1}{2r+e} + \&c. = v$ ; from the before-mentioned addition or subtraction there follows

$$\frac{a}{rz + e} + \frac{b}{rz + r + e} + \frac{c}{rz + 2r + e} + \&c. = \frac{\alpha z^m + \beta z^{m-1} + \gamma z^{m-2} + \&c.}{rz + e \cdot rz + r + e \cdot rz + 2r + e \cdot \&c.};$$

where  $m$  is not greater than the number ( $N$ ) of factors in the denominator diminished by unity. From  $\alpha$ ,  $\beta$ ,  $\gamma$ , &c.  $r$  and  $e$  being given, easily can be acquired by simple equations, or known theorems, the required co-efficients  $a$ ,  $b$ ,  $c$ , &c. If  $m = N - 1$ , and  $\alpha$  and  $a + b + c + d + \&c. = 0$ , then the sum of the series resulting will be finite.



8. 3. If the terms of the series assumed  $\frac{1}{e} - \frac{1}{r+e} + \frac{1}{2r+e} - \frac{1}{3r+e} +$   
&c. be alternately affirmative and negative; then by the preceding case find

$$\frac{\alpha z^m + \beta z^{m-1} + \gamma z^{m-2} + \&c.}{rz + e . rz + r + e . rz + 2r + e + \&c.} = \frac{a}{rz + e} + \frac{b}{rz + r + e} + \frac{c}{rz + 2r + e} + \&c.$$

Where the terms of the resulting series are alternately affirmative and negative, let the two subsequent terms be supposed  $\frac{\alpha z^m + \beta z^{m-1} + \gamma z^{m-2} + \&c.}{rz + e . rz + r + e \dots rz + (n-1)r + e}$

$$= \frac{a}{rz + e} + \frac{b}{rz + r + e} + \&c. \text{ and } \frac{\alpha(z+1)^m + \beta(z+1)^{m-1} + \gamma(z+1)^{m-2} + \&c.}{rz + r + e . rz + 2r + e \dots rz + nr + e} =$$

$\frac{a}{rz + r + e} + \frac{b}{rz + 2r + e} + \&c.$  of which the one is affirmative and the other negative: reduce the resulting series to an affirmative one by subtracting the subsequent term from its preceding, and it becomes

$$\frac{(rz + nr + e) . (\alpha z^m + \beta z^{m-1} + \&c.) - (rz + e) . (\alpha(z+1)^m + \beta(z+1)^{m-1} + \&c.)}{rz + e . rz + r + e . rz + 2r + e \dots rz + nr + e} = \frac{(n-m)r \alpha z^m + \&c.}{rz + e . rz + r + e \dots rz + nr + e} = \frac{a}{rz + e} + \frac{b-a}{rz + r + e} + \&c. \text{ In this case,}$$

since two terms are added into one, the distance from the first term of the series will be  $\frac{z}{2}$ , which suppose  $= w$ ; and write  $2w$  for  $z$  in the above-mentioned

term, and there results  $\frac{(n-m)r \alpha z^m + \&c.}{rz + e . rz + r + e \dots rz + nr + e}$

$$= \frac{(n-m)r \alpha \times 2^m w^m + \&c.}{2rw + e . 2rw + r + e \dots 2rw + nr + e} = \frac{a}{2rw + e} + \frac{b-a}{2rw + r + e} + \&c.; \text{ whence}$$

the sum of any series, whose general term is  $\frac{a'w^m + b'w^{m-1} + \&c.}{2rw + e . 2rw + r + e \dots 2rw + nr + e}$

where  $m$  is a whole number less than  $n$  by 2 or more, and  $w$  the distance from the first term of the series can be found from the sum of the series

$$\frac{1}{e} - \frac{1}{r+e} + \frac{1}{2r+e} - \frac{1}{3r+e} + \&c.$$

9. Let there be two serieses  $\frac{1}{e} + \frac{1}{e+r} + \frac{1}{e+2r} + \&c. = s$ , and  $\frac{1}{f} + \frac{1}{f+r}$

$+ \frac{1}{f+2r} + \frac{1}{f+3r} + \&c. = s'$ , whose general terms are respectively  $+\frac{1}{e+rz}$

and  $+\frac{1}{f+rz}$ ; then from the sum of these two serieses can be collected the sum of any series, whose general term is

$$\frac{\alpha z^m + \beta z^{m-1} + \&c.}{rz + e . rz + e + r . rz + e + 2r \dots rz + (n-1)r + e \times rz + f . rz + r + f \dots rz + f + (m-1)r} = \frac{a}{rz + e} + \frac{b}{rz + e + r} + \frac{c}{rz + e + 2r} \dots + \frac{\lambda}{rz + (n-1)r + e} + \frac{a'}{rz + f} +$$

$\frac{b'}{rz + r + f} + \frac{c'}{rz + 2r + f} \dots + \frac{\mu'}{rz + (m-1)r + f}$ ; where  $e-f$  is not a whole number. Let  $a+b+c \dots + \lambda = 0$ , and  $a'+b'+c' \dots + \mu' = 0$ , then the

sum will be  $a \left( \frac{1}{rz + e} + \frac{1}{rz + r + e} \dots + \frac{1}{rz + (n-2)r + e} \right) + b \left( \frac{1}{rz + e + r} + \frac{1}{rz + e + 2r} \dots + \frac{1}{rz + (n-2)r + e} \right) + c \left( \frac{1}{rz + e + 2r} + \frac{1}{rz + e + 3r} + \dots + \frac{1}{rz + e + 2r} \right) \dots$



$$\frac{1}{rz + (n-2)r + e}) + \&c. + a' \left( \frac{1}{rz + f} + \frac{1}{rz + r + f} \dots \frac{1}{rz + (m-2)r + f} \right) + b' \\ \left( \frac{1}{rz + r + f} \dots + \frac{1}{rz + (m-2)r + f} \right) + \&c.$$

2. If the serieses are  $\frac{1}{e} - \frac{1}{e+r} + \frac{1}{e+2r} - \&c.$  and  $\frac{1}{f} - \frac{1}{f+r} + \frac{1}{f+2r} - \&c.$ ; then from the sum of these two serieses can be collected, by the principles above given, the sum of any series whose general term is

$$\alpha z^m + \beta z^{m-2} + \gamma z^{m-2} + \&c.$$

$$2rz + e. 2rz + r + e. 2rz + 2r + e \dots 2rz + (n+1)r + e + 2rz + f. 2rz + r + f. 2rz + 2r + f \dots 2rz + (m-1)r + f.$$

The same principle may be applied to find the sum of any series of the above-mentioned sort, in whose denominator are contained other factors,  $rz + g$ ,  $rz + g + r$ ,  $\&c.$   $\&c.$ ; or  $2rz + g$ ,  $2rz + g + r$ ,  $2rz + g + 2r$ ,  $\&c.$  Like propositions may be deduced from serieses, in which  $r$  and  $r'$ ,  $\&c.$  and the factors  $rz + e$  and  $r'z + g$ ,  $\&c.$  denote different quantities.

10. An apparently more general method may be given from assuming a series or serieses as before; and adding every 2, 3, 4,  $\&c.$  ( $n$ ) successive terms together, for terms of a new series beginning from the 1st, 2d, 3d,  $\&c.$   $n^{\text{th}}$  term; and in general adding together 2, 3,  $\&c.$   $n$  successive general terms; and in their sum writing for  $z$  the distance from the first term of the series  $2z + a$ ,  $3z + a$ ,  $\&c.$   $nz + a$ ; there will result the general term of a series not to be found from the above-mentioned addition.

*Ex.* Let the series assumed be  $\frac{1}{1} + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \&c.$  in infinitum, of which the general term beginning from the first is  $\frac{1}{z+1}$ ; add 3 successive general terms  $\frac{1}{z+1} + \frac{1}{z+2} + \frac{1}{z+3} = \frac{3z^2 + 12z + 11}{z+1.z+2.z+3}$ ; in this term for  $z$  write  $3z$ , and there results  $\frac{27z^2 + 36z + 11}{3z+1.3z+2.3z+3}$ . In the same manner, if the beginning is instituted from the 2d or 3d term of the given series, the terms resulting will be  $\frac{3z^2 + 18z + 26}{z+2.z+3.z+4}$  and  $\frac{3z^2 + 24z + 47}{z+3.z+4.z+5}$ . In these terms for  $z$  write  $3z$ , and there result  $\frac{27z^2 + 54z + 26}{3z+2.3z+3.3z+4}$  and  $\frac{27z^2 + 72z + 47}{3z+3.3z+4.3z+5}$ .

If the terms of the given series are alternately affirmative and negative, the terms of the resulting series will be alternately affirmative and negative, if  $n$  be an odd number; otherwise its terms will be all affirmative. The sum of this series will be finite or infinite, as the sum of the series  $1 + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \&c.$  is finite or infinite; but from it, by the preceding method of addition or subtraction of Mr. Bernoulli's, or a like method applied to more serieses, may be found the sums of different finite serieses. It may be observed, that from Mr. Bernoulli's addition or subtraction can never be deduced the serieses which arise from this method; for, by his method, the denominator can never have any factors but what are contained in the denominators of the given series, viz: (in the series  $\frac{1}{1} + \frac{1}{2} +$



$\frac{1}{3} + \&c.$ ),  $2 + l$ , where  $l$  is a whole number; but by this method are introduced into the denominator the factors  $2z + l$ ,  $3z + l$ ,  $\&c.$  and  $nz + l$ , or which may be reduced to the same  $(z + \frac{l}{n}) \times n$ . If  $n$  successive general terms of the series arising from Mr. Bernoulli's addition or subtraction be added together, and in the quantity thence arising for  $z$ , the distance from the first term of the series, be substituted  $nz$ , there will be produced serieses of the above-mentioned formula.

11. Multiply two converging serieses  $a + bx + cx^2 + dx^3 + \&c. = s$  and  $\alpha + \beta x + \gamma x^2 + \&c. = v$ , or find any rational and integral function of them, and the series resulting will be finite and  $= s \times v$ ,  $\&c.$  Let  $\alpha + \beta x + \gamma x^2 + \&c. x^m = v$  be finite, and the resulting series will be finite and  $= s \times v$ ,  $\&c.$  If  $s$  be a series converging or not, whose ultimate terms are less than any finite quantity, then will the series  $(a + bx + cx^2 + \&c.) \times (\alpha + \beta x + \gamma x^2 + \&c. x^m) = v \times s$  be a converging one, if  $\alpha + \beta x + \gamma x^2 + \dots \&c. x^m = 0$ ; which case was given by Mr. de Moivre.

Mr. Bernoulli's addition,  $\&c.$  can be applied to serieses of this kind. For example, let the given series be  $\frac{1}{e} + \frac{1}{e+1}x + \frac{1}{e+2}x^2 + \&c. = s$ . From this

series subtract the same series diminished by  $m$  terms, viz.  $\frac{1}{e+m}x^m + \frac{1}{e+m+1}x^{m+1} + \frac{1}{e+m+2}x^{m+2} + \&c.$  and there remains  $\frac{e+m-e x^m}{e \cdot m + e} + \frac{e+m+1-(e+1)x^m}{e+1 \cdot e+m+1}x + \frac{e+m+2-(e+2)x^m}{e+2 \cdot e+m+2}x^2 + \frac{e+m+3-(e+3)x^m}{e+3 \cdot e+m+3}x^3 + \&c.$ ; for  $x^m$  write  $A$ , then will the series become  $\frac{m-eA}{e \cdot m + e} + \frac{e+m+1-(e+1)A}{e+1 \cdot e+m+1}x + \frac{e+m+2-(e+2)A}{e+2 \cdot e+m+2}x^2 + \frac{e+m+3-(e+3)A}{e+3 \cdot e+m+3}x^3 + \&c. = \frac{1}{e} + \frac{1}{e+1}x + \frac{1}{e+2}x^2 + \dots + \frac{1}{e+m-1}x^{m-1}.$

Let the general term be  $\frac{ax^m + bx^{m-1} + cx^{m-2} + \&c.}{z + e \cdot z + e + 1 \cdot z + e + 2 \dots z + e + n - 1} \times x^z$   
 $= (\frac{\alpha}{z+e} + \frac{\beta}{z+e+1} + \frac{\gamma}{z+e+2} \dots \frac{X}{z+e+n-1}) x^z$ . Suppose  $\beta = \beta'x$ ,  $\gamma = \gamma'x^2$ ,  $\delta = \delta'x^3$ , ..  $x = x'x^{n-1}$ ; then will the sum of the above-mentioned series be  $(\alpha + \beta' + \gamma' + \delta' + \&c.) \times s - \frac{1}{e}(\beta' + \gamma' + \delta' + \&c.) - \frac{1}{e+1}(\gamma' + \delta' + \&c.) - \frac{1}{e+2}(\delta' + \&c.) - \&c.$

From the sum of the series  $\frac{1}{e} - \frac{x}{e+1} + \frac{x^2}{e+2} - \&c.$  by these and the principles before delivered can be deduced the sum of any series whose general term is  $\frac{ax^m + bx^{m-1} + \&c.}{2z + e \cdot 2z + e + 1 \cdot 2z + e + 2 \cdot 2z + e + 3 \times \&c.} x^z$ .

In like manner from the sum of the serieses  $\frac{x}{e} + \frac{x^2}{e+1} + \frac{x^3}{e+2} + \&c. \frac{x}{f} + \frac{x^2}{f+1} + \frac{x^3}{f+2} + \&c. \frac{x}{g} + \frac{x^2}{g+1} + \frac{x^3}{g+2} + \&c. \&c.$  can be deduced the sum



of any series whose general term is

$$\frac{az^m + bz^{m-1} + \&c.}{z + e. z + e + 1. z + e + 2 \times \&c. \times z + f. z + f + 1. z + f + 2. \&c. \times z + g. z + g + 1. \&c.} \times x^2.$$

And also from the sum of the serieses  $\frac{1}{e} - \frac{1}{e+1}x + \frac{x^2}{e+2} - \&c. \frac{1}{f} - \frac{x}{f+1} + \frac{x^2}{f+2} - \&c. \frac{1}{g} - \frac{x}{g+1} + \frac{x^2}{g+2} - \&c. \&c.$  can be deduced the sum of any series whose general term is

$$\frac{az^m + bz^{m-1} + \&c.}{2z + e. 2z + e + 1. \&c. \times 2z + f. 2z + f + 1. \&c. 2z + g. 2z + g + 1. \&c.} \times x^2.$$

The method of adding more terms of a given series together, as before taught, may be applied to these and all other serieses. For example: let the given series be  $1 + \frac{1}{2}x + \frac{1}{2}x^2 + \frac{1}{4}x^3 + \&c.$ ; add two terms constantly together, and it becomes  $1 + \frac{1}{2}x + \&c. = \frac{2+x}{2} + \frac{4+3x}{3.4}x^2 + \frac{6+5x}{5.6}x^4 + \&c. = \frac{2+\Lambda}{2} + \frac{4+3\Lambda}{3.4}x^2 + \frac{6+5\Lambda}{5.6}x^4 + \&c.$  whence the general term is  $\frac{2z+2+(2z+1)}{2z+2} \frac{x}{2z+1} x^{2z}$ . From the methods before given of addition, subtraction, and multiplication; and the serieses found by this method, can be derived serieses, whose sums are known.

12. Suppose a given series  $ax^n + bx^{n\pm s} + cx^{n\pm 2s} + dx^{n\pm 3s} + \&c.$  whose sum  $p$  is either an algebraical, exponential, or fluent fluxion of  $x$ ; multiply the equation  $p = ax^n + bx^{n\pm s} + cx^{n\pm 2s} + dx^{n\pm 3s} + \&c.$  into  $x^{\pm r-n}$ , and there results  $ax^{\pm r-n}p = ax^{\pm r} + bx^{\pm r\pm s} + cx^{\pm r\pm 2s} + \&c.$ ; find the fluxion of this equation, and there follows  $\frac{1}{x}$  multiplied into the fluxion of the quantity  $(x^{\pm r-n}p) = \pm rax^{\pm r-1} + (\pm r\pm s)bx^{\pm r\pm s-1} + (\pm r\pm 2s)cx^{\pm r\pm 2s-1} + \&c.$  of which the general term is  $(\pm r\pm zs) \times t$ , where  $z$  denotes the distance from the first term of the series, and  $t$  is the term in the given series, whose distance from the first is  $z$ . In the same manner may be deduced the sum of a series whose general term is  $t' \times (\pm r\pm zs) \times (\pm r' \pm (z\pm ns))$ , or by repeated operations  $t' \times (ez^2 + fz + g)$ , where  $t'$  is a term of the given equation, whose distance from the first term is  $z$ . And in general, from the sum of a given series, whose fluxion can be found, and whose general term is  $t'$ , can be deduced by continued multiplication, and finding the fluxion, the sum of a series or quantity, of which the general term is  $\Lambda t'$ , where  $\Lambda$  is any function of the following kind  $a'z^m + b'z^{m-1} + c'z^{m-2} + \&c.$  in which  $z$  denotes the distance from the first term of the series, and  $m$  a whole number. It is to be observed, that if the given series converges in a ratio, which is at least equal to the ratio of the convergency of some geometrical series, the resulting equation will always converge. But if in a less ratio, then it will sometimes converge, sometimes not, according to the ratio which the successive terms of the resulting series have to each other at an infinite distance.



*Corol.*  $\frac{p \cdot p+1 \cdot p+2 \cdot p+3 \dots p+z}{r \cdot r+1 \cdot r+2 \cdot r+3 \dots r+z} = \frac{p+z \cdot p+z-1 \cdot p+z-2 \cdot p+z-3 \dots z+r+1}{r \cdot r+1 \cdot r+2 \cdot r+3 \dots p-1}$ , if  $p-r$  be a whole affirmative number; but this latter quantity has the formula above-mentioned  $ax^m + bx^{m-2} + cx^{m-3} + \&c.$ ; and consequently, if the sum of the series  $a + bx^r + cx^{2r} + dx^{3r} + \&c. = p$  be known, by this method can be deduced the sum of the series  $a + \frac{p}{r}bx^r + \frac{p \cdot p+1}{r \cdot r+1}cx^{2r} + \frac{p \cdot p+1 \cdot p+2}{r \cdot r+1 \cdot r+2}dx^{3r} + \&c.$

*Exam. 1.* Since  $(a+x)^{\frac{m}{n}} = a^{\frac{m}{n}}(1 + \frac{m}{n} \times \frac{x}{a} + \frac{m}{n} \times \frac{m-n}{2n} a^{-2}x^2 + \frac{m}{n} \cdot \frac{m-n}{2n} \cdot \frac{m-2n}{3n} a^{-3}x^3 + \&c.)$ ; multiply the successive terms of this series into the terms of the series  $1, \frac{p}{r}, \frac{p \cdot p+1}{r \cdot r+1}, \&c.$  and a series is deduced  $a^{\frac{m}{n}} + \frac{p \cdot m}{r \cdot n} a^{\frac{m}{n}-1} x + \frac{p \cdot p+1 \times m \cdot m-n}{r \cdot r+1 \cdot n \cdot 2n} x^{\frac{m}{n}-2} + \&c.$  whose sum is known, if the sum of the series  $= (a+x)^{\frac{m}{n}}$  is known.

*Exam. 2.* If the series begins from the  $l+1^{\text{th}}$  term of the above-mentioned binomial theorem  $a^{\frac{m}{n}} + \frac{m}{n} a^{\frac{m}{n}-1} x + \&c.$  viz. the series be  $H \times (1 + \frac{m-(l+1)n}{(l+2)n} \cdot \frac{x}{a} + \frac{m-(l+2)n}{(l+3)n} \cdot \frac{x^2}{a^2} + \frac{m-(l+3)n}{(l+4)n} \cdot \frac{x^3}{a^3} + \&c.)$ ; of which let the respective terms be multiplied into  $1, \frac{p}{r}, \frac{p \cdot p+1}{r \cdot r+1}, \&c.$  there will result a series whose sum is known.

*Exam. 3.* From the rule, first given by me, for finding the sum of the terms at  $h$  distances from each other, the sum of the series  $1 + \frac{m-(l+1)n}{(l+2)n} \times \frac{m-(l+2)n}{(l+3)n} \dots \frac{m-(l+h)n}{(l+h+1)n} \times \frac{x_h}{a^h} + p \times \frac{m-(l+h+1)n}{(l+h+2)n} \times \frac{m-(l+h+2)n}{(l+h+3)n} \dots \frac{m-(l+2h)n}{(l+2h+1)n} \frac{x^{2h}}{a^{2h}} + \&c.$  where  $p$  denotes the co-efficient of the preceding term, can be deduced; and consequently the sum of the series deduced from multiplying the successive terms of this series into the quantities  $1, \frac{p}{r}, \frac{p \cdot p+1}{r \cdot r+1}, \&c.$  respectively. The general principles of this case were first delivered by Mr. Bernoulli, Mr. de Moivre, Mr. Euler, &c.

12. Assume the series  $a + bx^r + cx^{2r} + \&c. = p$ ; multiply it into  $x^{a-1} \dot{x}$ , and find the fluent; then will  $\frac{1}{a} x^a p - \frac{1}{a} \int x^a \dot{p} = \frac{1}{a} ax^a + \frac{1}{a+n} bx^{a+n} + \frac{1}{a+2n} cx^{a+2n} + \&c.$ ; multiply this equation into  $x^{\beta-a-1} \dot{x}$ , and find the fluent of the equation resulting, which will be  $\frac{1}{\beta} \times \frac{1}{a} x^{\beta} p - \frac{1}{a} \cdot \frac{1}{\beta} \int x^{\beta} \dot{p} = \frac{1}{a} \times \frac{1}{\beta-a} x^{\beta-a} \int x^a \dot{p} + \frac{1}{a} \cdot \frac{1}{\beta-a} \int x^{\beta} \dot{p} = \frac{1}{a} \cdot \frac{1}{\beta} ax^{\beta} + \frac{1}{a+n} \cdot \frac{1}{\beta+n} bx^{\beta+n} + \frac{1}{a+2n} \cdot \frac{1}{\beta+2n} cx^{\beta+2n} + \&c.$ ; divide by  $x^{\beta}$ , and there results  $\frac{1}{\beta} \cdot \frac{1}{a} p + \frac{1}{a} \cdot \frac{1}{a-\beta} x^{-a} \int x^a \dot{p} +$



$\frac{1}{\beta} \cdot \frac{1}{\beta - \alpha} x^{-\beta} \int x^{\alpha} p = \frac{1}{\alpha} \cdot \frac{1}{\beta} a + \frac{1}{\alpha + n} \cdot \frac{1}{\beta + n} bx^n + \&c. : \text{ and in general } \frac{1}{\alpha} \cdot \frac{1}{\beta} \cdot \frac{1}{\gamma} \cdot \&c. p + \frac{1}{\alpha} \cdot \frac{1}{\alpha - \beta} \cdot \frac{1}{\alpha - \gamma} x^{-\alpha} \int x^{\beta} p + \frac{1}{\beta} \cdot \frac{1}{\beta - \alpha} \cdot \frac{1}{\beta - \gamma} \cdot \&c. x^{-\beta} \int x^{\gamma} p + \frac{1}{\gamma} \cdot \frac{1}{\gamma - \alpha} \cdot \frac{1}{\gamma - \beta} \cdot \&c. x^{-\gamma} \int x^{\delta} p = \frac{1}{\alpha} \cdot \frac{1}{\beta} \cdot \frac{1}{\gamma} \cdot \&c. a + \frac{1}{\alpha + n} \cdot \frac{1}{\beta + n} \cdot \frac{1}{\gamma + n} \cdot \&c. bx^n + \frac{1}{\alpha + 2n} \cdot \frac{1}{\beta + 2n} \cdot \frac{1}{\gamma + 2n} \cdot \&c. cx^{2n} + \&c. \text{ whence the law of continuation is immediately manifest.}$

Hence, if no two quantities  $\alpha, \beta, \gamma, \delta, \&c.$  be equal to each other; and the successive terms  $a, b, c, d, \&c.$  of any series  $a + bx^n + cx^{2n} + \&c. = p$  be divided by  $\alpha \cdot \beta \cdot \gamma \cdot \delta \cdot \&c. ; (\alpha + n) \cdot (\beta + n) \cdot (\gamma + n) \cdot (\delta + n) \cdot \&c. ; \alpha + 2n, \beta + 2n, \gamma + 2n, \delta + 2n, \&c. \&c. ;$  and in general by  $\alpha + nz, \beta + nz, \gamma + nz, \delta + nz, \&c. \&c. ;$  then can the sum of the series be found from the fluents of the fluxions  $x^{\alpha} p, x^{\beta} p, x^{\gamma} p, x^{\delta} p, \&c.$  as has been observed in the *Meditationes*. If two are equal, viz.  $\alpha = \beta$ , then also the fluent of the fluxion  $\frac{\dot{x}}{x} \int x^{\alpha} p$  is required. If three are equal, viz.  $\alpha = \beta = \gamma$ ; then it is necessary to find the fluent of the fluxion  $\frac{\dot{x}}{x} \int \frac{\dot{x}}{x} \int x^{\alpha} p$ ; and so on.

1. Let  $p = \frac{\dot{x}}{1 \pm x^n}$ ; and if the differences of the quantities  $\alpha, \beta, \gamma, \delta, \&c.$  are divisible by  $n$ , from the fluent of the fluxion  $x^{\alpha} p$  can be deduced the fluents of all the other fluxions  $x^{\beta} p, x^{\gamma} p, \&c. ;$  and in general, if  $\alpha - \beta$  is divisible by  $n$ , then from the fluent of the fluxion  $x^{\alpha} p$  can be deduced the fluent of the fluxion  $x^{\beta} p$ .

2. Suppose  $p =$  the terms of the binomial theorem expanded according to the dimensions of  $x$ , viz.  $(a + bx^n)^{\frac{r}{s}} = a^{\frac{r}{s}} + \frac{r}{s} a^{\frac{r}{s}-1} bx^n + \&c.$  beginning from the first or any other terms; then, if  $\alpha, \beta, \&c.$  divided by  $n$  give whole affirmative numbers, will all the fluxions  $x^{\alpha} p, x^{\beta} p, x^{\gamma} p, \&c.$  be integrable; and if the differences of the quantities  $\alpha, \beta, \gamma, \delta, \&c.$  are divisible by  $n$ , from the fluent of the fluxion  $x^{\alpha} p$  can be deduced the fluents of the fluxions  $x^{\beta} p, x^{\gamma} p, \&c.$  If  $p$  denotes the sum of the alternate or terms whose distance from each other are  $m$ , of the binomial theorem, the same may be applied.

3. If  $p = (a + bx^n + cx^{2n})^{\frac{r}{s}}$ ; and  $\alpha, \beta, \gamma, \delta, \&c.$  divided by  $n$  give whole affirmative numbers, then from  $\int x^{\alpha} p$  can be deduced all the remainder  $\int x^{\beta} p, \int x^{\gamma} p, \&c. : \text{ and in general from two can be deduced all the remainder. To find when the sum of any series of this kind can be found, add together each of the fluents, which can be found from each other, and not otherwise, and suppose their sum } = 0 ; \text{ and so of any other similar fluent, and from the resulting equations can be discovered when the series can be integrated.}$

13. If the general term of a series contains in it more variable quantities,  $z, v,$



$w$ , &c.; then find the sum of the series, first, from the hypothesis that one of them  $z$  is only variable, which, properly corrected, let be  $A$ ; in the quantity  $A$  suppose all the quantities invariable but some other  $v$ , and find the sum of the series thence resulting, which let be  $B$ , and so on; and the sum of the series will be deduced.

*Exam.* Let the term be  $\frac{1}{z \cdot z + 1 \times v \cdot v + 1 \cdot v + 2}$ ; the dimensions of  $z$  and  $v$ , &c. in the denominator must be at least greater than its dimensions in the numerator by a quantity greater than the number of the quantities  $z$ ,  $v$ , &c. which proceed in infinitum increased by unity. First, suppose  $z$  only variable, and the sum of the infinite series resulting will be  $\frac{1}{z \cdot v \cdot v + 1 \cdot v + 2} = A$ ; then suppose  $v$  only variable, and the sum resulting will be  $\frac{1}{2z \cdot v \cdot v + 1} = B$ , which is the sum required. If it be supposed, that the quantities  $z$  and  $v$ , &c. in the same term shall never have the same values, then suppose  $z$  and  $v$  always to have the same values, and the general term  $\frac{1}{z \cdot z + 1 \cdot v \cdot v + 1 \cdot v + 2}$  becomes  $\frac{1}{z^2 \cdot (z + 1)^2 \cdot (z + 2)^2}$ , of which let the sum be  $v$ , then will  $B - v$  be the sum required. On this and some other subjects more have been given in the *Meditationes*.

14. If the sum of the series cannot be found in finite terms, and it is necessary to recur to infinite series; it is observed in the *Meditationes* to be generally necessary to add so many terms together, that the distance from the first term of the series may considerably exceed the greatest root of an equation resulting from the general term made  $= 0$ ; and afterwards a series more converging may commonly be deduced from the fluents of fluxions resulting from neglecting all but the greatest quantities in the general terms resulting; and by other different methods. Mr. Nicholas Bernoulli and Mr. Monmort investigated the sum of the series (P)  $A + Br + Cr^2 + \&c.$  by a series (Q)  $\frac{A}{1-r} + \frac{d'r}{(1-r)^2} + \frac{d''r^2}{(1-r)^3} + \frac{d'''r^3}{(1-r)^4} + \&c.$ ; where  $d'$ ,  $d''$ ,  $d'''$ , &c. denote the successive differences of the terms  $A$ ,  $B$ ,  $C$ ,  $D$ , &c. If  $r$  be negative, the denominators become  $1+r$ ,  $(1+r)^2$ ,  $(1+r)^3$ , &c.

It has been observed, in the *Meditationes*, that in swift converging series the series P will converge more swiftly than the series Q; in series converging according to a geometrical ratio, sometimes the one will converge more swift, and sometimes the other. In other series, which converge more slowly, where most commonly  $r$  nearly  $= 1$ , it cannot in general be said, which of the serieses will converge the swiftest. The preceding remark, viz. the addition of the first terms of the series, is necessary in most cases of finding the sums by serieses of this kind.

It is not unworthy of observation, that in almost all cases of infinite series, the convergency depends on the roots of the given equations; which remark was first



published in the *Meditationes*. For example: in finding approximates to the roots of given equations, the convergency depends on how much the approximates given are more near to one root than to any other; and consequently, when 2 or more roots or values of an unknown quantity are nearly equal, different rules are to be applied, which are improvements of the rule of false. This rule, and the above-mentioned observations, were first given in the *Meditationes Algebraicæ et Analyticæ*, with several other additions on similar subjects.

Many more things concerning the summation of series, which depend on fluxional, &c. equations, might be added; but I shall conclude this paper with congratulating myself, that some algebraical inventions published by me have been since thought not unworthy of being published by some of the greatest mathematicians of this or any other age.

1st. In the year 1757, I sent to the Royal Society the first edition of my *Meditationes Algebraicæ*: they were printed and published in the years 1760 and 1762, with *Properties of Curve Lines*, under the title of *Miscellanea Analytica*, and a copy of them sent to Mr. Euler in the beginning of the year 1763, in which was contained a resolution of algebraical equations, not inferior, on account of its generality and facility, to any yet published, viz.  $y = a\sqrt[n]{p} + b\sqrt[n]{p^2} + c\sqrt[n]{p^3} + \dots \sqrt[n]{p^{n-1}}$ . This resolution was published by Mr. Euler in the *Petersburgh Acts* for the year 1764. Whether Mr. Euler ever received my book, I cannot pretend to say; nor is it material: for the fact is, that it was published by me in the year 1760 and 1762, and first by Mr. Euler in the year 1764. Mr. de la Grange and Mr. Bezout have ascribed this resolution to Mr. Euler, as first published in the year 1764, not having seen I suppose my *Miscell. Analyt.* Mr. Bezout found from it some new equations, of which the resolution is known, and applied it to the reduction of equations: more new equations are given, and the resolution rendered more easy by me in the *Philos. Trans.*

2d. In the above-mentioned *Miscell. Analyt.* an equation is transformed into another, of which the roots are the squares of the differences of the roots of the given equation; and it is asserted in that book, that if the co-efficients of the terms of the resulting equations change continually from + to - and - to +, the roots of the given equation are all possible, otherwise not; and in a paper, inserted by me in the *Philos. Trans.* for the year 1764, in which is found from this transformation, when there are none, 2 or 4 impossible roots, contained in an algebraical equation of 4 or 5 dimensions; it is observed, that there will be none or 4, &c. impossible roots contained in the given equation, if the last term be + or -; and 2, &c. on the contrary, if the last term be - or +. These observations and transformation have been since published and explained in the *Berlin Acts* for the years 1767 and 1768, by Mr. de la Grange.



3d. In the *Miscell. Anal.* an equation is transformed into another, whose roots are the squares, &c. of the roots of a given equation; and it is asserted, that there are at least so many impossible roots contained in the given equation, as there are continual progresses in the resulting equation from  $+$  to  $+$  and  $-$  to  $-$ . It is afterwards remarked, that these rules sometimes find impossible roots when Sir Isaac Newton's, and such like rules, fail; and that Sir Isaac Newton's, &c. will find them, when this rule fails. This rule may somewhat further be promoted by first changing the given equation, whose root is  $x$ , into another whose root is  $x\sqrt{-1}$ ; but, in my opinion, the rule of Harriot's, which only finds whether there are impossible roots contained in a cubic equation or not, is to be preferred to these rules, which, in equations of any dimensions, of which the impossible roots cannot generally be found from the rules, seldom find the true number.

4th. It is remarked, that rules which discover the true number of impossible roots require immense calculations, since they must necessarily find, when the roots become equal. In order to this, in the *Miscell. Anal.* there is found an equation, whose roots are the reciprocals of the differences of any two roots of the given equation; and from finding a quantity ( $\pi$ ) greater than the greatest root of the given, and  $(\frac{1}{A})$  greater than the greatest root of the resulting equation, and substituting  $\pi$ ,  $\pi - A$ ,  $\pi - 2A$ , &c. for  $x$  in the given equation; will always be found the true number of impossible roots.

5th. In the same book are assumed 2 equations ( $nx^{n-1} - (n-1)px^{n-2} + (n-2)qx^{n-3} - \&c. = 0$  and  $x^n - px^{n-1} + \&c. = w$ ), and thence deduced an equation whose root is  $w$ , from which, in some cases, can be found the number of impossible roots.

6. In the *Miscell. Anal.* is given the law of a series, and its demonstration, which finds the sum of the powers of the roots of a given equation from its coefficients. Mr. Euler has since published the same in the *Petersburg Acts*. Mr. De La Grange printed a property of this series, also printed by me about the same time; viz. that if the series was continued in infinitum, the powers would observe the same law as the roots, which indeed immediately follows from the series itself; but from thence with the greatest sagacity he deduces the law of the reversion of the series,  $y = a + bx + cx^2 + dx^3 + \&c.$ : it has since been given in a different manner from similar principles in the *Medit. Analyt.*

7. In the *Miscell. Analyt.* the law of a series is given for finding the sum of all quantities of this kind,  $\alpha^m \times \beta^n \times \gamma^r \times \delta^s \times \&c. + \&c.$  where  $\alpha$ ,  $\beta$ ,  $\gamma$ ,  $\delta$ , &c. denote the roots of a given equation, from the powers of the roots of the given equation. This law, with a different notation, has been since published in the *Paris Acts* by Mr. Vandermonde; who indeed mentions that he had heard, that a series for that purpose was contained in my book, but had not seen it. In the



same book is given a method of finding the aggregates of any algebraical functions of each of the roots of given equations, which is somewhat improved in the latter editions.

8. In the same book are assumed  $\frac{az^n + bz^{n-1} + \&c.}{pz^m + qz^{m-1} + \&c.}$  and  $\frac{Az^{n'} + Bz^{n'-1} + \&c.}{pz^m + qz^{m-1} + \&c.}$ , where  $z$  is any rational quantity whatever, for  $x$  and  $y$ , the unknown quantities of a given equation of two or more dimensions.

9. In the *Miscell. Analyt.* a biquadratic,  $x^4 + 2px^3 = qx^2 + rx + s$ , of which no term is destroyed, is reduced to a quadratic,  $x^2 + px + n = \sqrt{p^2 + 2n + qx} + \sqrt{s + n^2}$ ; and in the 2d edition of it, printed in the years 1767, 1768, 1769, and published in the beginning of the year 1770, the values of  $n$  are found  $\frac{\alpha\beta + \gamma\delta}{2}$ ,  $\frac{\alpha\gamma + \beta\delta}{2}$ , and  $\frac{\alpha\delta + \beta\gamma}{2}$ ; and the 6 values of  $\sqrt{y^2 + 2n + q}$  respectively  $\frac{\alpha + \beta - \gamma - \delta}{2}$ ,  $\frac{\alpha + \gamma - \beta - \delta}{2}$ ,  $\frac{\alpha + \delta - \beta - \gamma}{2}$ , and their negatives; and the six values of  $\sqrt{s + n^2}$  respectively  $\frac{\alpha\beta - \gamma\delta}{2}$ ,  $\frac{\alpha\gamma - \beta\delta}{2}$ ,  $\frac{\alpha\delta - \beta\gamma}{2}$ , and their negatives.

10. From a given biquadratic,  $y^4 + qy^2 + ry + s = 0$ , by assuming  $y^2 + ay + b = v$  and  $a$  and  $b$  such quantities as to make the 2d and 4th terms of the resulting equations to vanish, there results an equation,  $v^4 + Av^2 + B = 0$ , of the formula of a quadratic. Mr. De La Grange has ascribed this resolution to Mr. Tschirnhausen; but in the *Leipsic Acts* the resolution of a cubic is given by Mr. Tschirnhausen, but not of a biquadratic: his general design seems to be the extermination of all the terms.

11. Mr. Euler or Mr. De La Grange found, that if  $\alpha$  be a root of the equation  $x^n - 1 = 0$ , where  $n$  is a prime number, then  $\alpha$ ,  $\alpha^2$ ,  $\alpha^3$ , . . .  $\alpha^{n-1}$ , 1 will be  $(n)$  roots of it. More on a similar subject has been added in the last edition of the *Medit. Algebr.*

12. It is observed in the *Miscell. Analyt.* that Cardan's or Scipio Ferreus's resolution of a cubic is a resolution of 3 different cubic equations; and in the *Medit. Algebr.* 1770, the 3 cubics are given, and the rationale of the resolution (for example: if  $\alpha$ ,  $\beta$ , and  $\gamma$ , be the roots of the cubic equation  $x^3 + qx - r = 0$ , then is given the function of the above roots, which are the roots of the reducing equation  $z^6 - rz^3 = q^3$ ); and also the rationale of the common resolution of biquadratics.

13. It is asserted in the *Miscell.* that if the terms  $(my^n + by^{n-1}x + cy^{n-2}x^2 + \&c. \text{ and } Ny^m + By^{m-1}x + cy^{m-2}x^2 + \&c.)$  of two equations of  $n$  and  $m$  dimensions, which contain the greatest dimensions of  $x$  and  $y$ , have a common divisor, the equation whose root is  $x$  or  $y$ , will not ascend to  $n \times m$  dimensions; and if the equation, whose root is  $x$  or  $y$ , ascends to  $n \times m$  dimensions, the sum of its roots depends on the terms of  $n$  and  $n - 1$  dimensions in the one, and  $m$  and  $m - 1$  dimensions in the other equation, &c. It is also asserted in



the Miscell. that if 3 algebraical equations of  $n$ ,  $m$ , and  $r$  dimensions, contain 3 unknown quantities  $x$ ,  $y$ , and  $z$ , the equation, whose root is  $x$  or  $y$  or  $z$ , cannot ascend to more than  $n \cdot m \cdot r$  dimensions.

14. Mr. Bezout has given two very elegant propositions for finding the dimensions of the equation whose root is  $x$  or  $y$ , &c.; where  $x$ ,  $y$ , &c. are unknown quantities contained in two or more ( $h$ ) algebraical equations of  $\pi$ ,  $\rho$ , &c. dimensions, and in which some of the unknown quantities do not ascend to above the  $\pi$ ,  $\rho$ ,  $\sigma$ , &c. dimensions respectively. In demonstrating these propositions he uses one (among others) before given by me (viz. if an equation of  $n$  dimensions contains  $m$  unknown quantities, the number of different terms which may be contained in it will be  $(n + 1) \cdot \frac{n + 2}{2} \cdot \frac{n + 3}{3} \dots \frac{n + m}{m}$ ). In the Medit. 1770 there is given a method of finding in many cases the dimensions of the equation whose root is  $x$  or  $y$ , &c.; from which one, if not both, of the above-mentioned cases may more easily be deduced, and others added.

15. In the Medit. 1770 it is observed, that if there be  $n$  equations containing  $m$  unknown quantities, where  $n$  is greater than  $m$ , there will be  $n - m$  equations of conditions, &c.—16. In the Miscell. is given and demonstrated the subsequent proposition; viz. if two equations contain two unknown quantities  $x$  and  $y$ , in which  $x$  and  $y$  are similarly involved; the equation whose root is  $x$  or  $y$  will have twice the number of roots which the equation, whose root is  $x + y$ ,  $x^2 + y^2$ , &c. has. In the Medit. 1770 the same reasoning is applied to equations, which have 2, 3, 4, &c. quantities similarly involved.

17. Mr. De La Grange has done me the honour to demonstrate my method of finding the number of affirmative and negative roots contained in a biquadratic equation. A demonstration of my rule for finding the number of affirmative, negative, and impossible roots contained in the equation  $x^n + Ax^m + B = 0$  is also omitted, on account of its ease and length. From the Medit. the investigation of finding the true number of affirmative and negative roots appears to be as difficult a problem as the finding the true number of impossible roots; and it further appears, that the common methods in both cases can seldom be depended on. But their faults lie on different sides, the one generally finds too many, the other too few.

18. In the Medit. 1770, from the number of impossible roots in a given equation ( $x^n - px^{n-1} + \text{&c.} = 0$ ) is found the number of impossible roots in an equation, whose roots ( $v$ ) have any assignable relation to the roots of a given equation; and examples are given in the relation ( $nx^{n-1} - (n - 1)px^{n-2} + \text{&c.} = v$ ); and in an equation, whose roots are the squares of the differences of the roots of the given equation.—19. It is observed in the Medit. 1770, that in two or more equations, having two or more unknown quantities, the same



irrationality will be contained in the correspondent values of each of the unknown quantities, unless two or more values of one of them are equal, &c. The same observation is also applied to the co-efficients of an equation deduced from a given equation.—20. In the Miscell. was published a new method of exterminating, from a given equation, irrational quantities, by finding the multipliers which multiplied into it give a rational product.—21. In the Medit. 1770, are given the different resolutions of a certain quantity  $(a^2 + rb^2)^{2m+1}$  and  $(a^2 + rb^2)^{2m+2}$  into quantities of the same kind.—22. Mr. De La Grange has very elegantly demonstrated Mr. Wilson's celebrated property of prime numbers contained in my book. In the last edition of the Medit. the same property is demonstrated, and some similar ones added.

23. In the Miscell. is given a method of finding all the integral correspondent values of the unknown quantities of a given simple equation, having two or more unknown quantities; and, in the Medit. 1770, are given methods of reducing simple and other algebraical equations into one, so that some unknown quantities may be exterminated; and if the unknown quantities of the resulting equations be integral or rational, the unknown quantities exterminated may also be integral or rational.—24. In the Medit. are given rules for finding the different and correspondent roots of an equation, whose resolution is given.—25. Mr. De La Grange has recommended my new transformation of equations, published in the Miscell. which perhaps is not less general nor elegant than any yet published; and in the Meditat. 1770 is given a method very useful in finding the co-efficients.

If either here, or in the preface to the Medit. Algebraicæ, I have ascribed to myself any algebraical, or in the properties of curve lines any geometrical, or in the Medit. Analyt. any analytical invention, which has been before published by any other person, I can only plead ignorance of it, and shall on the very first conviction acknowledge it. I must further add, that I have been able to carry my algebraical improvements into geometry; for from them, with some geometrical principles added, I have, unless I am deceived, deduced as many new properties of conic sections and curve lines as have been published by any one since the great geometrician Apollonius.

*XXIX. Of a Remarkable Frost on the 23d of June, 1783. By the Rev. Sir John Cullum, Bart., F. R. S., and S. A. p. 416.*

About 6 o'clock, that morning, says Sir J. C., I observed the air very much condensed in my chamber-window; and on getting up was informed by a tenant, who lives near, that finding himself cold in bed, about 3 o'clock in the morning, he looked out at his window, and to his great surprize saw the ground covered with a white frost; and I was afterwards assured, on indubitable authority, that



two men at Barton, about 3 miles off, saw between 3 and 4 o'clock that morning, in some shallow tubs, ice of the thickness of a crown-piece, and which was not melted before 6.

This unseasonable frost produced some remarkable effects. The aristæ of the barley, which was coming into ear, became brown and withered at their extremities, as did the leaves of the oats; the rye had the appearance of being mildewed; so that the farmers were alarmed for those crops. The wheat was not much affected. The larch, Weymouth pine, and hardy Scotch fir, had the tips of their leaves withered; the first was particularly damaged, and made a shabby appearance the rest of the summer. The leaves of some ashes, very much sheltered in my garden, suffered greatly. A walnut-tree received a second shock (the first was from a severe frost on the 26th of May) which completed the ruin of its crop. Cherry-trees, a standard peach-tree, filberd and hasel-nut-trees, shed their leaves plentifully, and littered the walks as in autumn. The barberry-bush was extremely pinched, as well as the hypericum perforatum and hirsutum: as the last 2 are solstitial, and rather delicate plants, I wondered the less at their sensibility; but was much surprized to find that the vernal black-thorn and sweet violet, the leaves of which one would have thought must have acquired a perfect firmness and strength, were injured full as much. All these vegetables appeared exactly as if a fire had been lighted near them, that had shrivelled and discoloured their leaves. — penetrabile frigus adurit.

At the time this havock was made among some of our hardy natives, the exotic mulberry-tree was very little affected; a fig-tree, against a north-west wall, remained unhurt, as well as the vine, on the other side, though just coming into blossom. I speak of my own garden, which is high; for in the low ones about Bury, but a mile off, the fig-trees, in particular, were very much cut; and in general all those gardens suffer more by frost than mine.

*XXX. On a New Method of preparing a Test Liquor to show the Presence of Acids and Alkalis in Chemical Mixtures. By Mr. James Watt. p. 419.*

The syrup of violets was formerly the test of the point of saturation of mixtures of acids and alkalis, which was principally used; but since the late improvements in chemistry it has been found not to be sufficiently accurate, and the infusion of tournesol, or of an artificial preparation called litmus, have been substituted instead of it. The infusion of litmus is blue, and becomes red with acids. It is sensible to the presence of 1 grain of common oil of vitriol, though it be mixed with 100000 grains of water; but as this infusion does not change its colour on being mixed with alkaline liquors, in order to discover whether a liquor be neutral or alkaline, it is necessary to add some vinegar to the litmus, so as just to turn the infusion red, which will then be restored to its blue colour, by



being mixed with any alkaline liquor. The blue infusion of litmus is also a test of the presence of fixed air in water, with which it turns red, as it does with other acids.

The great degree of sensibility of this test would leave very little reason to search for any other, were there reason to believe that it is always a test of the exact point of saturation of acids and alkalis, which the following fact seems to call in question. I have observed, that a mixture of phlogisticated nitrous acid with an alkali will appear to be acid, by the test of litmus, when other tests, such as the infusion of the petals of the scarlet rose, of the blue iris, of violets, and of other flowers, will show the same liquor to be alkaline, by turning green so very evidently as to leave no doubt.

At the time I made this discovery, the scarlet roses and several other flowers, whose petals change their colour by acids and alkalis, were in flower. I stained paper with their juices, and found that it was not affected by the phlogisticated nitrous acid, except in so far as it acted the part of a neutralizing acid; but I found also, that paper, stained in this manner, was by no means so easily affected by acids of any kind as litmus was, and that in a short time it lost much of that degree of sensibility it possessed. Having occasion in winter to repeat some experiments, in which the phlogisticated nitrous acid was concerned, I found my stained paper almost useless. I was therefore obliged to search for some substitute among the few vegetables which then existed in a growing state; of these I found the red cabbage (*brassica rubra*) to furnish the best test, and in its fresh state to have more sensibility both to acids and alkalis than litmus, and to afford a more decisive test, from its being naturally blue, turning green with alkalis, and red with acids; to which is joined the advantage of its not being affected by phlogisticated nitrous acid any further than it acts as a real acid.

To extract the colouring matter, take those leaves of the cabbage which are freshest, and have most colour; cut out the larger stems, and mince the thin parts of the leaves very small; then digest them in water, about the heat of 120 degrees, for a few hours, and they will yield a blue liquor, which, if used immediately as a test, will be found to possess great sensibility. But as this liquor is very subject to turn acid and putrid, and to lose its sensibility, when it is wanted to be preserved for future use, the following processes succeed the best.

1. After having minced the leaves, spread them on paper, and dry them in a gentle heat; when perfectly dry, put them up in glass bottles well corked; and when you want to use them, acidulate some water with vitriolic acid, and digest, or infuse, the dry leaves in it till they give out their colour; then strain the liquor through a cloth, and add to it a quantity of fine whiting or chalk, stirring it frequently till it becomes of a true blue colour, neither inclining to green or purple; as soon as you perceive that it has acquired this colour, filter it immedi-



ately, otherwise it will become greenish by longer standing on the whiting. This liquor will deposit a small quantity of gypsum, and by the addition of a little spirit of wine will keep good for some days, after which it will become a little putrid and reddish. If too much spirit is added, it destroys the colour. If the liquor is wanted to be kept longer, it may be neutralized by means of a fixed alkali instead of chalk.

2. But as none of these means will preserve the liquor long without requiring to be neutralized afresh, just before it is used; and as the putrid and acid fermentation which it undergoes, and perhaps the alkalis or spirit of wine mixed with it, seem to lessen its sensibility; in order to preserve its virtues while it is kept in a liquid state, some fresh leaves of the cabbage, minced as before directed, may be infused in a mixture of vitriolic acid and water, of about the degree of acidity of vinegar; and it may be neutralized, as it is wanted, either by means of chalk, or of the fixed or volatile alkali. But it is necessary to observe, that if the liquor has an excess of alkali, it will soon lose its colour, and become yellow, from which state it cannot be restored; therefore care should be taken to bring it very exactly to a blue, and not to let it verge towards a green.\*

3. By the same process I have made a red infusion of violets, which, on being neutralized, forms at present a very sensible test; but how long it will preserve its properties I have not yet determined. Probably the coloured infusions of other flowers may be preserved in the same manner, by the antiseptic power of the vitriolic acid, so as to lose little of their original sensibility. Paper, fresh stained with these tests in their neutral state, has sufficient sensibility for many experiments; but the alum and glue which enter into the preparation of writing-paper seem in some degree to fix the colour; and paper which is not sized becomes somewhat transparent, when wetted, which renders small changes of colour imperceptible; so that where accuracy is required, the test should be used in a liquid state.†

XXXI. *An Account of a new Plant, of the Order of Fungi.‡* By T. Woodward, Esq. p. 423.

This extraordinary vegetable production arises from a volva, which is buried 6

\* Since writing the above, I have found, that the infusions of red cabbages, and of various flowers, in water acidulated by means of vitriolic acid, are apt to turn mouldy in the summer season, and also that the moulding is prevented by the addition of spirits of wine. The quantity of spirit necessary for this purpose I have not been able to ascertain; but I add it by little at a time, till the progress of the moulding is prevented.—Orig.

† I have found, that the petals of the scarlet rose, and those of the pink-coloured lychnis, treated in this manner, afford very sensible tests.—Orig.

‡ This plant is the *Lycoperdon phalloides* of the Gmelinian edition of the *Systema Naturæ*. Its specific character is thus given, viz. *Lycoperdon deflexum campanulatum, supra pulverulentum, calyptratum, subtus glabrum, liberum, stipite volvato.*



or 8 inches deep in dry sandy banks; and consequently it is extremely difficult to detect it in its earliest state. At its first appearance above ground, the powdery head is covered with a loose campanulated cap, which does not adhere by any of the smallest filaments; and which Mr. W. supposes to be the upper part of the volva, as both always appear ragged when taken up. When the plant is taken up immediately on its appearing above ground, the stem is about 6 or 8 inches long; and, as well as the volva, replete with mucilage, making it much heavier than when it has attained its full growth. The dust is now perfectly formed, and is dispersed by the slightest touch, or by the wind. A great alteration soon takes place, as it now proceeds very rapidly, and in a few days attains the summit of its growth, which is from 9 to 15 inches, more than half being generally buried in the ground. The stem becomes woody, though hollow, the bark still more ragged, and the whole plant much lighter, both volva and stem being now quite dry, and free from mucilage. The wind and showers soon disperse the greatest part of the dust; and at length the stalk appears with a naked, coriaceous, campanulated pileus, and considerably bleached, in colour and appearance not unlike a dry stalk of hemp. In this state some of them are now to be found, Aug. 28, 1783, with plants of this year rising near them.

Mr. Humphreys, of Norwich, who first found this very extraordinary plant, met with it only in the state last described, and without discovering the volva; so that no judgment of it could be formed. It has been taken by some persons for a decayed or abortive agaric; but that opinion could not be maintained by any one who had seen it in its recent state. Mr. W. first met with it in February or March 1783 in its dry and withered state; but as it was suspected to be a decayed agaricus procerus, he wished to examine the root carefully, in order to observe whether it was bulbous. The bulb of the agaricus procerus is scarcely hidden under the surface, and he was much surprized at the depth to which he was obliged to search for the root of this plant; at length however, removing the earth carefully to the depth of 7 or 8 inches, he met with it, and on raising the plant, he discovered the volva, which was so unlike the fugitive one of the agaric, that he was immediately convinced it must be something new. This plant agrees with the genus phallus in its volva, which has a double coat replete with mucilage; and its stipes crowned with a reflexed pileus. But it more nearly approaches the genus lycoperdon, by its head covered with a thick dust, contained in a substance of a spongy appearance, and by the form of the dust, which agrees perfectly with that of most of the true lycoperdons, when examined in the microscope. To this genus it must at present probably be referred, though the total want of an exterior coat prevents its agreeing with it so perfectly as it ought.



*XXXII. Experiments to Investigate the Variation of Local Heat. By James Six, Esq. p. 428.*

To investigate the variation of local heat, I made the following experiments. On Sept. 4, 1783, I placed thermometers in 3 different stations; one on the top of the high tower of Canterbury cathedral, about 220 feet from the ground; another at the bottom of the same tower, at about 110 feet; and a 3d in my own garden,\* not more than 6 feet from the ground. They were all carefully exposed to the open air in a shady northern aspect; the lowest was as little liable to be affected by the reflection of the sun's rays as the elevation would permit, the 2d still less, and the highest not at all. They thus remained in their several places, where I visited them daily for 3 weeks, and minuted down the greatest degree of heat and cold that happened each day and night in their respective stations, by a peculiar thermometer.

By these observations it appears that, notwithstanding some irregularities, the heat of the days at the lowest station always exceeded that at the middle, and still more the heat at the upper station. As in many instances the higher regions of the atmosphere have been found to be colder than the lower, and the thermometer in the garden was more liable to be heated by the reflection of the sun's rays from the earth than the upper ones, a difference of this kind might have been expected. But I was greatly surprized to find the cold of the night at the lowest, not only equal to, but very frequently exceeding the cold at the higher stations. As I wished to know, whether these variations would continue the same in the winter, when the weather was colder; and whether a thermometer, placed at some distance from the city, having an elevation equal to that on the top of the cathedral tower, would agree with it; on Dec. 19, 1783, I disposed the 3 thermometers in the following manner: one in my garden; one on the top of the high tower, as before; and the 3d on the top of St. Thomas's hill, about a mile distant from the city, where, at 15 feet from the ground, it was nearly level with that on the cathedral tower. The weather at this time proving cold, favoured the experiment; and I now found the several thermometers nearly agreeing with each other in the day-time; but in the night, the cold at the lower station exceeded the cold at the higher ones rather more than it did in the month of September, when the weather was warmer.

At the time of taking these thermometrical observations, I likewise noted the different dispositions of the atmosphere in other respects: such as the pressure, moisture, and dryness of the air; force and direction of the winds; quantity of rain; whether the appearances of the sky were clear or cloudy, &c. as I appre-

\* Situated not far from the cathedral, at the extremity of the buildings on the north side of the city.—Orig.



hended the local variation of the thermometers might, in a certain degree, correspond with some particular change in the state of the atmosphere. The event answered my expectation in a singular manner in respect to the nocturnal variation; for it generally happened, that when the sky was dark and cloudy, whatever was the condition of the atmosphere with regard to the other particulars above enumerated, the thermometers agreed pretty nearly with each other; but, on the contrary, whenever the sky became clear, the cold of the night at the lowest station in the garden constantly exceeded the cold at the top of the cathedral tower, where the instrument was placed 220 feet from the ground, entirely exposed to the open air, wind, dews, and rain, in a shady northern aspect.

The local variations in the day-time seemed to be regulated by the general degree of heat only, without being affected by any other particular disposition of the atmosphere, or the clearness or cloudiness of the sky, as the nocturnal variations were. In the month of September, when the glasses rose from  $60^{\circ}$  to  $70^{\circ}$ , the heat at the lower station constantly exceeded the heat at the upper station; and in some measure proportionably, as the weather was hotter.\* In December and January, when from below  $30^{\circ}$  they seldom rose to  $40^{\circ}$ , the local variation in the day-time nearly ceased, or was found in very small degrees inclining sometimes one way, sometimes the other.

That the clearness of the sky should contribute to the coolness of the air in the night, is not at all surprizing; but that, whenever the sky becomes clear, the cold should seem to arise from the earth, and be found in the greatest degree, as long as it continues clear, in the lowest situation, seems a little extraordinary: this however appeared to be the case, both in the warmer and in the colder weather, during the whole time these observations were taken. About noon, on the 3d of January, the sky becoming clear, the air got cooler; and going into my garden, about 8 in the evening, I perceived the surface of the ground, which had been wet by the rain in the forenoon, began to be frozen. Looking immediately at the thermometer, I saw the mercury at  $33\frac{1}{4}^{\circ}$ ; and observing a piece of wet linen hanging near the glass, not 5 feet from the ground, I took it into my hand, and found it not in the least frozen; by which it appeared, that the degree of cold which had frozen the surface of the ground, had not then ascended to the glass, nor to the linen, and consequently had not been communicated to the air 5 or 6 feet above the earth. The next day I found, as expected, a considerable local variation; the index for the cold of the night in the garden being at  $32^{\circ}$ , that on the hill being at  $35\frac{3}{4}^{\circ}$ , and that on the top of the tower at

\* As the heat at the lower station exceeded the heat at the upper ones, when the weather was hot; and equally so, whenever the sky was cloudy, as well as when it was clear; it appears, that the glass at the lower station was not materially affected by the reflection of the sun's rays from the earth, as at first I apprehended it would be.—Orig.



$37\frac{3}{4}^{\circ}$ . Probably the weather did not continue clear the whole night; if it had, it is likely the degrees of cold would have been found proportionably greater at every station. On the morning of the 4th there fell a misty rain, which continued only till noon, when the sky became clear again, and continued so till the 7th; during which time the nocturnal heights of the thermometers differed considerably from each other; but on the sky's becoming cloudy, the local variation ceased.

By experiments of this kind it may possibly in some measure be found, how far evaporations from the earth, at certain times, or vapours ascending, descending, or meeting, in different parts of the atmosphere, may increase or diminish the heat of the air in those places: or whether different degrees of heat and cold, subject however to change, may not be found in different strata of air, or vapour, floating in different parts of the atmosphere; or in what degree and proportion the cold increases at different altitudes, and in different seasons of the year; whether the cold, which is known to be very intense in the summer time on the tops of high mountains, receives a proportional increase, or be not less subject to variety by the return of winter and summer, night and day, than what we experience in the plains below.

*XXXIII. Of some Observations tending to Investigate the Construction of the Heavens. By Wm. Herschel, Esq., F. R. S. p. 437.*

In a former paper I mentioned that a more powerful instrument was preparing for continuing my reviews of the heavens. The telescope I have lately completed, though far inferior in size to the one I had undertaken to construct when that paper was written, is of the Newtonian form, the object speculum being of 20 feet focal length, and its aperture  $18\frac{7}{8}$  inches. The apparatus on which it is mounted is contrived so as at present to confine the instrument to a meridional situation, and by its motions to give the right ascension and declination of a celestial object in a coarse way; which however is sufficiently accurate to point out the place of the object, so that it may be found again.

Hitherto the sidereal heavens have, not inadequately for the purpose designed, been represented by the concave surface of a sphere, in the centre of which the eye of an observer might be supposed to be placed. It is true the various magnitudes of the fixed stars even then plainly suggested to us, and would have better suited, the idea of an expanded firmament of 3 dimensions; but the observations on which I am now going to enter still further illustrate and enforce the necessity of considering the heavens in this point of view. In future, therefore, we shall consider those regions into which we may now penetrate by means of such large telescopes, as a naturalist regards a rich extent of ground or chain of mountains, containing strata variously inclined and directed, as well as con-



sisting of very different materials. A surface of a globe or map therefore will but ill delineate the interior parts of the heavens.

It may well be expected, that the great advantage of a large aperture would be most sensibly perceived with all those objects that require much light, such as the very small and immensely distant fixed stars, the very faint *nebulæ*, the close and compressed clusters of stars, and the remote planets. On applying the telescope to a part of the *via lactea*, I found that it completely resolved the whole whitish appearance into small stars, which my former telescopes had not light enough to effect. The portion of this extensive tract, which it has hitherto been convenient for me to observe, is that immediately about the hand and club of Orion. The glorious multitude of stars of all possible sizes that presented themselves here to my view was truly astonishing; but as the dazzling brightness of glittering stars may easily mislead us so far as to estimate their number greater than it really is, I endeavoured to ascertain this point by counting many fields, and computing, from a mean of them, what a certain given portion of the milky way might contain. Among many trials of this sort I found, last January the 18th, that 6 fields, promiscuously taken, contained 110, 60, 70, 90, 70, and 74 stars each. I then tried to pick out the most vacant place that was to be found in that neighbourhood, and counted 63 stars. A mean of the first 6 gives 79 stars for each field. Hence, by allowing 15 minutes of a great circle for the diameter of my field of view, we gather, that a belt of 15 degrees long and 2 broad, or the quantity which I have often seen pass through the field of my telescope in 1 hour's time, could not well contain less than 50,000 stars, that were large enough to be distinctly numbered. But, besides these, I suspected at least twice as many more, which, for want of light, I could only see now and then by faint glittering and interrupted glimpses.

The excellent collection of *nebulæ* and clusters of stars which has lately been given in the *Connoissance des Temps* for 1783 and 1784, leads me next to a subject which indeed must open a new view of the heavens. As soon as the first of these volumes came to my hands, I applied my former 20-feet reflector of 12 inches aperture to them; and saw, with the greatest pleasure, that most of the *nebulæ*, which I had an opportunity of examining in proper situations, yielded to the force of my light and power, and were resolved into stars. For instance, the 2d, 5, 9, 10, 12, 13, 14, 15, 16, 19, 22, 24, 28, 30, 31, 37, 51, 52, 53, 55, 56, 62, 65, 66, 67, 71, 72, 74, 92, all which are said to be *nebulæ* without stars, have either plainly appeared to be nothing but stars, or at least to contain stars, and to show every other indication of consisting of them entirely. I have examined them with a careful scrutiny of various powers and light, and generally in the meridian. I should mention, that 5 of the above, viz. the 16th, 24, 37, 52, 67, are called clusters of stars containing *nebulosity*; but my instrument



resolving also that portion of them which is called nebulous into stars of a much smaller size, I have placed them into the above number. To these may be added the 1st, 3, 27, 33, 57, 79, 81, 82, 101, which in my 7, 10, and 20-foot reflectors showed a mottled kind of nebulosity, which I shall call resolvable; so that I expect my present telescope will perhaps render the stars visible of which I suppose them to be composed.

My present pursuits, as before observed, requiring this telescope to act as a fixed instrument, I found it not convenient to apply it to any other of the *nebulæ* in the *Connoissance des Temps* but such as came in turn; nor indeed was it necessary to take any particular pains to look for them, it being utterly impossible that any one of them should escape my observation when it passed the field of view of my telescope. The few which I have already had an opportunity of examining, show plainly that those most excellent French astronomers, Messrs. Messier and Mechain, saw only the more luminous part of their *nebulæ*; the feeble shape of the remainder, for want of light, escaping their notice. The difference will appear when we compare my observation of the 98th nebula with that in the *Connoissance des Temps* for 1784, which runs thus: “*Nébuleuse sans étoile, d’une lumière extrêmement foible, au dessus de l’aile boréale de la Vierge, sur le parallèle et près de l’étoile N° 6, cinquième grandeur, de la chevelure de Bérénice, suivant Flamsteed. M. Mechain la vit le 15 Mars, 1781.*” My observation of the 30th of December, 1783, is thus: A large, extended, fine nebula. Its situation shows it to be M. Messier’s 98th; but from the description it appears that that gentleman has not seen the whole of it, for its feeble branches extend above a quarter of a degree, of which no notice is taken. Near the middle of it are a few stars visible, and more suspected. My field of view will not quite take in the whole nebula. See fig. 2, pl. 8. Again, N° 53. “*Nébuleuse sans étoiles, decouverte au-dessous et près de la chevelure de Bérénice, à peu de distance de l’étoile quarante-deuxième de cette constellation, suivant Flamsteed. Cette nébuleuse est ronde et apparente, &c.*” My observation of the 170th sweep runs thus: A cluster of very close stars; one of the most beautiful objects I remember to have seen in the heavens. The cluster appears under the form of a solid ball, consisting of small stars, quite compressed into one blaze of light, with a great number of loose ones surrounding it, and distinctly visible in the general mass. See fig. 3.

When I began my present series of observations, I surmised, that several *nebulæ* might yet remain undiscovered, for want of sufficient light to detect them; and was therefore in hopes of making a valuable addition to the clusters of stars and *nebulæ* already collected and given in the work before referred to, which amount to 103. The event has plainly proved that my expectations were well founded: for I have already found 466 new *nebulæ* and clusters of stars, none



of which, to my present knowledge, have been seen before by any person; most of them indeed are not within the reach of the best common telescopes now in use. In all probability many more are still in reserve; and as I am pursuing this track, I shall make them up into separate catalogues, of about 2 or 300 at a time, and have the honour of presenting them in that form to the R. S.

A very remarkable circumstance attending the *nebulæ* and clusters of stars is, that they are arranged into strata, which seem to run on to a great length; and some of them I have already been able to pursue, so as to guess pretty well at their form and direction. It is probable enough, that they may surround the whole apparent sphere of the heavens, not unlike the milky way, which undoubtedly is nothing but a stratum of fixed stars. And as this latter immense starry bed is not of equal breadth or lustre in every part, nor runs on in one straight direction, but is curved and even divided into 2 streams along a very considerable portion of it; we may likewise expect the greatest variety in the strata of the clusters of stars and *nebulæ*. One of these nebulous beds is so rich, that in passing through a section of it, in the time of only 36 minutes, I detected no less than 31 *nebulæ*, all distinctly visible on a fine blue sky. Their situation and shape, as well as condition, seems to denote the greatest variety imaginable. In another stratum, or perhaps a different branch of the former, I have seen double and treble *nebulæ*, variously arranged; large ones with small, seeming attendants; narrow but much extended, lucid *nebulæ* or bright dashes; some of the shape of a fan, resembling an electric brush, issuing from a lucid point; others of the cometic shape, with a seeming nucleus in the centre; or like cloudy stars, surrounded with a nebulous atmosphere; a different sort again contain a nebulosity of the milky kind, like that wonderful, inexplicable phenomenon about  $\theta$  Orionis; while others shine with a fainter, mottled kind of light, which denotes their being resolvable into stars. See fig. 4, &c.

It is very probable, that the great stratum, called the milky way, is that in which the sun is placed, though perhaps not in the very centre of its thickness. We gather this from the appearance of the galaxy, which seems to encompass the whole heavens, as it certainly must do if the sun is within the same. For, suppose a number of stars arranged between two parallel planes, indefinitely extended every way, but at a given considerable distance from each other; and, calling this a sidereal stratum, an eye placed somewhere within it will see all the stars in the direction of the planes of the stratum projected into a great circle, which will appear lucid on account of the accumulation of the stars; while the rest of the heavens, at the sides, will only seem to be scattered over with constellations, more or less crowded, according to the distance of the planes or number of stars contained in the thickness or sides of the stratum.

Thus, in fig. 5, an eye at *s* within the stratum *ab*, will see the stars in the



direction of its length  $ab$ , or height  $cd$ , with all those in the intermediate situations, projected into the lucid circle  $ACBD$ ; while those in the sides  $mv$ ,  $nw$ , will be seen scattered over the remaining part of the heavens at  $MVNW$ . If the eye were placed somewhere without the stratum, at no very great distance, the appearance of the stars within it would assume the form of one of the less circles of the sphere, which would be more or less contracted to the distance of the eye; and if this distance were exceedingly increased, the whole stratum might at last be drawn together into a lucid spot of any shape, according to the position, length, and height of the stratum.

Let us now suppose that a branch, or smaller stratum, should run out from the former, in a certain direction, and let it also be contained between two parallel planes extended indefinitely onwards, but so that the eye may be placed in the great stratum somewhere before the separation, and not far from the place where the strata are still united. Then will this 2d stratum not be projected into a bright circle like the former, but will be seen as a lucid branch proceeding from the first, and returning to it again at a certain distance less than a semi-circle. Thus, in the same figure, the stars in the small stratum  $pq$  will be projected into a bright arch at  $PRRP$ , which, after its separation from the circle  $CBD$ , unites with it again at  $P$ . What has been instanced in parallel planes may easily be applied to strata irregularly bounded, and running in various directions; for their projections will of consequence vary according to the quantities of the variations in the strata and the distance of the eye from the same. And thus any kind of curvatures, as well as various different degrees of brightness, may be produced in the projections.

From appearances then, as before observed, we may infer, that the sun is most likely placed in one of the great strata of the fixed stars, and very probably not far from the place where some smaller stratum branches out from it. Such a supposition will satisfactorily, and with great simplicity, account for all the phenomena of the milky way, which, according to this hypothesis, is no other than the appearance of the projection of the stars contained in this stratum and its secondary branch. As a further inducement to look on the galaxy in this point of view, let it be considered, that we can no longer doubt of its whitish appearance arising from the mixed lustre of the numberless stars that compose it. Now should we imagine it to be an irregular ring of stars, in the centre nearly of which we must then suppose the sun to be placed, it will appear not a little extraordinary, that the sun, being a fixed star like those which compose this imagined ring, should just be in the centre of such a multitude of celestial bodies, without any apparent reason for this singular distinction; whereas, on our supposition, every star in this stratum, not very near the termination of its length or height, will be so placed as also to have its own galaxy, with only



such variations in the form and lustre of it, as may arise from the particular situation of each star.

Various methods may be pursued to come to a full knowledge of the sun's place in the sidereal stratum, of which I shall only mention one as the most general and most proper for determining this important point, and which I have already begun to put in practice. I call it gaging the heavens, or the star gage. It consists in repeatedly taking the number of stars in 10 fields of view of my reflector very near each other, and by adding their sums, and cutting off one decimal on the right, a mean of the contents of the heavens, in all the parts which are thus gaged, is obtained. By way of example, I have joined a short table, extracted from the gages contained in my journal, by which it appears, that the number of stars increases very fast as we approach the Via Lactea.

N. P. D. 92 to 94°.			N. P. D. 78 to 80°		
R. A.		Gage.	R. A.		Gage.
15	10 . . . .	9.4	11	16 . . . .	3.1
15	22 . . . .	10.6	12	31 . . . .	3.4
15	47 . . . .	10.6	12	44 . . . .	4.6
16	8 . . . .	12.1	12	49 . . . .	3.9
16	25 . . . .	13.6	13	5 . . . .	3.8
16	37 . . . .	18.6	14	30 . . . .	3.6

Thus, in the parallel from 92 to 94 degrees north polar distance, and R. A. 15<sup>h</sup> 10<sup>m</sup>, the star gage runs up from 9.4 stars in the field to 18.6 in about an hour and a half; whereas in the parallel from 78° to 80° north polar distance, and R. A. 11, 12, 13, and 14 hours, it very seldom rises above 4. We are however to remember, that with different instruments the account of the gages will be very different, especially on our supposition of the situation of the sun in a stratum of stars. For, let *ab*, fig. 6, be the stratum, and suppose the small circle *ghlk* to represent the space into which, by the light and power of a given telescope, we may penetrate; and let *GHLK* be the extent of another portion, which we are enabled to visit by means of a larger aperture and power; it is evident, that the gages with the latter instrument will differ very much in their account of stars contained at *MN*, and at *KG* or *LH*; when with the former they will hardly be affected by the change from *mn* to *kg* or *lh*. And this accounts for what a celebrated author says concerning the effects of telescopes, by which we must understand the best of those that are in common use. *M. De La Lande's Astron.* § 833.

It would not be safe to enter into an application of these, and such other gages as I have already taken, till they are sufficiently continued and carried all over the heavens. I shall therefore content myself with just mentioning that the situation of the sun will be obtained, from considering in what manner the star-gage agrees with the length of a ray revolving in several directions about an



assumed point, and cut off by the bounds of the stratum. Thus, in fig. 7, let  $s$  be the place of an observer;  $srrr$ ,  $srrr$ , lines in the planes  $rsr$ ,  $rsr$ , drawn from  $s$  within the stratum to one of the boundaries, here represented by the plane  $AB$ . Then, since neither the situation of  $s$ , nor the form of the limiting surface  $AB$ , is given, we are to assume a point, and apply to it lines proportional to the several gages that have been obtained, and at such angles from each other as they may point out; then will the termination of these lines delineate the boundary of the stratum, and consequently manifest the situation of the sun within the same. But to proceed.

If the sun should be placed in the great sidereal stratum of the milky way, and, as we have surmised above, not far from the branching out of a secondary stratum, it will very naturally lead us to guess at the cause of the probable motion of the solar system: for the very bright, great node of the Via Lactis, or union of the two strata about Cepheus and Cassiopeia, and the Scorpion and Sagittarius, points out a conflux of stars manifestly quite sufficient to occasion a tendency towards that node in any star situated at no very great distance; and the secondary branch of the Galaxy, not being much less than a semi-circle, seems to indicate such a situation of our solar system in the great undivided stratum as the most probable.

What has been said in a former paper on the subject of the solar motion, seems also to support this supposed situation of the sun; for the apex there assigned lies nearly in the direction of a motion of the sun towards the node of the strata. Besides, the joining stratum making a pretty large angle at the junction with the primary one, it may easily be admitted that the motion of a star in the great stratum, especially if situated considerably towards the side farthest from the small stratum, will be turned sufficiently out of the straight direction of the great stratum towards the secondary one. But I find myself insensibly led to say more on this subject than I am as yet authorized to do; I will therefore return to those observations which have suggested the idea of celestial strata.

In my late observations on nebulae I soon found, that I generally detected them in certain directions rather than in others; that the spaces preceding them were generally quite deprived of their stars, so as often to afford many fields without a single star in them; that the nebulae generally appeared some time after among stars of a certain considerable size, and but seldom among very small stars; that when I came to one nebula, I generally found several more in the neighbourhood; that afterwards a considerable time passed before I came to another parcel; and these events being often repeated in different altitudes of my instrument, and some of them at a considerable distance from each other, it occurred to me, that the intermediate spaces between the sweeps might also



contain nebulæ; and finding this to hold good more than once, I ventured to give notice to my assistant at the clock, "to prepare, since I expected in a few minutes to come at a stratum of the nebulæ, finding myself already (as I then figuratively expressed it) on nebulous ground." In this I succeeded immediately; so that I now can venture to point out several not far distant places, where I shall soon carry my telescope, in expectation of meeting with many nebulæ. But how far these circumstances of vacant places preceding and following the nebulous strata, and their being as it were contained in a bed of stars, sparingly scattered between them, may hold good in more distant portions of the heavens, and which I have not yet been able to visit in any regular manner, I ought by no means to hazard a conjecture. The subject is new, and we must attend to observations, and be guided by them, before we form general opinions.

Before concluding, I may however venture to add a few particulars about the direction of some of the capital strata or their branches. The well-known nebula of Cancer, visible to the naked eye, is probably one belonging to a certain stratum, in which I suppose it to be so placed as to lie nearest to us. This stratum I shall call that of Cancer. It runs from  $\epsilon$  Cancræ towards the south over the 67 nebula of the Connoissance des Temps, which is a very beautiful and pretty much compressed cluster of stars, easily to be seen by any good telescope, and in which I have observed above 200 stars at once in the field of view of my great reflector, with a power of 157. This cluster appearing so plainly with any good, common telescope, and being so near to the one which may be seen by the naked eye, denotes it to be probably the next in distance to that within the quartile formed by  $\gamma$ ,  $\delta$ ,  $\eta$ ,  $\theta$ ; from the 67th nebula the stratum of Cancer proceeds towards the head of Hydra; but I have not yet had time to trace it farther than the equator.

Another stratum, which perhaps approaches nearer to the solar system than any of the rest, and whose situation is nearly at rectangles to the great sidereal stratum in which the sun is placed, is that of Coma Berenices, as I shall call it. I suppose the Coma itself to be one of the clusters in it, and that, on account of its nearness, it appears to be so scattered. It has many capital nebulæ very near it; and in all probability this stratum runs on a very considerable way. It may perhaps even make the circuit of the heavens, though very likely not in one of the great circles of the sphere: for unless it should chance to intersect the great sidereal stratum of the milky way before-mentioned, in the very place in which the sun is stationed, such an appearance could hardly be produced. However, if the stratum of Coma Berenices should extend so far as (by taking in the assistance of M. Messier's and M. Mechain's excellent observations of scattered nebulæ, and some detached former observations of my own) I apprehend it may, the direction of it towards the north lies probably, with some



windings, through the great Bear onwards to Cassiopeia; thence through the girdle of Andromeda and the northern Fish, proceeding towards Cetus; while towards the south it passes through the Virgin, probably on to the tail of Hydra and the head of Centaurus. But though I have already fully ascertained the existence and direction of this stratum for more than 30 degrees of a great circle, and found it almost every where equally rich in fine nebulæ, it still might be dangerous to proceed in more extensive conjectures, that have as yet no more than a precarious foundation. I shall therefore wait till the observations in which I am at present engaged shall furnish me with proper materials for the disquisition of so new a subject. And though my single endeavours should not succeed in a work that seems to require the joint effort of every astronomer, yet so much we may venture to hope, that by applying ourselves with all our powers to the improvement of telescopes, which I consider as yet in their infant state, and turning them with assiduity to the study of the heavens, we shall in time obtain some faint knowledge of, and perhaps be able partly to delineate, the interior construction of the universe.

*XXXIV. An Account of a new Species of the Bark Tree,\* found in the Island of St. Lucia. By Mr. George Davidson. Dated St. Lucia, July 15, 1783. p. 452.*

It is now about 4 years since Mr. Alexander Anderson discovered in the woods, near the Grand Cul de Sac, some trees resembling, in the botanical characters, the true *Quinquina* of Linnæus. He brought the bark, flowers, and seeds, to Dr. Young of the General Hospital, and trial was made of it there; but not being sufficiently dried, its strong emetic and purgative qualities prevented its exhibition. The publication of Dr. Saunders, received here about 2 months ago, mentioning the introduction of a species of bark of a redder colour, and possessing greater powers than the bark formerly in use, induced us here to try the bark of this country. Dr. Young had by him some that was collected in general Grant's time: on account of the length of time it had been kept, and its being sufficiently dried, he has met with all the success he could wish. It is manifestly more astringent than the bark, and the bitter is also more durable on the palate. Hitherto I have generally used the cold infusion, either in lime or simple water, in the proportion of 1 oz. to 3 pints of the water. I have also given it in substance from 20 to 30 grains; but never exceeded the last quantity; for I never found the stomach able to retain more than 20 grains. Joined with the *canella alba*, it forms in spirits an agreeable and elegant tincture. I have

\* *Cinchona floribunda*. *C. foliis ellipticis acuminatis glabris, floribus paniculatis, capsulis turbinatis lævibus.* *Lin. Syst. Nat. Gmel.*



made a tincture from the seeds, which are infinitely stronger in taste than the bark itself.

*Mr. Geo. Davidson's account of the bark-tree of the island of St. Lucia.*—The bark-tree of this island (St. Lucia) is nearly about the size of the cherry-tree, seldom thicker than the thigh, and tolerably straight; the wood is light and porous, without any of the bitterness and astringency of the bark itself. It delights in a shady situation, the north-west aspect of hills, under larger trees; and is generally to be found about the middle of a hill, near some running water. The leaves are large, oblong, opposite, and plain, preserving, as well as the flowers and seeds, the bitter taste of the bark.

In the beginning of the rainy season (June,) the tree puts forth its flowers in small tufts; at first they are white, but afterwards turn purplish. The stamina are 5 in number, with a single style. The germen is oblong, bilocular, and furrowed on each side. The seeds are many, and of the winged kind. The corolla is monopetalous, with its mouth divided into 5 long segments.

The soil in general where it grows is a stiff red clay. The bark itself is of a lighter red than that sent out here to the hospital under the name of red bark. It inclines more to the colour of cinnamon. The bitterness and astringency appear to be greater than in either of the other barks. Infused in cold water, in which form, or in lime-water, I generally use it, it forms a very red tincture, possessing the bitterness and astringency of the bark very strongly. A few drops of the *tinctura florum martialium* give it a very black colour, and occasion a copious deposition of a black sediment. It does the same with the spirituous tincture. With spirits it forms a beautiful red tincture.

*XXXV. An Observation of the Meteor of Aug. 18, 1783, on Hewit Common near York. By N. Pigott, Esq. F. R. S. p. 457.*

August 18, about 10 o'clock P. M. after a hot day, the weather a little hazy, but not so as to obliterate the stars, and no wind, being on horseback, in company with 2 other gentlemen, on Hewit Common, about 3 miles from York, my attention was attracted towards the W. N. W. by several faint flashes of lightning, such as are often seen near the horizon, or which may be still better compared to flashes of an aurora borealis. Soon after which I perceived some luminous matter in motion, and collecting together from several directions, fig. 8, A, pl. 8, which immediately taking fire presented itself under the form of a ball, of so vivid a brightness, that the whole horizon was illuminated, so that the smallest object might have been seen on the ground. This ball, when formed, began to move, with an easy sliding motion from W. N. W. towards the S. S. E. It suggested the idea of a highly brilliant comet, emitting a train or tail, but of a different colour from the ball itself, this last being of a most bril-



liant bluish white, and the tail of a dusky red, the length of which appeared to extend over  $15^\circ$  or more, of the heavens, B. The apparent diameter of the nucleus seemed  $\frac{1}{3}$  or  $\frac{1}{4}$  of the full moon's diameter. The greatest difficulty in this estimation hence arises, that I cannot, notwithstanding all my endeavours, represent in my mind the moon otherwise than as a plane or disc; nor the meteor, than as a spherical body. Its altitude, when it formed in the w. n. w. was about  $30^\circ$ ; and about  $19^\circ$  or  $20^\circ$  above the horizon, when it became extinct in the s. s. e. a few sparks of the tail, nearest the nucleus, scattering themselves much in the same manner as those of a sky-rocket when burnt out, c.

It has been said, that the ball divided itself into 3 or 4 parts before its extinction. To me it appeared to vanish or gently die away: what confirms me in the opinion, that it did not divide, is, that the 3 or 4 scattering parts above-mentioned were not of the bright colour of the ball itself, but of the dusky red which the tail invariably showed. The interval of time from the meteor's formation to its extinction was nearly 20 seconds, perhaps 2 or 3 seconds less. The long habit I have of counting seconds in astronomical observations induces me to think this quantity may be relied on; and this I mention, because some have estimated it more, some less. Nine or 10 minutes after its dissipation, I heard a noise, much resembling the report of a cannon at a very great distance; but I would not wish to have it understood, that I speak to this last interval with the same certainty as to the other; if however it be exact, and supposing sound to move 1106 feet in one second of time, and the same in the upper regions of the atmosphere as here below, which however may be very different, its distance from me, at its extinction, must have been about 120 miles, and its perpendicular altitude above the earth's surface about 40 miles.

XXXVI. *Observations of the Comet of 1783. By Edw. Pigott, Esq. p. 460.*

Dates.	Apparent time.	R. A.	North declinat.	Greatest error of each R. A.	Longit.	Latitude.
1783	h m	° ' "	° ' "	' "	s. ° ' "	° ' "
November 19	11 4	41 1 30	3 11 $\frac{1}{2}$	3 00	1 9 37	12 4 $\frac{1}{2}$ s.
20	10 55—	40 0 3	4 32 $\frac{3}{4}$	0 22	1 9 2 $\frac{2}{3}$	10 31
22	6 52+	38 21 10	6 50	0 30	1 8 11 $\frac{1}{4}$	7 50
24	10 24—	36 29 28	9 36 $\frac{1}{4}$	0 15	1 7 19	4 52 $\frac{1}{4}$
26	10 9—	34 49 20	12 3 $\frac{1}{4}$	0 15	1 6 33 $\frac{1}{3}$	2 6
December 3	15 54	29 21 59	20 15 $\frac{1}{4}$	0 40	1 4 24	7 42 $\frac{1}{2}$ N.

The comet had exactly the appearance of a nebula: its light was so faint that it could not be seen in a good opera glass. In the night telescope the nucleus was scarcely visible, and the diameter of the surrounding coma was about 3' of a degree. Between Nov. 19 and 26 it rather diminished in brightness. Dec. 1st and 3d it was very difficult to be seen, occasioned perhaps by its little eleva-



tion above the horizon. Between Dec. 3 and 10 the comet was entirely effaced by the increased light of the moon. On the 10th, the moon being in the horizon did not obliterate stars of the 8th or 9th magnitude; but he could not find the comet.

The following observations were made by Mr. John Goodricke.

Dates.	Apparent time.		R. A.	North declination.	Longitude.	Latitude.
1783	h.	m.	° ' "	° ' "	s. ° ' "	° ' "
Novemb. 24	8	16	36 32 57	9 30 $\frac{3}{4}$	1 7 21	1 42 $\frac{1}{2}$ S.
28	6	8 $\frac{1}{2}$	33 20 0	14 16 $\frac{1}{2}$	1 5 55 $\frac{1}{2}$	0 52 $\frac{1}{4}$ N.

*XXXVII. Experiments on Mixing Gold with Tin.\* By Mr. Stanesby Alchorne, of the Mint. p. 463.*

It is a generally received opinion among metallurgists, that tin has a property of destroying the ductility of gold, on being melted with it, even in very small quantities. The late Dr. Lewis, in his *Phil. Commerce of Arts*, p. 85, has well expressed the sense of most writers on this subject, in the following words: "The most minute proportion of tin and lead," says he, "and even the vapours which rise from them in the fire, though not sufficient to add to the gold any weight sensible in the tenderest balance, make it so brittle that it flies in pieces under the hammer." Divers circumstances, however, says Mr. A. long since induced me to disbelieve the fact; but these, having chiefly arisen from small experiments, did not seem to warrant any general conclusion. But a late public occasion, which led me to various trials of mixing these metals together, in different proportions, and in sufficiently large quantities, has put the matter out of doubt; and shown me, that tin, in small quantity at least, may be added to gold, either pure or alloyed, without producing any other effect than what might easily be conceived, *à priori*, from the different texture of the two metals. In confirmation of which, I beg leave to lay some of the experiments before you.

*Exper. 1.* Sixty Troy grains of pure tin were stirred into 12 oz. of refined gold, in fusion; and the mixture was then cast into a mould of sand, producing a flat bar, 1 inch wide, and  $\frac{1}{8}$  of an inch thick. The bar appeared sound and good, suffered flattening under the hammer, drawing several times between a large pair of steel rollers, and cutting into circular pieces, of near an inch diameter, which bore stamping in the money-press, by the usual stroke, without showing the least sign of brittleness; or rather with much the same ductility as pure gold.

*Exper. 2.* Ninety grains of like tin were added to 12 oz. of fine gold, stirred, and cast as above. The bar produced was scarcely distinguishable from the former, and bore all the operations, as before-mentioned, quite as well.

\* This subject has been further prosecuted by Mr. Hatchett in the *Philos. Trans.* for 1783.



*Exper. 3.* 120 grains of fine tin were mixed with 12 oz. of fine gold, and being cast like the foregoing, produced a bar rather paler and harder than the preceding, but which suffered the like operations very well; except that, on drawing between rollers, the outer edges were disposed to crack a little.

*Exper. 4.* 140 grains, or half an ounce, of the like grained tin, were mixed, as before, with 12 oz. of fine gold; and the bar resulting from this mixture was completely sound and good; evidently paler and harder, however, than any of the foregoing, and cracking rather more than the last on passing between the rollers; but bearing every other operation, even stamping under the press, by the common force, without any apparent injury.

*Exper. 5.* One ounce of tin was next stirred into 12 oz. of the like refined gold, and then cast as before; but the bar produced, though seemingly solid and good, was bad coloured, brittle in texture, and, on the first passing between the rollers, split into several pieces, so that no further trials were made with it.

*Exper. 6.* To inquire how far the fumes of tin, brought into contact with the gold, would do more than mixing the metal in substance, a small crucible, filled with 12 oz. of standard gold,  $\frac{11}{12}$  fine, was placed in a larger crucible, having 1 oz. of melted tin in it, and kept there in fusion, the whole being covered with another large inverted crucible, for about half an hour. In this time a full quarter part of the tin was calcined; but the gold remained unaltered, and equally capable of being manufactured as another portion of the same gold melted in the common manner.

It may well be asked, whether the tin, or part of it, in every trial, might not be destroyed, and thus render the conclusions fallacious? But as, in any of these experiments, not more than 6 or 8 grains of the original weight were missing after the casting, and as even fine gold can scarcely be melted without some loss in the operation, so we may reasonably suppose, that our small losses, in the foregoing trials, do not deserve consideration. The above experiments then seem to show, that tin is not so mischievous to gold as hath been generally represented. But it would be unfair to infer, that the original author of this doctrine, from whom so many have implicitly transcribed, had no foundation for the assertion. Gold and tin indeed are substances pretty well known; but it is easy to imagine, that coins or trinkets may have been used for one, and impure tin, or pewter perhaps, for the other; and it is difficult to guess what might be the result of such uncertain combinations. To inquire further, therefore, the experiments were continued as follows.

*Exper. 7.* To determine whether the two metals might be more intimately combined, and the mass rendered brittle, by additional heat; the mixture of gold and tin, produced in the first of these experiments, was re-melted in a stronger fire than before, and thus kept in fusion full half an hour. By this



operation 6 grains only were lost in the weight; and the bar obtained was no less manufacturable than at first.

*Exper. 8 and 9.* The mixtures of gold and tin, from the 2d and 4th experiments, were re-melted separately, and 1 oz. of copper added to each. Being both well stirred, they were cast as usual; and the bars, though sensibly harder, bore all the operations of manufacturing as before. The last bar cracked a little at the edges, on drawing through the rollers, as it had done without the copper, but not materially, and bore cutting rather better than in its former state.

*Exper. 10 and 11.* A quarter of an ounce of the last mixture (being tin  $\frac{1}{4}$  an oz. and copper 1 oz. with gold 12 oz.), and as much of the bar from the 3d experiment (being tin 120 grs. with gold 12 oz.) were each melted by a jeweller, in the most ordinary manner, with a common sea-coal fire, into small buttons, without any loss of weight. These buttons were forged by him into small bars, nealing them often by the flame of a lamp, and afterwards drawn each about 20 times through the apertures of a steel plate, into fine wire, with as much ease as coarse gold commonly passes the like operation.

*Exper. 12.* To inquire whether the adding of tin to gold, already alloyed, would cause any difference, 60 grs. of tin were stirred into 12 oz. of standard gold,  $\frac{1}{12}$  fine; and the result passed every operation before described, without showing the least alteration from the tin.

For greater certainty, several other trials were made, of different mixtures of copper, tin, and silver, with gold, even so low as  $2\frac{1}{2}$  oz. of copper, with  $\frac{1}{4}$  an oz. of tin, to 12-oz. of gold. But these are not worth particularizing; for they all bore hammering, and flatting by rollers, to the thinness of stiff paper, and afterwards working into watch-cases, cane-heads, &c. with great ease. They all, indeed, became more hard and harsh, in proportion to the quantity of alloy; but not one of them had the appearance of what all workmen well know by the name of brittle gold. Whence it should seem, that neither tin in substance, nor the fumes of it, tend much to render gold unmanufacturable. Whenever therefore brittleness has followed the adding small quantities of tin to fine gold, it must be supposed to have arisen from some unfriendly mixture in the tin, probably from arsenic; for other experiments have shown me, that 12 grs. of regulus of arsenic, injected into as many oz. of fine gold, will render it totally unmalleable. From the foregoing experiments I presume we may fairly conclude, that though tin, like other inferior metals, will contaminate gold, in proportion to the quantity mixed with it, yet there does not appear any thing in it specifically inimical to this precious metal. And this being contrary to the doctrine of most chemical writers, I submit whether it may not be useful to publish these experiments, by laying them before the R. S.



*XXXVIII. On the Means of Directing Aerostatic Machines. By the Count de Galvez. From the French. p. 469.*

This project was conceived from the manner in which birds employ their wings and tails in flying, and fishes their fins and tails in swimming. The inventor explained his idea by a peculiar disposition of sails in a boat, 25 feet long, on a canal, by which it was made to sail against the current of the water. But it does not appear that it has ever been tried or succeeded in an air-balloon.

*XXXIX. An Extraordinary Case of a Dropsy of the Ovarium, with some Remarks. By Mr. Philip Meadows Martineau, Surgeon. p. 471.*

Sarah Kippus, a pauper in the city of Norwich, was for many years, says Mr. M., a patient of my father's, and at his decease was under the care of Mr. Scott, as city surgeon, who obliged me many times by taking me to the poor woman, from whom Mr. M. received the account of the early part of her disease. Her complaints came on first after a miscarriage at the age of 27. She had never been pregnant before; and her discharges at that time were so great as to bring her into a very weak condition. She soon perceived some uneasiness, attended with a swelling, on one side, which after a few months became too large to distinguish whether it was greater on one side or the other. As the swelling was found to arise from water, it was drawn off, which was in the year 1757. She was never afterwards pregnant; but the catamenia continued regularly till the usual period of their cessation. When he first saw her, which was in the year 1780, she had been many times tapped, and she was then full of water. Her appearance was truly deplorable, not to say shocking. She was rather a low woman, and her body so large as almost wholly to obscure her face, as well as every other part of her: with all she was tolerably cheerful, and seldom regarded the operation. He saw her just before they took away 106 pints of water, and he begged leave to take a measure of her. She was  $67\frac{1}{2}$  inches in circumference, and from the cartilago ensiformis to the os pubis 34 inches. Her legs were then greatly swelled; but this, and every other symptom of which she complained, evidently arose from the quantity and weight of water. She neither ate nor drank much, and made but a small quantity of urine.

The operation of drawing off the water was generally performed on a Sunday, as the most convenient day for her neighbours to assist her, and before the latter end of the week she was able to walk very well. She was first tapped in the year 1757, and died in August 1783. Thus she lived full 25 years with some intervals of ease, having 80 times undergone the operation, and in all had taken from her 6631 pints of water, or upwards of 13 hogsheads. Mr. M. subjoined the account of the dates, and the quantity drawn off at each time, as given him by



Mr. Scott, observing that till 1769 no exact memorandum was kept, except of the number of times, though the quantity of water drawn off was always measured. By his father she was tapped 26 times, averaged at 70 pints each time: by Mr. Donne once, 73 pints, which makes 1683 pints from some part of the year 1757 to 1769. By Mr. Scott 3, 4, or 5 times in each year, from 1769 till 1783, amounting in all to 6631 pints.

It appears, that 108 pints was the greatest quantity ever taken away at any one time; that she was never tapped more than 5 times in any 1 year; and the largest quantity in a year was 495 pints. The most collected in the shortest space of time was 95 pints in 7 weeks, from July 24th to September 10th in 1780, which is very nearly 2 pints a day. It appears also, that in the last 14 years of her life, when a regular account was kept, she increased faster in the winter than in the summer months. If the 6 summer months from April to Sept. inclusive are reckoned, she lost in the 14 years in 23 operations 1972 pints, and in the winter months from Oct. to March inclusive, by 30 tapplings, 2596 pints; and it will be found, that 30 is to 2596 rather more than 23 to 1972, so that 7 more tapplings were at least necessary in the winter than in the summer. In the months of March and Nov. she oftener underwent the operation than in any other. In these calculations the 3 months in 1783 are not included, as the year was not finished.

If this case be compared with the famous case of Lady Page, related by Dr. Mead, the quantity of water taken from her ladyship appears small when opposed to the number of pints drawn from Sarah Kippus. The one lost 1920, the other 6631. It must be confessed, however, that Lady Page collected faster than the poor woman whose case is here related. Mr. M. next gives an account of the dissection, and makes some observations on the whole. On the 10th of August 1783, the woman died; and the following day Dr. Dack, an eminent physician of this place, accompanied him to open the body. Mr. M. first drew off 78 pints of clear water: supposing therefore all the water to have been taken away at the last operation, then in 3 weeks she had collected 78 pints, which is more than  $3\frac{1}{2}$  pints in each day: a quantity far exceeding what she had taken. Mr. M. then opened into the cavity from which the water came, and separated the sac from the peritoneum, and found the sac had arisen in the ovarium of the left side. After this, he dissected out the uterus, with the right ovarium in a natural state, and thus obtained every part necessary to show the disease, viz. the uterus, the right ovarium sound, and the left enlarged into an immense pouch. The cyst itself was not very thick, but lined in almost every part of it, but more especially in the fore part, with small ossifications. The peritoneum was prodigiously thickened, and thus, by its additional strength, became the chief support of the water. There was something singular in the sac itself, for it was rather 2 than



one, from there being an opening in the side of what appeared at first the only cavity, which led to another cavity, almost equally large with the first, so that if all the water in any operation had not been evacuated, it must probably have been owing to a difficulty in its passage from the 2d into the first or more external cyst. From the size, however, of the woman after each operation, it is evident that there being 2 sacs did not prevent the total drawing off of the water. The other viscera appeared all in a natural state. The intestines were quite empty, and pushed up under the ribs, so as to have left but very little room for the expansion of the lungs within the thorax. The bladder was contracted, or rather it appeared lessened. The kidneys were healthy, and both ureters in a natural state. The sac is in the collection of John Hunter, esq.

In reflecting on this case, an obvious question arises; whence proceeded this immense collection of water? At different periods of this woman's life the quantity drawn off, without considering the urine she made, was much greater than the fluids she drank, which appeared from measuring whatever she took. It appears then pretty certain, that this superabundant quantity must have been taken into the body by absorption; and if we allow the bodies of animals to have this power of absorbing, which we very well know vegetables are possessed of, it will account for many appearances in the animal economy. Hence also appears the reason why this woman collected faster in the wet moist months of winter, than in summer. From all, this happy conclusion may be drawn, that though human art is at present insufficient to the perfect cure of diseases similar to this woman's case, yet nature is continually defending herself from sudden death; and such relief may be granted as to protract life a long time without much pain, and often with intervals of great ease and comfort.

*XL. Method of finding Curve Lines from the Properties of the Variation of the Curvature. By Nicholas Landerbeck, Adjunct Professor of Mathematics at Upsal. p. 477.*

This is a 2d part to the paper on this subject noticed in p. 456 of this vol.; but omitted for the same reason as there given.

END OF VOL. SEVENTY-FOUR OF THE ORIGINAL.

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*I. Of an Artificial Spring of Water. By Erasmus Darwin, M. D. F. R. S. Vol. 75, Anno 1785. p. 1.*

Near my house, says Dr. D., was an old well, about 100 yards from the river Derwent in Derby, and about 4 yards deep, which had been many years disused



on account of the badness of the water, which I found to contain much vitriolic acid, with at the same time a slight sulphureous smell and taste ; but did not carefully analyse it. The mouth of this well was about 4 feet above the surface of the river ; and the ground, through which it was sunk, consisted of a black, loose, moist earth, which appeared to have been very lately a morass, and is now covered with houses built on piles. At the bottom was found a bed of red marl, and the spring, which was so strong as to give up many hogsheads in a day, oozed from between the morass and the marl: it lay about 8 feet beneath the surface of the river, and the water rose within 2 feet of the top of the well.

Having observed that a very copious spring, called Saint Alkmund's well, rose out of the ground about half a mile higher on the same side of the Darwent, the level of which I knew by the height of the intervening wier to be about 4 or 5 feet above the ground about my well ; and having observed that the higher lands at the distance of a mile or 2 behind these wells, consisted of red marl like that in the well ; I concluded, that, if I should bore through this stratum of marl, I might probably gain a water similar to that of St. Alkmund's well, and hoped that at the same time it might rise above the surface of my old well to the level of St. Alkmund's. With this intent a pump was first put down for the purpose of more easily keeping dry the bottom of the old well, and a hole about  $2\frac{1}{4}$  inches diameter was then bored about 13 yards below the bottom of the well, till some sand was brought up by the auger. A wooden pipe, which was previously cut in a conical form at one end, and armed with an iron ring at the other, was driven into the top of this hole, and stood up about 2 yards from the bottom of the well, and being surrounded with well-rammed clay, the new water ascended in a small stream through the wooden pipe. Our next operation was to build a wall of clay against the morassy sides of the well, with a wall of well-bricks internally, up to the top of it. This completely stopped out every drop of the old water ; and, on taking out the plug which had been put in the wooden pipe, the new water in 2 or 3 days rose up to the top, and flowed over the edges of the well.

Afterwards, to gratify my curiosity in seeing how high the new spring would rise, and for the agreeable purpose of procuring the water at all times quite cold and fresh, I directed a pipe of lead, about 8 yards long, and  $\frac{3}{4}$  of an inch diameter, to be introduced through the wooden pipe described above, into the stratum of marl at the bottom of the well, so as to stand about 3 feet above the surface of the ground. Near the bottom of this leaden pipe was sewed, between 2 leaden rings or flanches, an inverted cone of stiff leather, into which some wool was stuffed to stretch it out, so that, after having passed through the wooden pipe, it might completely fill up the perforation of the clay. Another leaden ring or flanch was soldered round the leaden pipe, about 2 yards below the surface of the ground, which, with some doubles of flannel placed under it, was



nailed on the top of the wooden pipe, by which means the water was perfectly precluded from rising between the wooden and the leaden pipes.

This being accomplished, the bottom of the well remained quite dry, and the new water quickly rose about a foot above the top of the well in the leaden pipe: and, on bending the mouth of this pipe to the level of the surface of the ground, about 2 hogsheads of water flowed from it in 24 hours, which had similar properties with the water of St. Alkmund's well, as on comparison both these waters curdled a solution of soap in spirit of wine, and abounded with calcareous earth, which was copiously precipitated by a solution of fixed alkali; but the new water was found to possess a greater abundance of it, with numerous small bubbles of aërial acid or calcareous gas. The new water has now flowed about 12 months, and seems already increased to almost double the quantity in a given time; and I think it is now less replete with calcareous earth, approaching gradually to an exact correspondence with St. Alkmund's well, as it probably has its origin between the same strata of earth.

As many mountains bear incontestible marks of having been forcibly raised up by some power beneath them; and other mountains, and even islands, have been lifted up by subterraneous fires in our own times, we may safely reason on the same supposition in respect to all other great elevations of ground. Proofs of these circumstances are to be seen on both sides of this part of the country: whoever will inspect, with the eye of a philosopher, the lime-mountain at Breedon, on the edge of Leicestershire, will not hesitate a moment in pronouncing, that it has been forcibly elevated by some powers beneath it; for it is of a conical form, with the apex cut off, and the strata, which compose its central parts, and which are found nearly horizontal in the plain, are raised almost perpendicularly, and placed on their edges, while those on each side decline like the surface of the hill; so that this mountain may well be represented by a bur made by forcing a bodkin through several parallel sheets of paper. At Router, or Eagle-stone, in the Peak, several large masses of grit-stone are seen on the sides and bottom of the mountain, which by their form evince from what parts of the summit they were broken off at the time it was elevated; and the numerous loose stones scattered about the plains in its vicinity, and half buried in the earth, must have been thrown out by explosions, and prove the volcanic origin of the mountain. Add to this the vast beds of toad-stone or lava in many parts of this county, so accurately described, and so well explained by Mr. Whitehurst, in his *Theory of the Formation of the Earth*.

Now as all great elevations of ground have been thus raised by subterraneous fires, and in a long course of time their summits have been worn away, it happens, that some of the more interior strata of the earth are exposed naked on the tops of mountains; and that in general those strata which lie uppermost, or



nearest to the summit of the mountain, are the lowest in the contiguous plains. This will be readily conceived if the bur, made by thrusting a bodkin through several parallel sheets of paper, had a part of its apex cut off by a pen-knife, and is so well explained by Mr. Michell, in an ingenious paper on the Phenomena of Earthquakes, published a few years ago in the Philosophical Transactions.

And as the more elevated parts of a country are so much colder than the vallies, owing perhaps to a concurrence of 2 or 3 causes, but particularly to the less condensed state of the air on hills, which thence becomes a better conductor of heat, as well as of electricity, and permits it to escape the faster; it is from the water condensed on these cold surfaces of mountains that our common cold springs have their origin; and which, sliding between 2 of the strata above described, descend till they find or make themselves an outlet, and will in consequence rise to a level with the part of the mountain where they originated. And hence, if by piercing the earth you gain a spring between the 2d and 3d, or 3d and 4th stratum, it must generally happen, that the water from the lowest stratum will rise the highest, if confined in pipes, because it comes originally from a higher part of the country in its vicinity.

*II. An Account of an English Bird\* of the Genus Motacilla, supposed to be hitherto unnoticed by British Ornithologists. By the Rev. John Lightfoot,† M. A. F. R. S. p. 8.*

As every discovery in natural history is esteemed worthy the notice of that Society which was instituted on purpose to improve natural knowledge, I have

\* This bird is the *Sylvia arundinacea* or *Reed Wren* of Latham, and is suspected by that author to be the *lesser reed-sparrow* of Willoughby.

† A short biographical account of Mr. Lightfoot was published in the year 1788 by the late Mr. Pennant, who informs us that Mr. Lightfoot was the son of a highly reputable yeoman or gentleman-farmer, and was born at Newent, in the Forest of Dean, in the county of Gloucester, on the 9th of December 1735. He was educated at St. Crypt's school at Gloucester, whence he became an exhibitioner in Pembroke College, Oxford, where he continued his studies with much reputation. He was first appointed curate at Colnbrook, and afterwards at Uxbridge, which appointment he retained to the time of his death. His first patron was the honourable Mr. Lane, son to the late Lord Bingley. Lord Chancellor Northington presented him to the living of Shelden in Hants, which he resigned on taking the rectory of Gotham, in the county of Nottingham. He had also Sutton in Lownd, in the same county; to both of which he was presented by his Grace the Duke of Portland. His ecclesiastical preferments amounted to above 500l. a year. He was also domestic chaplain to his illustrious patroness the late Duchess Dowager of Portland, and by her liberality enjoyed, during her Grace's life, an annuity of 100l. a year. During her Grace's summer residence at Bulstrode, he did duty in the family twice a week, and at other times was of very considerable service in arranging her magnificent collection of natural history, particularly the botanical and conchyliological part. He was an excellent scholar in many branches of literature; but after the study of his profession, he addicted himself chiefly to that of botany and conchyliology, and excelled in both; but in the former was, according to Mr. Pennant, nearly unrivalled in Great Britain.



taken the liberty (says Mr. L.) to send a description and drawing of a bird which haunts the reeds of the river Coln, in the neighbourhood of Uxbridge, and which seems to have hitherto escaped the notice of writers on British Ornithology. The nest and eggs of this bird first attracted my attention, and led to the discovery of the bird itself. They were supposed by the fisherman who brought them, to belong to the Sedge-bird of Pennant, or *Motacilla Salicaria* of Linnæus; but being well acquainted with the nest and eggs of this, I was very sure he was mistaken, though he actually produced this bird as the true proprietor of the subjects in question. The structure and position of the nest having a singular appearance, and both that and the eggs belonging to a bird unknown to me, I became desirous of finding out the secret architect, and to that end made use of such means as I thought most likely to promote the discovery.

In a short time my expectations were gratified; for on July 26, 1783, intelligence was brought me, that such a nest as I wanted was found. I had given previous direction, that it should not be disturbed before I had seen it. On examination I instantly perceived it to be of the same kind and structure with that under inquiry, containing 2 eggs, and 2 young ones just excluded from the shell. One of the old birds was sitting at this time on the nest, which a person in company attempting to seize, it flew at him with so much resentment and acrimony, as to draw blood from his hand. Both the parent birds continued hovering about their nest with much watchful care and anxiety, while I made several attempts to take them alive; but, finding all endeavours in vain, lest I should lose the opportunity of examining them with accuracy, I at length, with reluctance, caused them to be shot; and from these specimens the following descriptions were made.

From the generic characters delivered by Linneus, our bird must evidently be reduced to the family of his *Motacilla*, for it has a weak, slender, subulate bill, almost straight; the mandibles nearly equal; the nostrils oval and naked, or not covered with bristles; the tongue lacerated at the extremity; the legs slender; the toes divided to the origin, except that the exterior one is joined, at the under part of the last joint, to the middle toe; the claws of nearly equal length. The

In 1772 he accompanied Mr. Pennant in a tour into Scotland, and a voyage to the Hebrides. This expedition gave rise to his excellent work, the *Flora Scotica*, which was published at the expense of Mr. Pennant.

In November, 1780, Mr. Lightfoot married Matilda, only daughter of Mr. William Barton Raynes, of Uxbridge, a lady who brought him a considerable fortune, and by whom he left two sons and three daughters. On the 20th of February 1788, while in apparent good health, he was suddenly seized with a paralytic stroke, and expired the same day.

His character was highly amiable and exemplary, and he was extremely liberal in affording his scientific assistance to those who requested it; never hiding his talents, or considering them impaired by communication.



male and female have the same coloured plumage, so that one description will serve for both. They differ a little in size, but their external appearance is the same. They are both larger than the Pettychaps described by Willoughby; smaller than the White-throat, and nearly of the same size with the Willow-wren; but to be more particular.

The cock-bird weighed, when just killed, exactly 7 dwts. 9 grs.; the hen 6 dwts. 9 grs. The male measured, from tip to tip of the extended wings,  $7\frac{1}{2}$  inches; the female  $6\frac{3}{4}$ .

From the end of the bill to the extremity of the tail, the cock measured  $5\frac{1}{2}$  inches; the hen only 5 inches. The bill in both measured half an inch, which is longer in proportion than in most of this genus. The upper mandible is of a dark horn colour, slightly incurved near the extremity, with a minute indenture on either side near the point; the lower is pale red or flesh-coloured, with a shade of yellow; the inside of the mouth deep orange-coloured; the tip of the tongue cloven and ciliated; the nostrils oval, and destitute of a bristly covering; but at the base of the upper mandible, on either side, near the angle of the mouth, arise 3 short vibrissæ pointing downwards, black at their summits, white at their bases; a circumstance common to many others of this genus. The iris of the eye is olive-brown; the pupil black. The short feathers of the orbits or eye-lashes are of a dirty white colour. From the corner of each eye to the nostril is a broad stroke or band of tawny-white feathers, lying over each other, and running narrowest towards the bill; this affords an excellent mark to distinguish the species.

The feathers of the head, neck, back, coverts of the wings and rump, are of an olive-brown, with a slight tinge of green. The quill and tail feathers are all of a darker hue, or simply brown; their outward edges of a paler shade. The tail is 2 inches long, slightly cuneated, the middle feathers being a little longer than the rest, the others gradually shorter; all of one uniform dun-brown colour edged with paler brown, and a little wedge-shaped at their ends. The chin is white; the throat, breast, belly, and parts about the vent, are white with a slight shade of buff or tawny; but all these feathers (as in several others of this genus) when blown asunder, or closely examined, are found to have their base or lower half black, except the shafts, which are white throughout. The ridge and under coverts of the exterior angle of the wing are of a yellowish-tawny colour, as are also the feathers of the thighs; but those of the knees are a shade darker, or a pale yellowish brown. The legs are a light olive; the soles of the feet bright yellow, with a tinge of green, which soon fades after the bird is dead. The instep is covered with 7 large imbricated scales, and 5 smaller on the toes, as in others of the genus. The toes stand 3 before, and 1 behind: the claws are nearly of equal length and curvature; but the hindmost is thickest and strongest.



From the foregoing remarks it is evident, that the bird mentioned is a species of *Motacilla*, which, as I can find no such described by any systematic writer, I shall venture to name, after the Linnean manner, *Motacilla (arundinacea) supra olivaceo-fusca, subtus albida, loris et orbitis fusco-albescentibus, angulo carpi subtus luteo-fulvo, cauda subcuneata fusca, plantis luteo-virescentibus.*

As we have already a bird, called in English the Willow-wren; ours, being nearly of the same size and shape, as well as the same genus, may, from its haunts, not improperly be denominated the Reed-wren. It frequents the banks of the river Coln near Uxbridge, as far as from Harefield-Moor down to Iver, about the space of 5 miles, and very probably most other parts of the same river, though not as yet observed. It is also certainly found in the neighbourhood of Dartford in Kent, whence a nest and eggs were communicated by the ingenious Mr. Latham of that place, but without knowledge of the bird to which they belonged; so that there is little doubt but that it may be found in many parts of the kingdom.

Its food is insects, at least in part, for I observed it catching flies. It hops continually from spray to spray, or from one reed to another, putting itself into a stooping posture before it moves. I heard it make no other than a single note, not unlike the sound of the word peep, uttered in a low plaintive tone; but this might probably be only a note of distress, and it may have, perhaps, more pleasing and melodious ones at other times, with which I am unacquainted. The nest is a most curious structure, unlike that of any other I am acquainted with, enough to point out the difference of the species, if every other character was wanting. It is composed externally of dry stalks of grass, lined, for the most part, with the flowery tufts of the common reed, or *arundo vallatoria*, but sometimes with small dead grasses, and a few black horse-hairs to cover them. This nest is usually found suspended or fastened on, like a hammock, between 3 or 4 stalks of reeds, below the panicles of flowers, in such a manner that the stalks run through the sides of the nests at nearly equal distances; or, to speak more properly, the nest is tied on to the reeds with dead grass, and sometimes (as being more eligible when it can be had) even with thread and pack-thread, emulating the work of a sempstress, as was the case of the nest exhibited in the drawing. The bird however, though generally, does not always confine her building to the support of reeds; sometimes she fixes it on to the branches of the water-dock; and, in one instance only (that here delineated), it was found fastened to the trifurcated branch of a syringa bush, or *Philadelphus*, growing in a garden hedge by the river side. She lays commonly four eggs; the ground colour a dirty white, stained all over with dull olive-coloured spots, but chiefly at the greater end, where are generally seen 2 or 3 small irregular black scratches; but these are sometimes scarcely visible.



I have reason to think, that this is a bird of migration; for the inhabitants on the sides of the Coln do not recollect ever to have seen it in the winter months; and its food being insects, it is probable it must be obliged to shift its quarters for a warmer climate at the approach of a severe season; but this at present is only matter of conjecture, and not certainty.

*III. An Account of Morne Garou, a Mountain in the Island of St. Vincent, with a Description of the Volcano on its Summit. By Mr. James Anderson, Surgeon. p. 16.*

“The many ridges of mountains which intersect this island in all directions, and rise in gradations, one above another, to a very great height, with the rivers tumbling from their sides over very high precipices, render it exceeding difficult to explore its interior parts. The most remarkable of these mountains is one that terminates the n. w. end of the island, and the highest in it, and has always been mentioned to have had volcanic eruptions from it. The traditions of the oldest inhabitants in the island, and the ravins at its bottom, seem to vindicate the assertion. As I was determined, during my stay in the island, to see as much of it as I could; and as I knew, from the altitude of this mountain, there was a probability of meeting with plants on it I could not find in any other part of the island; I should have attempted going up if I had heard nothing of a volcano being on it. But viewing the mountain at a distance, its structure was different from any in the island, or any I had seen in the West Indies. I could perceive it divided into many different ridges, separated by very deep chasms, and its summit appeared quite destitute of any vegetable production. On examining several ravins, that run from the bottom a great way up the mountain, I perceived they were quite destitute of water, and found pieces of pumice-stone, charcoal, several earths and minerals, that plainly indicated there must be some very singular place or other on some part of the mountain. I also recollected a story told by some very old men in the island, that they had heard the captain of a ship say, that between this island and St. Lucia he saw, towards night, flames and smoke issuing from the top of this mountain, and next morning his decks were covered with ashes and small stones. This was excitement enough to examine it, if I possibly could; but I was much discouraged on being told it was impossible to gain the summit of it; nor could I get either white men, Carribbee, or Negro, that would undertake to conduct me up for any reward I could offer; nor could I get any information relative to it. But as difficulty to attain enhances the value of the object, so the more I was told of the impossibility of going up, the more was I determined to attempt it.” Accordingly, having at length been afforded assistance by some friends, Mr. A. set out on this dangerous and desperate adventure. After suffering amazing hardships for several days and nights, some-



times advancing and sometimes being obliged to return, he arrived greatly exhausted at the long desired object.

As soon as we could see in the morning, proceeds Mr. A., we returned to the ridge we left the night before, and began to work with alacrity, as we were almost chilled with cold. I pushed on as fast as possible, and about 10 o'clock found the woods began to get thin. I could not see the top of the mountain, but had a view of several ridges that joined it. From the wind falling, and the heat becoming intense, I thought we must then be under the cover of the summit: I here found many new plants. About 11 I was overjoyed to have a full view of the summit of the mountain, nearly a mile distant from us, and that we were nearly out of the woody region. The top seemed to be composed of 6 or 7 different ridges, very much broken in the sides, as if they had suffered great convulsions of nature; they were divided by amazing deep ravins, without any water in them. I observed where the ridges meet the edge of a large excavation, as it seemed to be, on the highest part. I imagined this might be the mouth of the crater, and directed my course to a high peak which overlooked it. I found here a most beautiful tree which composed the last wood. After that I entered into a thick long grass; intermixed with fern, which branched and ran in every direction. To break it was impossible, and with great difficulty I could cut it; so that in clearing our way through this grass, 8 or 10 feet high, there was equal difficulty as in the woods, and it seemed to continue very near to the top of the mountain. Being now about noon, I and the negroes were so fatigued as hardly to be able to stand; our thirst very great, to allay which, as much as possible, we chewed the leaves of the *Begonia obliqua*. Two of the negroes returned, and the others said they would go no farther with me, as they must perish for want of water, and it would be impossible to get to the bottom before night, and they must all die in the woods. The propriety of their reasoning was evident to me; yet I thought it hard, after the fatigues of 3 days and 2 nights, to be within half a mile of the top, and not be able to get up, and to know little more about it than I did at the bottom. As the negroes had not the same motive for going up as I, all my reasoning was to them ineffectual; I found I was obliged to return myself, as I could not persist alone. At half past 12 we began to descend the same way we came. As there was now a clear path all the way to the bottom, we got down to Mr. Gasco's by sun-set. After sitting some time here, I was hardly able to rise again, I was so tired; and my feet were so sore I could hardly stand on them, for, my shoes being torn to pieces, I came down the whole way bare-footed. I continued my journey however down to Mr. Maloune's, where I arrived between 6 and 7 at night.

March 4th, being the day I had fixed to finish my excursion, about 4 in the morning, I left the house of Mr. Fraser, who out of curiosity agreed to accom-



pany me, of which I was very glad, as he was a sensible young man; and with the assistance of 2 negroes we pursued our journey. We found very little obstruction in our way up, till we got to the place whence I returned; and there for about a quarter of a mile, we had considerable difficulty to clear our way through grass and ferns. After we came within a quarter of a mile of the top, we found ourselves suddenly in another climate, the air very cold, and the vegetable productions changed; here was nothing but barrenness over the whole summit of the mountain. On the confines of the grassy and the barren region I found some beautiful plants. Moss grows here in such plenty, that I frequently sunk up to my knees in it. This is the only place in the West Indies that produced any moss that I have seen. About noon we gained the top of the peak I had directed my course to before; when in an instant we were surprized with one of the grandest and most awful scenes I had ever beheld. I was struck with it amazingly, as I could not have conceived such a very large and so singularly formed an excavation. It is situated on the centre of the mountain, and where the various ridges unite. Its diameter is something more than a mile, and its circumference to appearance a perfect circle. Its depth from the surrounding margin is above a quarter of a mile, and it narrows a little, but very regularly, to the bottom. Its sides are very smooth, and for the most part covered with short moss, except towards the south, where there are a number of small holes and rents. This is the only place where it is possible to go down to the bottom; it is exceedingly dangerous, owing to the numberless small chasms. On the west side is a section of red rock like granite, cut very smooth, and of the same declivity with the other parts. All the rest of the surrounding sides seem to be composed of sand, that appear to have undergone the action of intense fire. It has a crust quite smooth, of about an inch thick, and hard almost as rock; after breaking through which, is found nothing but loose sand. In the centre of the bottom is a burning mountain of about a mile in circumference, of a conic form, but quite level. On the summit, out of the centre of the top, arises another mount, 8 or 10 feet high, a perfect cone; from its apex issues a column of smoke. It is composed of large masses of red granite-like rock of various sizes and shapes, which appear to have been split into their present magnitudes by some terrible convulsion of nature, and are piled up very regularly. From most parts of the mountain issue great quantities of smoke, especially on the north side, which appears to be burning from top to bottom, and the heat is so intense, that it is impossible to go upon it. Going round the base is very dangerous, as large masses of rock are constantly splitting with the heat, and tumbling to the bottom. At the bottom, on the north side, is a very large rock split in two; each of these halves, which are separated to a considerable distance from each other, is rent in all directions, and from the crevices issue efflorescences of a



glossy appearance, which taste like vitriol, and also beautiful crystallizations of sulphur. On all parts of the mountain are great quantities of sulphur in all states; also alum, vitriol, and other minerals. From the external appearance of this mountain, I imagine it has only begun to burn lately, as on several parts of it I saw small shrubs and grass, which looked as if they had been lately scorched and burnt. There are several holes on the south, from which issues smoke, seemingly broken out lately, as the bushes around are but lately burnt. On two opposite sides of the burning mountain, east and west, reaching from its base to that of the side of the crater, are two lakes of water, about a stone's throw in breadth; they appear to be deep in the middle; their bottom to be covered with a clay-like substance. The water seems pleasant to the taste, and is of a chalybeate nature. I suppose these lakes receive great increase, if they are not entirely supported, by the rain that tumbles down the side of the crater. I observed on the north side of the bottom traces of beds of rivers, that to appearance run great quantities of water at times to both these lakes. By the stones at their edges, I could perceive that either absorption or evaporation, or perhaps both, go on fast. The greater part of the bottom of the crater, except the mountain and two lakes, is very level. On the south part are several shrubs and small trees. There are many stones in it that seem to be impregnated with minerals: I saw several pieces of pumice-stone. I also found many stones about the size of a man's fist, rough, on one side blue, which appearance, I imagine, they have got from heat, and being in contact with some mineral. These stones are scattered over the whole mountain.

After I had got up from the bottom of the crater, I could not help viewing it with admiration, from its wonderful structure and regularity. Here I found an excavation cut through the mountain and rocks to an amazing depth, and with as much regularity and proportion of its constituent parts, as if it had been planned by the hand of the most skilful mathematician. I wished much to remain on the mountain all night, to examine its several ridges with more attention next day; but I could not prevail on my companion to stay, and therefore thought it advisable to accompany him. I observed the motion of the clouds on this mountain to be very singular. Though there are several parts on it higher than the mouth of the crater, yet I saw their attraction was always to it. After entering on its east or windward side, they sunk a considerable way into it; then, mounting the opposite side, and whirling round the north-west side, they ran along a ridge, which tended nearly north-east, and afterwards sunk into a deep ravin, which divided this ridge from another on the north-west corner of the mountain, and the highest on it, lying in a direction nearly south and north. They keep the course of this ridge to the south end, and then whirl off west in their natural course.



I took my departure from the mountain with great reluctance. Though I encountered many difficulties to get up, yet it amply rewarded me for all my toil; but I had not time to examine it with that attention I wished. When I got on the peak from which I had my first view of it, and from which I could see its different parts, I could not help reviewing it several times. After imprinting its structure on my mind, I took my final adieu of it, and returned down, and got to Mr. Fraser's house about 7 at night, much fatigued. I am sorry I had no instruments, to take the state of the air, or the exact dimensions of the different parts of the mountain; but I believe on measurement, they will be more than I have mentioned. From the situation of these islands to each other, and to the continent of South America, I imagine there are sub-marine communications between the burning mountains or volcanoes in each of them, and from them to the volcanoes on the high mountains of America. The islands, which are situated next the continent, seem to tend in the direction of those mountains; and I have observed, that the crater in this island lies nearly in a line with Soufriere in St. Lucia and Morne Pelée in Martinique, and I dare say from Morne Pelée to a place of the same kind in Dominique, and from it to the others; as it is certain there is something of this kind in each of these islands, Barbadoes and Tobago excepted, which are quite out of the range of the rest. There is no doubt but eruptions or different changes in some of them; though at a great distance, may be communicated to and affect the others in various manners. It is observed by the inhabitants round these burning mountains, that shocks of earthquakes are frequent near them, and more sensibly felt than in other parts of the island, and the shocks always go in the direction of them.

*IV. A Supplement to the Third Part of the Paper on the Summation of Infinite Series, in the Philos. Trans. for 1782.\* By the Rev. S. Vince, M. A. p. 32.*

The reasoning in the 3d part of my paper on the Summation of infinite Series having been misunderstood, says Mr. V., I have thought it proper to offer to the R. S. the following explanation. When I proposed, for example, to sum the series  $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} - \&c.$  sine fine, I wanted to find some quantity which, by its expansion, would produce that series, and that quantity I called its sum; not, as I conceived must have been evident to every one, in the common acceptation of that word, that the more terms we take, the more nearly we should approach to that quantity, and at last arrive nearer to it than by any assignable difference, for there manifestly can be no such quantity; but as being a quantity from which the series must have been deduced by expansion, which quantity I found to be  $-\frac{1}{2} + H. L. 2.$  If therefore, in the solution of any problem, the conclusion,

\* Page 309, of this volume.



whose value I want, is expressed by the above series, and which arose from the necessity of expanding some quantity in the preceding part of the operation, surely no one can deny that I may substitute for it  $-\frac{1}{2} + \text{H. L. } 2$ . For whatever quantity it was, which by its expansion produced at first a series, the same reduction which, from that series, produced the series  $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} - \&c.$  must also have produced  $-\frac{1}{2} + \text{H. L. } 2$ , from the quantity which was expanded. This value of the series I obtained in the following manner. I supposed the series  $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} - \&c.$  to be divided into 2 parts; the first part to contain all the terms till we come to those where the numerators and denominators become both infinitely great, in which case every term afterwards may be supposed to be equal to unity; the 2d part therefore would necessarily be, supposing the first part to terminate at an even number of terms,  $1 - 1 + 1 - 1 + \&c.$  sine fine. The first part, by collecting 2 terms into 1, becomes  $-\frac{1}{2 \cdot 3} - \frac{1}{4 \cdot 5} - \frac{1}{6 \cdot 7} - \&c.$  which series, as it is continued till the terms become infinitely small, is equal to  $-1 + \text{H. L. } 2$ . The 2d part  $1 - 1 + 1 - \&c.$  has not, taken abstractedly of its origin, any determinate value, as will be afterwards observed, but considered as part of the original series it has, for that series must have been deduced from the expansion of the binomial  $(1+x)^{-1}$ , or  $\frac{1}{1+x}$ ; and hence, when  $x=1$ ,  $1 - 1 + 1 - \&c.$  can in this case have come only from  $\frac{1}{1+1}$ , which, therefore, must be substituted for it; consequently the 2 parts together give  $-\frac{1}{2} + \text{H. L. } 2$ .

Having thus explained the nature of the series which I proposed to sum, and the principle on which the correction depends, I must beg leave to acknowledge my obligations to my very worthy and ingenious friend George Atwood, Esq., F. R. S., who first observed that the series  $1 - 1 + 1 - 1 + \&c.$  has no determinate value in the abstract, as it may be produced by  $\frac{1}{1+1+1+\&c.}$  whatever be the number of units in the denominator;\* and it may also be added, that the same series arises from  $\frac{1+1+1+\&c.}{1+1+1+1+\&c.}$ , provided the number of units be greater in the denominator than in the numerator. The correction will therefore be different in different circumstances, and will depend on the nature of the quantity which was at first expanded. In the 3d part of my paper, I applied the correction to those cases where the original series arose from the expansion of a binomial, where the correction is in general as I there gave it; but as I did not apply my method to any other series, I confess that it did not appear to me, that the correction would then be different, which it necessarily would, had I extended my reasoning to other cases. I shall therefore add one example to show

\* I have been since informed by Mr. Wales, F. R. S., that a pupil of his, Mr. Bond, made the same observation.—Orig.



the method of correction in other instances, where the value of the correction will be found to be different, according as we begin to collect at the 1st or 2d term. Let the series be  $\frac{2}{1} - \frac{3}{2} + \frac{4}{3} - \frac{5}{4} + \frac{6}{5} - \&c.$  sine fine, which came originally from  $\frac{1}{1+x+x^2}$ ; now if we begin to collect at the 1st term, the series becomes  $\frac{1}{1.2} + \frac{1}{4.5} + \&c.$  and for the same reason as before, the correction, to be added, is  $\frac{1}{3}$ ; but  $\frac{1}{1.2} + \frac{1}{4.5} + \&c. = \frac{4}{3}$  of a circular arc ( $\Delta$ ) of  $30^\circ$  to the radius  $\frac{1}{2}\sqrt{3}$ ; hence the sum required  $= \frac{4}{3}\Delta + \frac{1}{3}$ . If we begin to collect at the 2d term, the series becomes  $2 - \frac{2}{2.4} - \frac{2}{5.7} - \&c.$ ; and the correction to be subtracted is  $\frac{2}{3}$ ; for the 2d part of the original series is now  $-1 + 1 - 1 + 1 - \&c.$  which was produced by  $\frac{1+1}{1+1+1}$ ; but  $2 - \frac{2}{2.4} - \frac{2}{5.7} - \&c. = 1 + \frac{4}{3}\Delta$ ; therefore the sum required  $= \frac{1}{3} + \frac{4}{3}\Delta$  as before. In the same manner we may apply the correction in all other cases. Though, therefore, the series  $1 - 1 + 1 - 1 + \&c.$  or  $-1 + 1 - 1 + 1 - \&c.$  have no determinate value in the abstract, yet the given series will fix its value by pointing out the quantity from which the series must have been originally produced.

V. *Description of a Plant yielding Asafoetida.* By John Hope,\* M.D.,  
F.R.S. p. 36.

ASAFŒTIDA. [FERULA ASSAFŒTIDA.]

An umbelliferous plant, about 3 feet high, up- ovate, many times pinnate-divided; leaflets cut,  
right, branchy, glaucous, with yellow flowers. subacute, subdecurrent; the common footstalk  
Root perennial. flat above, with an elevated line running longitu-  
Leaves radical six, procumbent, three-lobed- dinally through the middle.

\* Dr. John Hope was the son of a respectable surgeon at Edinburgh, where he was born in 1725. After the usual grammatical education, he entered on the study of physic at his native place. He afterwards went to Paris, and attended the lectures and demonstrations of the celebrated Bernard Jussieu. Returning from his travels, he obtained the degree of M. D. from the university of Glasgow, in 1750, and soon after was admitted a member of the Royal College of Physicians at Edinburgh, where he settled for the purpose of engaging in practice. In 1761 he was appointed to the professorships of botany and mat. med. vacant by the death of Dr. Alston. His health becoming impaired, he some years afterwards resigned the professorship of the mat. med. but still continued to hold his botanical appointment; to which was afterwards added the office of physician to the Royal Infirmary. At the time of his death, which happened in November 1786, he held the high office of president of the Royal College of Physicians at Edinburgh, and several years before he had been elected a fellow of the R. S. of London.

Dr. H. was indefatigable in promoting the progress of his favourite science, botany. With him originated the botanical garden near Edinburgh, planted on a spot, which before was little better than a dreary waste; but which in a few years was stocked with the rarest plants of every clime. The pecuniary supplies requisite for this establishment were obtained from the Sovereign, first during the administration of Lord Bute; and the sum granted for this purpose was afterwards augmented, when the Duke of Portland presided over his Majesty's councils. It was in this garden that Dr. H. reared



*Stem* 2 feet high, upright, roundish, annual, slightly striated, smooth, naked, except at one juncture towards the middle of the imperfect leaves; footstalk membranaceous, concave.

*Branches* naked, spreading; of which the 3 lowermost and alternate ones are sustained, by the membranaceous concave footstalk of each imperfect leaf.

The 4 intermediate ones are verticillated. The upper ones from the top of the stem are 8, of which the internal ones are upright.

All these branches bear on the top a compound, sessile, terminal umbel, besides from 3 to 6 branchlets placed beyond, and supporting compound umbels.

In this manner the lower branches support 5, rarely 6 branchlets; the intermediate ones 3 or 4; and the upper ones 1 or 2.

*CALYX.* The *universal umbel* consists of from 20 to 30 rays.

The *partial umbel* of from 10 to 20, with subsessile flowers.

The *sessile compound umbel* is plano-convex.

———— the pedunculated, hemispheric.

*Involucre universal* none.

———— *partial* none.

*Perianth proper* scarcely conspicuous.

*COROL universal* uniform.

*Floscules* of the sessile umbel fertile.

———— of the pedunculated umbel generally abortive.

*COROL proper* consisting of 5 equal, flat, ovate petals; at first spreading, then reflex; with ascending tip.

*STAMENS filaments* 5, subulate, longer than corol, incurvated; *anthers* roundish.

*PISTIL. Germ* turbinated, inferior.

*Styles* 2, reflex.

*Stigmas* thickened at the tip.

*PERICARP* none: fruit oblong, flat, compressed, and marked on each side by 3 elevated lines.

*SEEDS* 2, oblong, large, flat on each side, and marked by 3 elevated lines.

The plant diffuses an alliaceous odor. The leaves, branches, footstalks, root, and trunk, when cut, afford a milky juice, with the taste and smell of asafœtida.

Though asafœtida has been used in medicine for many ages, having been introduced by the Arabian physicians near a thousand years ago; yet there was no satisfactory account of the plant which yielded it, till Kæmpfer published his *Amœnitates Exoticæ* about 70 years since. Towards the end of the last century he travelled over a great part of Asia, and was in Persia, and on the spot where the asafœtida is collected. He gives a full account of the manner of collecting it. He describes the plant; and also gives a figure of it, differing in many res-

the rheum palmatum, obtaining from it roots equal in medicinal efficacy to those imported from the Levant, and accordingly recommending the cultivation of it in this country, on a large scale; a recommendation which has since been adopted with the best results. Here he also reared the plant which yields asafœtida. On these subjects he communicated 2 papers to the R. S. besides a 3d on a rare plant found in the Isle of Skye.

Among the cultivators of natural history in Great Britain, Dr. H. was one of the first who embraced the Linnean arrangement of plants. The sexual system, says Dr. Pulteney, (*Sketches of the Progress of Botany in England*, 2d vol.) was received nearly about the same time in the universities of Britain, being publicly taught by Professor Martyn at Cambridge, and by Dr. Hope at Edinburgh. The adoption of it (he adds) by these learned professors, I consider as the æra of the establishment of the Linnean system in Britain. Dr. H.'s name has been given to a beautiful tree, *Hopea*, which affords a yellow dye, and is a native of Carolina. For other interesting particulars concerning Dr. H. see Dr. Duncan's *Medical Commentaries* for 1788; from which has been extracted for the most part the account here given.



pects from those now presented to the Society.\* Six years ago, I received from Dr. Guthrie, of St. Petersburg, 2 roots of the asafoetida, with the following card from Dr. Pallas, addressed to Dr. Guthrie: "Dr. Pallas's compliments to Dr. Guthrie; he sends him 2 roots of the ferula asafoetida, a plant which he thinks never was cultivated in any European garden, and which nobody has been so fortunate as to raise from seed but himself, though the seeds sent to the Academy from the mountains of Ghilan in Persia had been distributed among several curious persons."

Both these roots were planted in the open ground, in the Botanic Garden at Edinburgh; one died; the other after some time did well, and last summer flowered and produced seed. The plant was of a pale sea-green colour, and grew to the height of 3 feet. The stem is deciduous, but the root is perennial. Every part of the plant, when wounded, poured out a rich milky juice, resembling in smell and taste asafoetida; and at times a smell resembling garlick, such as a faint impregnation of asafoetida yields, was perceivable at the distance of several feet. In Persia, at the proper season, the root is cut over once and again; from the incisions there flows a thick juice like cream, which, hardened, is the asafoetida. As the plant grows in the open air, without protection, and even in an unfavourable season produced a good deal of seed, and as the juice seems to be of the same nature with the officinal asafoetida, there is some reason to hope, that it may become an article of cultivation in this country of no inconsiderable importance.

*VI. Catalogue of Double Stars. By Wm. Herschel, Esq. F. R. S. p. 40.*

The great use of double stars having been already pointed out in a former paper, on the parallax of the fixed stars, and in a latter one, on the motion of the solar system, I have now drawn up a 2d collection of 434 more, which I have found out since the first was delivered. The happy opportunity of giving all my time to the pursuit of astronomy, which it has pleased the royal patron of this society to furnish me with, has put it in my power to make the present collection much more perfect than the former; almost every double star in it having the distance and position of its 2 stars measured by proper micrometers; and the observations have been much oftener repeated. The method of classing them is in every respect the same as that which has been used in the first collection; for which reason I refer to the introductory remarks that have been given with

\* Probably Kæmpfer's asafoetida plant is a different species from that described by Dr. Hope in this paper. Kæmpfer was himself on the mountains where the drug is collected, and his fidelity in describing, as well as delineating, has not hitherto been impeached. Sanguis Draconis, and some other gums, are indifferently the produce of various species of plants; and why may not asafoetida be similarly circumstanced? JOS. BANKS.—Orig.



that collection for an explanation of several particulars necessary to be previously known. The numbers of the stars are here also continued, so that the first class ending there at 24 begins here at 25, and the same is done with the other classes.

Most of the double stars in my first collection are among the number of those stars which have their places determined in Mr. Flamsteed's extensive catalogue; but of this collection many are not contained in that author's work. I have therefore adopted a method of pointing them out, which it will be proper to describe. The finder of my reflector is limited, by a proper diaphragm, to a natural field of  $2^{\circ}$  of a great circle in diameter. The intersection of the cross wires, in the centre of it, points out  $1^{\circ}$ ; and by the eye this degree, or the distance from the centre to the circumference, may be divided into  $\frac{1}{4}$ ,  $\frac{1}{2}$ ,  $\frac{3}{4}$ ,  $\frac{1}{3}$ , and  $\frac{2}{3}$ . Thus we are furnished with a measure which, though coarse, is however sufficiently accurate for the purpose here intended; and which, if more than  $2^{\circ}$  are wanted, may be repeated at pleasure. In such measures as these I have given the distance of a double star, whose place I wanted to point out, from the nearest star in Flamsteed's catalogue. And since, besides the distance, it is also required to have its position with regard to the star thus referred to, I have used the neighbouring stars for the purpose of pointing it out.

It will sometimes happen, that other stars are very near those which are thus pointed out, that might be mistaken for them. In such cases an additional precaution has been used by mentioning some circumstance either of magnitude or situation, to distinguish the intended star from the rest. After all, if any observer should be still at a loss to find these stars without having their right ascension and declination, he may furnish himself with them by means of Flamsteed's *Atlas Cœlestis*; for my description will be sufficiently exact for him to make a point in the maps to denote the star's place; then, by means of the graduated margin, he will have its AR and declination to the time of the Atlas, which he may reduce to any other period by the usual computations.

Before quitting this subject I must remark, that it will be found on trial, that this method of pointing out a double star is not only equal, but indeed superior, to having its right ascension and declination given: for, since it is to be viewed with very high powers, not such as fixed instruments are generally furnished with, the given right ascension and declination would be of no service. We might, indeed, find the star by a fixed or equatorial instrument; and, taking notice of its situation with regard to other neighbouring stars, find, and view it afterwards, by a more powerful telescope; but this will nearly amount to the very same way which here is pursued, with more deliberate accuracy than we are apt to use, while we are employed in seeking out an object to look at.

It will be required, that the observer should be furnished with Flamsteed's



Atlas Cœlestis, which must have the stars marked from the author's catalogue, by a number easily added to every star with pen and ink, as I have done to mine. The catalogue should also be numbered by an additional column, after that which contains the magnitudes. I hope in some future editions of the Atlas to see this method adopted in print, as the advantage of it is very considerable, both in referring to the catalogue for the place of a star laid down in the Atlas, and in finding a star in the latter whose place is given in the former.

I would recommend a precaution to those who wish to examine the closest of my double stars. It relates to the adjustment of the focus. Supposing the telescope and the observer long enough out in the open air to have acquired a settled temperature, and the night sufficiently clear for the purpose; let the focus of the instrument be re-adjusted with the utmost delicacy on a star known to be single, of nearly the same altitude, magnitude, and colour, as the star which is to be examined, or one star above and another below the same.

The measures of the distances were all taken with a parallel silk-worm's-thread micrometer, and a power of 227 only. They are not, as in the former catalogue, with the diameters included, but from the centre of one star to the centre of the other. I have adopted these measures on finding that I could procure threads fine enough to subtend only an angle of about  $1''13'''$ , and that by this means there was no longer any great difficulty of judging when the stars were centrally covered by the threads. However, I do not know whether these measures, with stars at a considerable distance, may not be liable to an additional error of perhaps  $1''$ , owing to the remaining uncertainty in judging of their exact central position while the measure is taking.

The positions have all been measured, unless marked otherwise, with a power of 460, adapted to an excellent micrometer, executed by Messrs. Nairne and Blunt, according to the model given in the Philos. Trans. vol. 71, p. 500; but with a great and necessary improvement of making the wheel d, d, of that figure perform its whole revolution; by which means the two silk-worms-threads may be adjusted to a greater degree of exactness; for if they are not placed so as perfectly to bisect the circle, the two threads will not coincide exactly after having performed one semi-revolution, which they must be made to do with the utmost rigour. I found the absolute necessity of this precaution when I came critically to examine the positions of the Georgium Sidus, as they are given in table 3, Phil. Trans. vol. 71, p. 497. The measures were affected with a small and pretty regular error, which I was at a loss to account for; and the distance of this star being then totally unknown, I looked for the cause of the deviation at first in a diurnal parallax of that heavenly body; but soon found it owing to the inconvenience before-mentioned, of not being able experimentally to adjust the moveable thread to that critical nicety which I have now introduced and used in all the angles of the present catalogue.



The arrangement of this catalogue of multiple stars, in the original Transactions, being very extensive and voluminous, it is here greatly abridged by contracting the description, omitting the less material parts of it, and disposing the whole in the form of short tables of 4 columns; viz. 1st, the number of the star continued from the former catalogue; 2d, the name of the star, or Flamsteed's number or other description of it; 3d, its multiple, or the number of stars in it, whether double, or triple, or quadruple, &c.; 4th, the position of the inferior with respect to the chief or principle star. These stars were observed in the years 1782 and 1783. And they are continued through classes the same as in the former part of the catalogue above-mentioned. In the 1st and 3d columns, prec. denotes preceding, and foll. following.

## CATALOGUE OF DOUBLE STARS.

*First Class.*

No.	Name, or Flamsteed's marks, &c.	Multiple	Position, &c.	No.	Name, or Flamsteed's marks, &c.	Multiple	Position, &c.
I.				I			
25	A Orionis, Flam. 32.	Double	52° 10' s. pre.	62	Foll. Fl. 2 Equulei	Double	35° 9' s. pre.
26	α Leonis, Fl. 2.	Double	20 54 s. foll.	63	Fl. 5 γ Equulei s.	Double	5 57 s. pre.
27	Fl. 90 Leonis	Treble	35 12 s. pre.	64	Fl. 42 π Arietis	Treble	19 19 s. foll.
28	γ Leonis. Fl. 41	Double	5 24 n. foll.	65	In neb. β Sagit. foll.	Double	14 0 n. pre.
29	Near Fl. 44 Leonis	Double	96 32 n. foll.	66	Fl. 23 ε Dracon. s. prec.	Double	2 24 s. pre.
30	Fl. 57, ad δ Cancri	Double	68 12 n. pre.	67	Nebu. Aurigæ prec.	Double	23 57 n. foll.
31	Inter Fl. 41 & 39, Lyncis	Double	51 21 s. pre.	68	Near Fl. 10 Orionis	Double	84 54 s. foll.
32	Fl. 44 α Lyncis s. prec.	Double	8 27 s. pre.	69	In Lyncis pectore	Double	77 0 s. foll.
33	ζ Libræ Fl. 51	Treble	82 2 n. foll.	70	Fl. 123 ζ Tauri n. prec.	Double	36 24 s. pre.
34	Fl. 55 Cassiop.	Treble	20 30 n. pre.	71	Fl. 24 Ursæ Maj. s. pre.	Double	2 6 n. foll.
35	Fl. 38 Serpent.	Double	60 48 n. pre.	72	Fl. 65 Ursæ Majoris	Double	22 21 s. foll.
36	Fl. 40 ζ Hercul.	Double	20 42 n. foll.	73	Fl. 6 β Arietis n. prec.	Double	77 24 s. foll.
37	Fl. 11 φ Hercul. seq.	Double	59 43 s. foll.	74	Fl. 39 Arietis n. prec.	Double	20 36 n. pre.
38	Fl. 18 Persei n. prec.	Double	9 42 n. pre.	75	Fl. 26 Orionis s. prec.	Double	89 36 n. pre.
39	Fl. 11 β Cassiop s. prec.	Double	50 42 n. pre.	76	In pectore Lyncis	Double	3 42 s. pre.
40	Fl. 25 Cassiop. n. prec.	Double	50 30 s. foll.	77	Fl. 7 α Crateris boreal.	Double	82 24 n. foll.
41	Fl. 31 Draconis n.	Double	84 21 n. pre.	78	Fl. 11 Libræ borealior	Double	58 24 n. pre.
42	Fl. 13 δ Serpent.	Double	42 48 s. pre.	79	Fl. 46 Herculis	Double	66 36 s. foll.
43	Ad. Fl. 48 Dracon.	Double	88 44 n. pre.	80	Fl. 81 Virginis	Double	41 12 n. foll.
44	Fl. 4 Aquar.	Double	81 30 n. pre.	81	Fl. 44 π Serp. s. prec.	Double	31 48 s. pre.
45	Fl. 11 ρ Aurig. s. prec.	Double	47 33 s. pre.	82	Fl. 49 Serpentis	Double	21 33 n. pre.
46	Fl. 13 ν Aquar. n. foll.	Treble	62 27 n. pre.	83	Fl. 10 λ Ophiuchi	Double	14 30 n. foll.
47	Fl. 29 Capric. n. prec.	Double	84 48 n. foll.	84	Fl. 50 Aurigæ austr.	Double	14 0 n. foll.
48	Fl. 6 Cephei prec.	Double	14 9 s. pre.	85	Fl. 36 Lyncis s. foll.	Double	88 57 n. foll.
49	Fl. 22 λ Ceph. n. foll.	Double	85 48 n. foll.	86	Fl. 105 Herculis boreal.	Double	79 24 n. pre.
50	Fl. 73 λ Aquar. prec.	Double	41 12 n. pre.	87	Fl. 73 ρ Ophiuchi	Double	2 48 s. pre.
51	Fl. 32 ι Ceph. foll.	Double	3 36 s. pre.	88	Fl. 69 τ Ophiuchi	Double	61 36 n. pre.
52	Near Fl. 25 Orion.	Double	52 48 n. pre.	89	Fl. 56 Androm. n. prec.	Double	75 30 s. foll.
53	Near Fl. 30 Orion.	Double	43 24 n. foll.	90	Fl. 22 β Aquarii s. pre.	Double	76 36 s. foll.
54	Fl. 20 τ Orion. prec.	Double	35 42 n. pre.	91	Fl. 50 γ Aquilæ n. pre.	Double	8 18 n. pre.
55	Fl. 8 Tauri n. prec.	Double	82 48 s. foll.	92	Fl. 52 π Aquilæ	Double	34 24 s. foll.
56	Fl. 54 Ceti s. foll.	Double	87 39 n. foll.	93	Fl. 62 Aquilæ n. prec.	Double	19 9 n. pre.
57	Fl. 70 & 67 Orion. prec.	Multi.	19 43 s. foll.	94	Fl. 18 δ Cygni	Double	18 21 n. foll.
58	Fl. 12 δ Lyræ foll.	Double	13 0 n. pre.	95	Fl. 33 Cygni s. foll.	Double	72 15 n. pre.
59	Fl. 18 ab δ Lyræ	Double	75 0 s. pre.	96	Fl. 21 ν Cygni s. foll.	Treble	89 18 s. foll.
60	Ex. γ & λ Lyræ s. foll.	Double	16 48 n. pre.	97	Fl. 51 Cygni foll.	Double	46 24 n. foll.
61	Prec. Fl. 1 Equulei	Double	18 24 n. pre.				



*Second Class of Double Stars.*

No.	Name, or Flamsteed's marks, &c.	Multiple	Position, &c.	No.	Name, or Flamsteed's marks, &c.	Multiple	Position, &c.
II.				II.			
39	Near Procyon	Double	54° 28' s. foll.	71	Fl. 58 Aurigæ s.	Mult.	44° 36' n. pre.
40	2d to Fl. 23 $\phi$ Cancr.	Double	56 42 n. foll.	72	Fl. 13 Lyncis s.	Double	10 0 s. pre.
41	1st to Fl. 24 $\nu$ Cancr.	Double	32 9 n. foll.	73	Fl. 21 Ursæ majoris	Double	36 45 n. foll.
42	Ex. $k$ Virginis prec.	Double	52 24 s. foll.	74	Fl. 74 $\nu$ Crateris n.	Treble	68 or 69° s. pr.
43	Fl. 43 Leonis s. prec.	Double	85 2 n. foll.	75	Fl. 118 Tauri	Double	77 15
44	Fl. 84 $\alpha$ Virginis	Double	29 5 s. pre.	76	Fl. 63 $\tau$ Arietis s. foll.	Double	15 24 s. pre.
45	Fl. 54 Virginis	Double	57 0 n. foll.	77	Fl. 17 Hydræ	Double	90 0 n.
46	Fl. 42 Comæ Ber. s. foll.	Double	6 42 s. foll.	78	Fl. 63 $\chi$ Leonis s. foll.	Double	75 21 s. foll.
47	Fl. 2 Comæ Beren.	Double	27 42 s. pre.	79	Fl. 39 Bootis	Double	38 21 n. foll.
48	Near Fl. 16 Aurigæ	Double	15 48 n. foll.	80	Fl. 40 $d$ Eridani adj.	Double	56 42 n. pre.
49	By Fl. 110, $\alpha$ Pisc. Bore.	Double	59 6 n. pre.	81	Fl. 49 Eridani foll.	Double	51 36 n. pre.
50	Fl. 38 Piscium	Double	25 3 s. pre.	82	Fl. 31 Bootis s. foll.	Double	1 0 s. foll.
51	Fl. 11 $\xi$ Capricorni	Double	84 0 s. foll.	83	Fl. 22 Andromedæ n.	Double	5 48 n. foll.
52	Fl. 40 $\alpha$ Persei n. foll.	Double	8 24 n. pre.	84	Fl. 65 Piscium	Double	30 57 n. foll.
53	Fl. 12 Camelopar. prec.	Double	18 33 s. foll.	85	Fl. 36 $b$ Serpent. n. foll.	Double	46 9 n. pre.
54	Prec. Fl. 74 $\epsilon$ Tauri	Double	68 42 s. pre.	86	Fl. 49 Serpentis s. foll.	Double	53 9 s. foll.
55	Fl. 4 Ceti s. foll.	Double	21 42 n. pre.	87	Fl. 29, 30 Monocerotiss.	Mult.	86 12 s. foll.
56	Fl. 6 $\beta$ Arietis n. foll.	Double	23 12 n. pre.	88	Fl. 51 $\omega$ Serpent. s. foll.	Double	44 45 n. pre.
57	Near Fl. 72 Aquarii	Treble	50 or 55° s. foll.	89	Ad genam Monocerotis	Double	50 51 n. foll.
58	Fl. 56 Ceti s. foll.	Double	25 12 n. pre.	90	Fl. 100 Herculis n. pre.	Double	75 9 s. foll.
59	Fl. 46 $\xi$ Aquarii s. foll.	Double	61 12 n. pre.	91	Fl. 15 $z$ Sagittæ s.	Treble	40 or 50 n. pre.
60	Fl. 5 $\xi$ Canis maj. n. foll.	Double	67 36 n. pre.	92	In Camelopard. clune	Double	22 42 s. foll.
61	Fl. 47 $\omega$ Orionis s. foll.	Treble	50 0 s. foll.	93	Fl. 13 $\epsilon$ Aquilæ s.	Double	16 0 n. pre.
62	Fl. 3 Pegasi adjēcta	Double	88 24 n. pre.	94	Fl. 17 $\iota$ Androm. n. pre.	Double	34 24 n. pre.
63	Fl. 2 $\&$ + Navis prec.	Multi.	30 12 n. foll.	95	Fl. 55 $\eta$ Aquilæ s.	Double	29 3 n. pre.
64	Fl. 81 $g$ Gemin. s. foll.	Double	4 9 n. pre.	96	Fl. 65 $\theta$ Aquilæ n. foll.	Double	56 12 s. pre.
65	Pollux s. foll.	Double	89 12 n. foll.	97	Fl. 64 $\zeta$ Cygni prec.	Treble	50 0 s. pre.
66	Near $\gamma$ Delphini	Double	78 42 n. foll.	98	Fl. 49 Cygni	Double	31 48 n. foll.
67	Fl. 10 $\beta$ Lyræ n. foll.	Double	68 6 s. foll.	99	Fl. 6 $\epsilon$ Cygni n. foll.	Double	87 48 n. foll.
68	Near $\epsilon$ Lyræ	Treble	65 12 s. foll.	100	Fl. 51 Cygni n. foll.	Double	15 51 n. foll.
69	Fl. 4 Cygni n. foll.	Double	29 12 n. foll.	101	Fl. 57 Camelopar. n. pre.	Double	67 15 n. pre.
70	Fl. 15 $z$ Sagittæ ult. foll.	Double	72 57 n. foll.	102	Fl. 29 $e$ Orionis s. prec.	Double	52 25 s. foll.

*Third Class of Double Stars.*

III.				III.			
47	Fl. 38 Gem. $e$ Pollux	Double	89° 54' s. foll.	67	Fl. 3 $\iota$ Leporis	Double	89 21 n. pre.
48	Fl. 61 $r$ Gemin. n. pre.	Double	43 54 n. foll.	68	Fl. 17 $\eta$ Arietis s. prec.	Double	55 42 s. foll.
49	Fl. 4 $\delta$ Hydræ n. prec.	Double	62 48 n. foll.	69	Near Fl. 64 Aquarii	Double	20 3 s. foll.
50	Fl. 51 $\theta$ Virginis	Treble	69 18 n. pre.	70	Fl. 1 $\kappa$ Cephei	Double	32 30 s. foll.
51	Fl. 88 Leonis	Double	47 33 n. pre.	71	Tiamam Cephei prec.	Treble	73 57 n. pre.
52	Fl. 10 Orionis foll.	Double	37 3 n. foll.	72	Tiamam Cephei prec.	Double	32 0 n. foll.
53	$\gamma$ Virginis n. foll.	Double	79 0 n. pre.	73	Fl. 25 Ceti s. foll.	Double	89 12 s. pre.
54	Fl. 13 2d to $\sigma$ Urs. maj.	Double	13 0 n. pre.	74	Fl. 18 Pegasi s.	Double	31 33 n. foll.
55	Fl. 18 $\nu$ Cor. bo. n. foll.	Double	64 24 n. foll.	75	Ad genam Monocerotis.	Double	
56	Fl. 72 S Serpent. n.	Double	9 42 s. pre.	76	$\delta$ Orionis 4th foll.	Double	13 6 n. pre.
57	In anseris corpore	Double	58 36 s. foll.	77	Fl. 65 Arietis s. foll.	Double	73 18 s. foll.
58	Fl. 13 $\theta$ Persei	Double	20 0 n. pre.	78	Fl. 13 Tauri s. prec.	Double	87 57 n. pre.
59	Near Fl. 19 Persei	Double	0 0 foll.	79	Fl. 83 $\epsilon$ Ceti n.	Double	45 12 s. pre.
60	Fl. 20 2d to $p$ Persei	Double	30 31 s. foll.	80	Fl. 76 $\sigma$ Ceti prec.	Double	22 24 n. pre.
61	Sub finem caudæ Draco.	Double	87 42 n. pre.	81	A little from $\zeta$ to $\epsilon$ Lyræ	Double	66 18 n. foll.
62	Fl. 35 Piscium	Double	58 54 s. foll.	82	Fl. 41 Aurigæ	Double	80 0 n. pre.
63	Near Fl. 65 Sagittarii	Double	73 48 n. foll.	83	Fl. 19 Lyncis	Double	45 54 s. pre.
64	Fl. 26 Aurigæ	Double	2 36 n. pre.	84	Fl. 40 Lyncis	Double	48 12 n. pre.
65	Fl. 58 $e$ Persei s.	Double	48 54 n. foll.	85	Fl. 2 Canum Venatic.	Double	11 0 s. pre.
66	Fl. 30 $e$ Tauri	Double	17 15 n. foll.	86	Fl. 57 Ursæ majoris	Double	75 36 n. foll.



No.	Name, or Flamsteed's marks, &c.	Multiple	Position, &c.	No.	Name, or Flamsteed's marks, &c.	Multiple	Position, &c.
III.				III.			
87	Fl. 59 Ursæ maj. n.	Treble	4° 0' n. foll.	102	Fl. 29 <i>h</i> Hercul. s. prec.	Double	67° 12' n. foll.
88	Fl. 11 Tauri n. foll.	Double	89 51 n. foll.	103	Fl. 37 <i>e</i> Serpen. n. foll.	Double	50 12 n. pre.
89	By 63 Herculis	Double	47 48 n. foll.	104	Fl. 83 Herculis pre.	Double	83 48 n. pre.
90	Fl. 103 Tauri n.	Double	64 0 n. foll.	105	Fl. 12 <i>γ</i> Sagitt. n. pre.	Double	50 24 s. pre.
91	Fl. 62 Arietis n. foll.	Double	12 24 n. pre.	106	Fl. 5 Serpentis	Double	30 or 40° n. foll.
92	Fl. 77 <i>ξ</i> Cancrī n. prec.	Double	65 12 s. pre.	107	Conger. Stell. Sagitt. n.	Double	54 48 s. pre.
93	Fl. 117 Tauri	Double	52 27 s. foll.	108	Fl. 19 Aquilæ n. prec.	Double	58 27 s. foll.
94	Fl. 7 <i>ν</i> Leporis n. prec.	Double	4 0 n. pre.	109	Fl. 19 Aquilæ n. prec.	Double	22 6 n. pre.
95	Fl. 48 Eridani s. prec.	Double	9 18 s. pre.	110	Fl. 77 Cygni n. prec.	Quadr.	40 33 n. foll.
96	Fl. 17 Crateris	Double	64 27 s. pre.	111	Fl. 46 <i>ε</i> Orionis n. foll.	Treble	
97	Fl. 54 Hydræ	Double	38 15 s. foll.	112	Fl. 18 <i>δ</i> Cygni s. foll.	Double	71 0 s. foll.
98	Ad genam Monocerotis.	Double	61 57 s. pre.	113	Fl. 27 Cygni s. prec. {	Quadr. & Sext.	} 57 12 n. fol.
99	Fl. 55 Eridani	Double	44 9 n. pre.	114	Fl. 16 Monocer. n. pre.	Double	
100	Fl. 55 Eridani s. prec.	Double	16 24 s. pre.				
101	Fl. 3 <i>k</i> Centauri	Double	22 0 s. foll.				

*Fourth Class of Double Stars.*

IV.				IV.			
45	In pectoris crate Orion.	Double	62 24 s. foll.	83	Fl. 26 Ceti	Double	14 36 s. pre.
46	Fl. 21 Geminorum	Double		84	Fl. 23 <i>m</i> Orionis	Double	59 33 n. foll.
47	Fl. 3 Leonis	Double	15 0 s. foll.	85	Fl. ultima Lacertæ	Treble	44 24 n. foll.
48	Fl. 1 H Gemin. n. prec.	Quint.	7 27 s. pre.	86	Fl. 8 Lacertæ	Quadr.	84 30 s. pre.
49	Fl. 4 <i>ξ</i> Virginis n. foll.	Double	56 30 s. pre.	87	Fl. 29 <i>e</i> Orionis prec.	Double	82 18 n. foll.
50	Fl. 17 Virginis	Double	58 21 n. pre.	88	Fl. 7 Tauri	Double	23 15 n. foll.
51	Fl. 44 <i>k</i> Virginis	Double	32 30 n. foll.	89	Ex caudam Arietis foll.	Double	62 0 s. foll.
52	Fl. 48 <i>ε</i> Cancrī	Double	39 54 n. pre.	90	By Fl. 18 Ursæ minoris	Double	3 12 n. foll.
53	Fl. 80 <i>π</i> Geminorum	Double		91	Fl. 2 Navis	Double	69 12 n. pre.
54	Fl. 4 <i>δ</i> Hydræ foll.	Double	59 24 n. foll.	92	Between <i>β</i> & <i>ζ</i> Delphini	Treble	18 27 n. pre.
55	Fl. 41 Lyncis foll.	Double	50 48 n. pre.	93	Fl. 4 <i>ε</i> Lyræ foll.	Double	24 0 s. pre.
56	Fl. 18 Libræ	Double	44 45 n. foll.	94	Ex <i>β</i> Lyræ n. prec.	Double	5 24 n. foll.
57	Fl. 42 Comæ Ber. s. foll.	Double	46 31 s. pre.	95	Fl. 25 Monocerotis pre.	Quadr.	foll.
58	Fl. 36 Comæ Ber. n. pre.	Double	67 57 s. pre.	96	Fl. 25 Monocerotis foll.	Double	24 0 s. pre.
59	Near <i>α</i> Lyræ	Double	59 12 s. pre.	97	Fl. 29 Monocerotis	Double	15 12 s. foll.
60	Fl. 4 Ursæ maj n. foll.	Double		98	Fl. 58 <i>α</i> Orionis s. prec.	Double	
61	Fl. 7 <i>ζ</i> Coron. aust. pre.	Double	4 57 n. foll.	99	Near <i>δ</i> Sagittæ s. foll.	Treble	10 36 s. pre.
62	Fl. 22 <i>ι</i> Herculis s. foll.	Double	72 15 s. pre.	100	Fl. 13 <i>χ</i> Sagittæ	Treble	10 or 15° n. pre.
63	Fl. 42 Herculis	Double	3 42 s. foll.	101	Fl. 24 <i>φ</i> Aurigæ n. prec.	Double	76 0 n. pre.
64	Fl. 12 near <i>q</i> Persei	Double	57 57 s. pre.	102	Fl. 59 Aurigæ	Double	50 3 s. pre.
65	Near Fl. 3 Cassiopeiæ	Double	41 12 s. foll.	103	Follow. Fl. 77 Draconis	Double	45 48 n. foll.
66	Fl. 33 <i>θ</i> Cassiop. prec.	Double	13 12 n. foll.	104	Betw. <i>γ</i> & 55 Androm.	Double	22 33 n. foll.
67	Fl. 40 and 41 Draconis	Double	30 0 s. foll.	105	Fl. 7 <i>δ</i> Corvi	Double	14 0 s. pre.
68	Fl. 77 Piscium	Double	4 48 n. foll.	106	Fl. 50 <i>α</i> Urs. ma. n. fol.	Double	44 33 s. foll.
69	Fl. 23 Andromedæ pre.	Double	70 36 n. pre.	107	Fl. 79 Pegasi s. prec.	Double	50 21 n. foll.
70	Fl. 51 Piscium	Double	0 36 n. foll.	108	Fl. 69 Ursæ majoris s.	Double	0 12 n. foll.
71	Fl. 12 <i>ο</i> Capricorni	Double	30 45 s. pre.	109	Fl. 62 Tauri	Double	21 12 n. pre.
72	Fl. 55 Persei n.	Double	27 24 n. foll.	110	Fl. 112 <i>β</i> Tauri n. foll.	Double	74 54 n. pre.
73	In constell. Camelopard.	Double	85 0 s. pre.	111	Fl. 54 Cancrī	Double	29 0 s. foll.
74	Fl. 68 <i>δ</i> Tauri n. foll.	Double	25 45 n. foll.	112	Fl. 15 <i>γ</i> Crateris n. foll.	Double	58 42 n. pre.
75	Fl. 66 <i>r</i> Tauri foll.	Double	61 36 s. foll.	113	Fl. 61 Cygni n. prec.	Double	28 24 n. pre.
76	Fl. 13 Ceti s. p. rec.	Double	40 24 n. foll.	114	Fl. 12 <i>t</i> Virginis s.	Double	15 54 n. pre.
77	Fl. 37 Ceti n.	Double	63 24 n. pre.	115	Fl. 11 <i>ι</i> Herculis s. pre.	Double	43 48 n. foll.
78	Fl. 3 <i>ν</i> Cephei prec.	Double	40 36 n. foll.	116	Fl. 83 Pegasi n. foll.	Double	68 21
79	Fl. 13 <i>μ</i> Cephei	Double	77 48 s. pre.	117	Fl. 42 Eridani s.	Double	31 48 s. pre.
80	Fl. 2 <i>β</i> canis majoris n.	Double	2 24 n. foll.	118	Fl. 48 <i>ε</i> Cancrī foll.	Double	25 0 n. foll.
81	Fl. 6 <i>ν</i> canis majoris	Double	prec.	119	Fl. 68 <i>ι</i> Virginis s. prec.	Double	36 54 n. pre.
82	Near Fl. 16 Cephei	Double	79 18 n. pre.	120	Fl. 82 Piscium n. foll.	Double	21 0 s. pre.



No.	Name, or Flamsteed's marks, &c.	Multiple	Position, &c.	No.	Name, or Flamsteed's marks, &c.	Multiple	Position, &c.
IV.				IV.			
121	Fl. 20 $\sigma$ Scorpii	Double	1° 0' n. pre.	127	Fl. 16 $\lambda$ Aquilæ n. foll.	Double	69° 54' n. pr.
122	Fl. 32 Ophiuchi n. prec.	Double	25 3 s. pre.	128	Fl. 57 $\gamma$ Andro. s. prec.	Double	24 12 n. foll.
123	Fl. 19 Ophiuchi	Double	3 9 s. foll.	129	Fl. 59 Andromedæ	Double	38° or 60° s. pr.
124	Fl. 4 $\psi$ Ophiuchi s. pre.	Double	62 54 n. foll.	130	Fl. 99 $\pi$ Piscium n. foll.	Double	62 15 n. foll.
125	Fl. 29 Camelopard.	Double	47 36 s. foll.	131	Fl. 100 Piscium.	Double	5 0 n. foll.
126	Fl. 22 $\lambda$ Cephei n. prec.	Double	45 39 n. pre.	132	Fl. 46 Aquilæ n. foll.	Double	41 24 n. pre.

*Fifth Class of Double Stars.*

V.				V.			
52	2d from $\nu$ to $\mu$ Gemin.	Double		95	Fl. 51 Aquarii	Double	
53	Fl. 63, $p$ Geminorum	Double		96	Fl. 59, $\nu$ Aquarii s. foll.	Double	15 or 20 s. pr.
54	Fl. 22, $\theta$ Hydræ	Double	75 0 s. foll.	97	Fl. 10 Lacerta	Double	38 45 n. foll.
55	By Fl. 12 Geminorum	Treble		98	Fl. 3 Pegasi	Double	
56	Fl. 15 Geminorum	Double	60 0 s. pre.	99	Fl. 33 Pegasi	Double	89 12 n. foll.
57	Fl. 9 Orionis n. foll.	Treble	33 36	100	Fl. 59 Orionis	Double	65 0 s. pre.
58	Fl. 7 Leonis	Double	8 36 n. foll.	101	Fl. 36, $\nu$ Orionis prec.	Double	15 0 s. foll.
59	Fl. 31, $\theta$ Cancri	Double	n. foll.	102	Fl. 61 Ceti	Double	76 21 s. pre.
60	Fl. 95, $\sigma$ Leonis prec.	Double	70 48 n. foll.	103	Fl. 18, fr. $\epsilon$ tow. $\beta$ Lyra	Double	29 12 n. foll.
61	Fl. 81 Leonis	Double		104	Fl. 4, $\epsilon$ Sagittæ s. prec.	Double	16 18 s. foll.
62	Fl. 57 Leonis	Double		105	Fl. 14, $\gamma$ Sagittæ s. foll.	Double	74 15 s. foll.
63	Fl. 25 Leonis	Double		106	Fl. 12, $\gamma$ Sagittæ n. prec.	Double	60 42 n. pre.
64	Fl. 43 Leonis $s$	Double		107	Fl. 56 Aurigæ	Double	72 36 n. foll.
65	Fl. 17, 2d to $\pi$ Canis maj.	Treble	85 0 s. pre.	108	Fl. 13, $\pi$ Canis maj. n.	Double	23 18 n. foll.
66	Fl. 63, $p$ Geminorum n.	Double	1° or 2° n. pre.	109	Bet. $\beta$ Cancri & $\delta$ Hydræ	Double	55 0 n. pre.
67	Near Pollux	Double		110	Fl. 111 Tauri	Double	3 48 n. pre.
68	Fl. 75 Leonis n. prec.	Treble		111	Fl. 42 Ursæ maj. s. foll.	Double	51 27 n. foll.
69	Fl. 7 Leonis minoris	Double		112	Ex. $\mu$ & $\nu$ Gemin. foll.	Double	
70	Fl. 2 Bootis n. prec.	Double	7 0 s. pre.	113	Betw. fl. 9 & 11 Orionis	Treble	33 54 n. pre.
71	Fl. 24, near $\gamma$ Gemin.	Double		114	Fl. 103 Tauri	Double	72 24
72	Fl. 36 & 37, $m$ Herculis	Double	36 57 s. pre.	115	Fl. 114, $\sigma$ Tauri	Double	77 54 s. pre.
73	Fl. 14, $\tau$ Ursæ majoris	Double	45 0 n. foll.	116	Fl. 41 Arietis	Treble	80 48 s. pre.
74	Fl. 72, S Serpentarii n.	Double	39 15	117	Fl. 58, $\zeta$ Arietis, n. prec.	Double	47 33 n. pre.
75	Ex. $\epsilon$ Coronæ borealis fol.	Double	16 0 s. foll.	118	Fl. 46, $\epsilon$ Orionis n. prec.	Double	13 6 s. pre.
76	Fl. 22, $\beta$ Aquarii	Double	55 48	119	Fl. 46, $\epsilon$ Orionis s. prec.	Double	21 33 s. pre.
77	Fl. 43, $d$ Sagittarii n. fol.	Double	78 45 s. foll.	120	Fl. 15 Hydræ	Double	70 0 n. pre.
78	Fl. 48, $\zeta$ Sagittarii	Double	28 6 n. pre.	121	Fl. 12, $c$ Comæ Beren.	Double	77 0 s. foll.
79	Fl. 9 Cassiopeiæ	Double	50 36 n. pre.	122	Fl. 44, Bootis s. prec.	Double	67 6 s. pre.
80	Fl. 69, $\tau$ Aquarii	Double	19 54 s. foll.	123	In Andromedæ pectore	Double	32 24 s. pre.
81	Fl. 35 Cassiopeiæ	Double	85 12 n. foll.	124	Fl. 2, $g$ Centauri s. foll.	Double	
82	Fl. 25, $\epsilon$ Cassiopeiæ prec.	Double	7 48 n. foll.	125	Fl. 46 Bootis n. foll.	Double	37 33 s. pre.
83	Fl. 36, $\psi$ Cassiopeiæ	Double	10 12 s. foll.	126	Fl. 5, $r$ Herculis s. prec.	Double	52 6 s. pre.
84	Fl. 47 Cassiopeiæ	Double	3 33 n. pre.	127	Fl. 41 Herculis n. prec.	Double	19 45 n. pre.
85	Fl. 27, $\epsilon$ Cassiopeiæ	Double	79 24 n. foll.	128	Fl. 68, $\epsilon$ Virginis foll.	Double	
86	Fl. 12 Ursæ minoris	Treble		129	Fl. 25, $f$ Virginis n. foll.	Double	6° or 7° s. foll.
87	Fl. 7, $\sigma$ Capricorni	Double	85 12 s. foll.	130	Fl. 35 Comæ Berenices	Double	36 51 s. foll.
88	Fl. 15, $\lambda$ Aurigæ n.	Double	54 6 s. pre.	131	Fl. 21 Libræ n. foll.	Double	
89	Fl. 37, $\theta$ Aurigæ	Double	16 0 n. pre.	132	Betw. fl. 29 & 30 Libræ	Double	
90	Fl. 32 Aurigæ	Double	61 48 s. pre.	133	Fl. 60 Herculis	Double	37 0 n. pre.
91	Fl. 34, $\beta$ Aurigæ adjunct.	Double	45 6 n. pre.	134	Fl. 4, $\psi$ Ophiuchi s. pre.	Double	
92	Fl. 3 Arietis n.	Double	52 45 s. foll.	135	By fl. 49 Camelopardali	Double	85 0 s. pre.
93	Fl. 103 Herculis s. foll.	Double	45 42 s. foll.	136	Fl. 65, $\theta$ Aquilæ n.	Double	65 48 s. pre.
94	Fl. 31 Cephei s. of 2 fol.	Double	45 15 s. foll.	137	Fl. 17, $\gamma$ Cygni n.	Double	57 3 n. foll.



*Sixth Class of Double Stars.*

No.	Name, or Flamsteed's marks, &c.	Multiple	Position, &c.	No.	Name, or Flamsteed's marks, &c.	Multiple	Position, &c.
VI.				VI.			
67	Fl. 28, $\eta$ Orionis	Double	35 12 n. foll.	95	Fl. 8, Bootis	Double	25° or 30° s. fol.
68	Fl. 28, $\eta$ Orionis s.	Double	7 54 n. pre.	96	Fl. 44, $\zeta$ Persei	Treble	66 36 s. pre.
69	Fl. 14 Arietis	Double	11 12 n. pre.	97	Fl. 71, 2d to $\tau$ Aquarii	Double	18 30 n. pre.
70	Fl. 70, $\circ$ Geminorum	Treble		98	Fl. 46 Tauri s. foll.	Double	43 48 n. pre.
71	Fl. 31, $\tau$ Hydræ	Double	88 36 n. pre.	99	Fl. 57, $m$ Persei	Double	71 51 s. pre.
72	By fl. 68 Orionis	Double	41 0 s. pre.	100	Fl. 32, $\iota$ Cephei foll.	Double	8 9 n. pre.
73	Fl. 27, $\epsilon$ Geminorum	Double		101	Fl. 68, $\delta$ Tauri	Double	50 0 n. pre.
74	Fl. 51 Geminorum	Double	40° or 50° n. fol.	102	Fl. 5 Lyncis	Double	2 0 n. pre.
75	Fl. 4, $\omega$ Cancri	Double	30 0 n. pre.	103	Fl. 8, $\epsilon$ Pegasi	Double	52 45 n. pre.
76	Fl. 14, $\circ$ Leonis	Double	49 36 n. foll.	104	Fl. 30, $\zeta$ Bootis	Double	
77	Fl. 93, $\tau$ Virginis	Double		105	Fl. 105 Tauri	Double	18 0 s. pre.
78	Fl. 16, $\zeta$ Cancri foll.	Double		106	Fl. 62, $b$ Eridani	Double	15 9 n. foll.
79	Fl. 74, $\phi$ Leonis	Double	10° or 12° n. pr.	107	Fl. 31 Monocerotis s. pr.	Double	50° or 60° s. fol.
80	Fl. 93 Leonis	Double		108	Fl. 22, $\theta$ Hydræ n. prec.	Double	1 or 2 n. pre.
81	Fl. 27 Virginis	Double		109	Either fl. 22 or 26 Cancri	Double	
82	Fl. 31 Monocerotis	Double	40 0 n. pre.	110	By $\circ$ Ceti	Double	33 42
83	Near fl. 1 Orionis	Double	88 15 n. foll.	111	Fl. 30, $\alpha$ Hydræ	Double	
84	Fl. 14 Canis majoris	Treble	26 24 n. foll.	112	Fl. 13 Bootis	Double	7 24 n. pre.
85	Fl. 27 Hydræ	Double	60 0 s. pre.	113	Fl. 4 Virginis	Double	
86	Fl. 51, 1st to $\sigma$ Cancri	Double	n. foll.	114	Fl. 69 Orionis s. prec.	Double	22 6 s. foll.
87	Fl. 64, 3d to $\sigma$ Cancri	Double	25 12 n. pre.	115	Fl. 21 Crateris s. foll.	Double	12 12 n. foll.
88	Fl. 34, $\beta$ Aurigæ	Double	40° or 50° n. fol.	116	Fl. 43 Herculis	Double	38 48 s. pre.
89	Fl. 6 Bootis adjecta	Double	58 6 s. pre.	117	Fl. 12 Libræ n. prec.	Double	40 0 s. pre.
90	Fl. 61 Virginis	Double	75 0 n. pre.	118	Fl. 30 Monocerotis	Double	
91	Fl. 24, near $\gamma$ Gemin.	Double		119	Fl. 18, $\epsilon$ Piscis. austs. pr.	Double	67 46 s. foll.
92	Fl. 1, $\xi$ Capricorni n.	Double	2 3 s. pre.	120	Fl. 43 Sagittarii s. foll.	Double	37 0 n. pre.
93	Fl. 15, $\epsilon$ Coronæ borealis	Double	54 27 s. foll.	121	Fl. 12 Lacertæ	Double	73 0 n. foll.
94	Fl. 12, $\lambda$ Coronæ borealis	Double	33 12 n. foll.				

*VII. Observations of a New Variable Star. By Edward Pigott, Esq. p. 127.*

For some years past I have been employed in verifying all the stars suspected to be variable, that hereafter we may know with certainty what to depend on. This undertaking, which is nearly completed, has already proved of use in detecting many mistakes, and producing some discoveries; among which, the following is one of the most important. Sept. 10, 1784, I first perceived a change in the brightness of the star  $\eta$  Antinoi, and by a series of observations made ever since, I find it subject to a variation very similar to that of Algol, though not exactly the same in any one particular.  $\eta$  Antinoi, when brightest, is of the 3d or 4th magnitude, being between  $\delta$  and  $\beta$  Aquilæ; and at its least brightness of the 4th or 5th magnitude, being then between that of  $\iota$  Antinoi and  $\mu$  Aquilæ; therefore its greatest variation in brightness may be accounted about one magnitude; and the changes it undergoes, though probably not nicely ascertained from so few observations, are nearly these annexed: all these changes, which hitherto seem to be regular and constant, are performed in 7<sup>d</sup> 4<sup>h</sup> 38<sup>m</sup>; and this I shall stile its period.

At its greatest brightness 44  $\pm$  hours.  
 In decreasing..... 62  $\pm$  .....  
 At its least brightness .. 30  $\pm$  .....  
 In increasing..... 36  $\pm$  .....



The stars to which  $\eta$  Antinoi was compared are in order thus:  $\delta$  Aquilæ 3d magnitude,  $\beta$  Aquilæ and  $\theta$  Serpentis 4th magnitude,  $\iota$  Antinoi 4th or 5th magnitude, and  $\mu$  Aquilæ a bright 5th. I find, by several years observation, that  $\beta$  Aquilæ retains the same brightness.  $\iota$  Antinoi, which has been examined with particular attention by Mr. Goodricke and myself, is suspected by us both to be subject to a small variation, but not so apparent as to affect materially these comparisons, and possibly it may be only the effect of some optical illusion; for I have frequently remarked, that both in the twilight and moon-light, or when the air is in the least hazy, there is a greater difference between the brightness of many of the stars, than in a dark night and clear sky. Mr. P. then gives a journal of the comparisons with those stars, from July 17 till Dec. 4. And then proceeds: In order to obtain a point of comparison, for settling the periodical changes of  $\eta$  Antinoi, which I suppose to be constant, it is natural to fix on that phasis which can be determined with the greatest precision; and this seems to be at the time when it is between its least and greatest brightness, as almost the whole increase of brightness is completed in less than 24 hours, though the perfect completion is performed only in  $36 \pm$  hours; thus having settled this necessary point, and found roughly the length of a single period, the computations, in order to obtain greater exactness, are as annexed.

Time when $\eta$ Antinoi was between its least and greatest brightness.	Intervals between the observations.	Numb. of periods in ditto.	Length of a single period.
1784 h	d h		d h
Sept. 12, at 20 } Oct. 11, at 11 }	28 15	4 each of	7 $3\frac{3}{4}$
Sept. 12, at 20 } Oct. 18, at 20 }	36 0	5 .....	7 $4\frac{3}{4}+$
Sept. 12, at 20 } Oct. 26, at 00 }	43 4	6 .....	7 $4\frac{3}{4}-$
Sept. 12, at 20 } Nov. 16, at 8 }	64 12	9 .....	7 4
Sept. 19, at 20 } Oct. 18, at 20 }	29 0	4 .....	7 6
Sept. 19, at 20 } Oct. 26, at 00 }	36 4	5 .....	7 $5\frac{1}{2}+$
Sept. 19, at 20 } Nov. 16, at 8 }	57 12	8 .....	7 $4\frac{1}{2}$
Oct. 11, at 11 } Nov. 16, at 8 }	35 21	5 .....	7 $4\frac{1}{4}-$
Oct. 18, at 20 } Nov. 16, at 8 }	28 12	4 .....	7 3
Length of a single period, on a mean,			7 4 30

Perhaps other astronomers may not exactly agree with me, in fixing the times as set down in the first column; for my part, I determined them without paying any regard to the results, by taking a medium between the times when  $\eta$  Antinoi had rather passed its least brightness, being nearly equal to  $\iota$  Antinoi, and when it was a little, but undoubtedly, brighter than  $\beta$  Aquilæ. Though it does not appear, as already said, that any of the other phases can be settled with equal precision, yet different comparisons may prove satisfactory towards



corroborating the above; I have therefore also deduced its period from the best and most distant observations, made when at its least brightness; they are thus:  $7^d 0^h$  and  $7^d 5^h$ . These results I reject, and retain the mean given by the first set, with which we may proceed on to gain a much greater exactness. After making other comparisons, Mr. P. states the results for the periods as annexed:

$7^d$	$4^h$	$39\frac{1}{2}^m$
7	4	$44\frac{1}{2}$
7	4	$53\frac{1}{3}$
7	4	$54\frac{2}{3}$
7	4	32
7	4	$26\frac{1}{2}$
7	4	32
7	4	$42\frac{1}{2}$
7	4	43—
7	4	26
7	4	$21\frac{1}{2}$
On a mean length of } a single period.		
7	4	38

As this approaches the most to the preceding result, it may be assumed as nearest the truth, provided the changes be uniformly periodical.

*VIII. Astronomical Observations. By M. Francis de Zach, Professor of Mathematics, &c. p. 137.*

This paper contains an account of the observations on the eclipse of the moon, made in the Observatory at Lyons, called au grand Collège; also observations of the vernal equinox; some observations on Jupiter's satellites, made at Marseilles by M. Saint Jacques de Sylvabelle; and lastly a new solution of a problem that occurs in computing the orbits of comets. The lunar eclipse was a total one on March 18, 1783. The beginning  $7^h 53^m 39^s$  ap. time; total immersion  $8^h 50^m 55^s$ ; beginning of the emersion  $10^h 32^m 2^s$ ; end of the eclipse  $11^h 32^m 18^s$ , whole duration  $3^h 39^m 0^s$ . After this follow some very few observations of Jupiter's satellites, of no use now; and then, with regard to the problem on the orbits of comets, Mr. Z. says, it is known, that the indirect method to calculate the orbits of comets in a conic section, by means of 3 observations given, is rendered more easy and expeditious if there is a possibility of drawing a graphical figure that represents nearly the orbit under consideration by means of which the calculation is directed, and the required elements of the comet's path may be rigorously determined. To draw the orbit of a comet that moves in a parabola or ellipsis, the problem is reduced to find the position of the axis and the perihelial distance; this position of the axis will be determined as soon as the angle is known that the axis forms with another line whose position is given; this line may be an ordinate to a given point of the curve, or a tangent, or a radius vector, &c. The latter is to be employed in preference, because the perihelial distance being a constant quantity, the angle of position then becomes the true anomaly of the comet; but as the data of this problem are only geocentric longitudes and latitudes of the comet, deduced from the immediate observations of right ascension and declination, the heliocentric longitudes and latitudes must first be calculated; but as those data are not sufficient, what is not given must be arbitrarily supposed, viz. the shortened distances (dis-



tantias curtatas.) This supposition is changed and altered till the calculation will agree with the 3 observations, then the difference between 2 longitudes is the angle comprehended between the 2 shortened distances in the plane of the ecliptic; the whole reduced to the plane of the comet's orbit by means of the heliocentric latitude, gives the difference between the anomalies comprehended by 2 radius vectors, the problem then is reduced to 2 radius vectors being given, with the angle comprehended, to find the 2 true anomalies, the perihelial distance, and the time the comet takes in running its anomalies. Mr. Z. then gives the algebraical solution at full length, which is to be found in the authors on astronomy.

With regard to the transit of Mercury, which happened Nov. 12, 1782, it is remarked that the sky not being very favourable, only the two internal contacts were observed; the first internal contact was observed by M. St. Jacques de Sylvabelle, at  $3^h 18^m 30^s$  apparent time; the last internal contact by the same, at  $4^h 30^m 16^s$ ; by M. Bernard, his assistant, at  $4^h 29^m 13^s$ . The nearest distances of Mercury's limb to that of the sun in the northern part of its disc were, at

$3^h 33^m 14^s$	31
$3^h 42^m 57^s$	34
$4^h 22^m 17^s$	19

Parts of the micrometer

The apparent diameter of the sun was 2174 parts of this micrometer: I suppose the before-mentioned 2174 parts =  $32' 26''.9$ . I conclude further, by the observations, the middle of the transit at  $3^h 54^m 7^s.25$ , whereas I fix, by interpolation, the distances of the limbs at  $3^h 54^m 7^s.25 = 35''.6$ ; I have therefore semi-diameter of the sun =  $16' 13''.4 - 35''.6 = 15' 37''.8$  + semi-diameter of Mercury  $6'' = 15' 43''.8$  = to the least distance of centres of the sun and Mercury. By M. De La Lande's tables it is  $15' 42''$ , only a difference of  $1''.8$ .

M. Wallot at Paris has observed this transit at the Royal Observatory.

Mr. Z. then adds a remark on the diameter of Mercury, which the astronomers supposed in this transit =  $12''$ . Let ABC, fig. 9, pl. 8, represent the sun's disc; in p an external, in a an internal contact; ANC the apparent path of Mercury over the sun. The semi-diameter of the sun =  $972''$ , this of Mercury in our supposition =  $6''$ , MN =  $942''$  the least distances of the centres. In the right-angled triangle MNP it is  $MP = 972'' + 6'' = 978''$ ,  $MQ = 972'' - 6'' = 966''$ ; therefore NP will be found =  $260''$ , and  $NQ = 210''$ : now  $NP - NQ = PQ = 50''$ , which converted into time gives  $8^m 14^s$  for the time the diameter of Mercury employed to run over the sun's limb; but by the observations of M. Wallot I find this time constantly in both contacts  $5^m 35^s$ ; therefore  $8^m 14^s : 12'' :: 5^m 35^s : 8''.137$ , which should be the diameter of Mercury; and indeed M. Wallot, by an immediate measure, taken with an excellent wire-micrometer, finds this apparent

First external contact..	$2^h 56^m 28^s$
First internal contact..	$3 \quad 2 \quad 3$
Second .....	$4 \quad 17 \quad 18$
Second external.....	$4 \quad 22 \quad 53$



diameter not greater than  $9''$ , which sufficiently shows that this diameter supposed  $7''$  in the mean distance is also too great.

*IX. Observations of a New Variable Star. By John Goodricke, Esq. p. 153.*

On Sept. 10, 1784, while my attention was directed towards that part of the heavens where  $\beta$  Lyræ was situated, I was surprized to find this star much less bright than usual, on which I suspected that it might be a variable star: my suspicions were afterwards confirmed by a series of observations, which have been regularly continued since that time, and which will presently follow in their proper place. At first I thought the light of this star subject to a periodical variation of nearly 6 days and 9 hours, though the degree of its diminution did not then appear to be constant; but now, on a more close examination of the observations themselves, I am inclined to think, that the extent of its variation is 12 days and 19 hours, during which time it undergoes the following changes.

1. It is of the 3d magnitude for about 2 days.—2. It diminishes in about one day and a quarter.—3. It is between the 5th and 4th magnitude for less than a day.—4. It increases in about 2 days.—5. It is of the 3d magnitude for about 3 days.—6. It diminishes in about 1 day.—7. It is something larger than a star of the 4th magnitude for little less than a day.—8. It increases in about  $1\frac{3}{4}$  day to the 1st point, and so completes a whole period.

These 8 points of the variation are perhaps inaccurately ascertained; and indeed it cannot be expected to be otherwise in estimations of this nature, where it is very possible to err even several hours. The magnitudes of the stars to which  $\beta$  Lyræ was compared during the progress of its variation, are as follow:  $\beta$  Cygni and  $\gamma$  Lyræ of the 3d magnitude;  $\xi$  and  $\theta$  Herculis of between the 4th and 3d magnitude;  $\circ$  Herculis is something less than a star of the 4th magnitude;  $\zeta$ ,  $\kappa$ , and  $\delta$  Lyræ are stars of between the 4th and 5th magnitude, if not nearer the 5th. The relative brightness of these stars follows the order in which they are set down. Then follows a series of daily observations and comparisons, from Sept. 10, 1784, till Jan. 6, 1785. From which Mr. G. deduced his conclusions relative to the 8 points of the variation, as above stated.

With regard to the period of the variation, Mr. G. proceeds, it is evident from a collation of the preceding observations in a coarse way, that it is nearly 12 days and  $\frac{3}{4}$ . To determine it with greater accuracy is a subject of considerable difficulty, in the present case; for unless we can obtain very exact points of comparison, the period would come out erroneous, especially if deduced from intervals consisting of only a very few periods, as is the case here. However, as I have been able to obtain a few observations of the middle of its obscuration in the 3d point accurate enough for our purpose, I have formed the following calculation.



Times of the middle of its obscura-  
tion in the 3d point.

1784, Oct. 6	....	1 <sup>h</sup>	}	only a single period of	12 <sup>d</sup>	21 <sup>h</sup>
18	....	22				
18	....	22	}	.....	12	17
31	....	15				
6	....	1	}	2 periods, each of....	12	19
31	....	15				

Hence the period on a mean is  $\frac{12 \quad 19 \pm}{2}$

In ascertaining the above times, I attended particularly to the nearest observations both preceding and following. In the manner above stated the period may also be deduced from the middle of its obscuration in the 7th point; but as these observations are not so exact as the above, I shall only, as a further confirmation, compare 2 of the most distant of them, viz. Sept. 29, 22<sup>h</sup> and Nov. 20, 6<sup>h</sup>, which interval I find contains 6 periods, each of 12<sup>d</sup> 20<sup>h</sup>  $\pm$ .

*X. On the Motion of Bodies affected by Friction. By the Rev. S. Vince, A. M.*  
p. 165.

The law by which the motions of bodies are retarded by friction has never, that I know of, been truly established. Musschenbroek says, that in small velocities the friction varies very nearly as the velocity, but that in great velocities the friction increases; he has also attempted to prove, that by increasing the weight of a body the friction does not always increase exactly in the same ratio; and that the same body, if by changing its position you change the magnitude of the surface on which it moves, will have its quantity of friction also changed. Helsham and Ferguson, from the same kind of experiments, have endeavoured to prove, that the friction does not vary by changing the quantity of surface on which the body moves; and the latter of these asserts, that the friction increases very nearly as the velocity; and that by increasing the weight, the friction is increased in the same ratio. These different conclusions induced me to repeat their experiments, in order to see how far they were conclusive in respect to the principles deduced from them: when it appeared, that there was another cause operating besides friction, which they had not attended to, and which rendered all their deductions totally inconclusive. Of those who have written on the theory, no one has established it altogether on true principles: Euler (whose theory is extremely elegant, and which, as he has so fully considered the subject, would have precluded the necessity of offering any thing further, had its principles been founded on experiments) supposes the friction to vary in proportion to the velocity of the body, and its pressure on the plane; neither of which are true: and others, who have imagined that friction is a uniformly retarding force (and which conjecture will be confirmed by our experiments,) have still retained the other supposition, and therefore rendered their solutions not at all applicable



to the cases for which they were intended. I therefore endeavoured by a set of experiments to determine,

1st, Whether friction be a uniformly retarding force.

2dly, The quantity of friction.

3dly, Whether the friction varies in proportion to the pressure or weight.

4thly, Whether the friction be the same on whichever of its surfaces a body moves.

The experiments, in which I was assisted by my ingenious friend the Rev. Mr. Jones, Fellow of Trinity College, were made with the utmost care and attention, and the several results agreed so very exactly with each other, that I do not scruple to pronounce them to be conclusive.

2. A plane was adjusted parallel to the horizon, at the extremity of which was placed a pulley, which could be elevated or depressed in order to render the string which connected the body and the moving force parallel to the plane. A scale accurately divided was placed by the side of the pulley perpendicular to the horizon, by the side of which the moving force descended; on the scale was placed a moveable stage, which could be adjusted to the space through which the moving force descended in any given time, which time was measured by a well regulated pendulum clock vibrating seconds. Every thing being thus prepared, the following experiments were made to ascertain the law of friction. But let me first observe, that if friction be a uniform force, the difference between it and the given force of the moving power must be also uniform, and therefore the moving body must descend with a uniformly accelerated velocity, and consequently the spaces described from the beginning of the motion must be as the squares of the times, just as when there was no friction, only they will be diminished on account of the friction.

3. *Exper. 1.* A body was placed on the horizontal plane, and a moving force applied, which from repeated trials was found to descend  $52\frac{1}{2}$  inches in  $4^s$ ; for by the beat of the clock and the sound of the moving force when it arrived at the stage, the space could be very accurately adjusted to the time; the stage was then removed to that point to which the moving force would descend in  $3^s$ , on supposition that the spaces described by the moving power were as the squares of the times; and the space was found to agree very accurately with the time; the stage was then removed to that point to which the moving force ought to descend in  $2^s$ , on the same supposition, and the descent was found to agree exactly with the time; lastly, the stage was adjusted to that point to which the moving force ought to descend in  $1^s$ , still on the same supposition, and the space was observed to agree with the time. Now, in order to find whether a difference in the time of descent could be observed, by removing the stage a little above and below the positions which corresponded to the above times, the experiment was



tried, and the descent was always found too soon in the former, and too late in the latter case; by which I was assured that the spaces first mentioned corresponded exactly to the times. And, for the greater certainty, each descent was repeated 8 or 10 times; and every caution used in this experiment was also made use of in all the following.

*Exper. 2.* A 2d body was laid on the horizontal plane, and a moving force applied which descended  $41\frac{3}{4}$  inches in  $3^s$ ; the stage was then adjusted to the space corresponding to  $2^s$ , on supposition that the spaces descended through were as the squares of the times, and it was found to agree accurately with the time; the stage was then adjusted to the space corresponding to  $1^s$ , on the same supposition, and it was found to agree with the time.

*Exper. 3.* A third body was laid on the horizontal plane, and a moving force applied, which descended  $59\frac{5}{8}$  inches in  $4^s$ ; the stage was then adjusted to the space corresponding to  $3^s$ , on supposition that the spaces descended through were as the squares of the times, and it was found to agree with the time; the stage was then adjusted to the space corresponding to  $2^s$ , on the same supposition, and it was found to agree with the time; the stage was then adjusted to the space corresponding to  $1^s$ , and was found to agree with the time.

*Exper. 4.* A 4th body was then taken and laid on the horizontal plane, and a moving force applied, which descended 55 inches in  $4^s$ ; the stage was then adjusted to the space through which it ought to descend in  $3^s$ , on supposition that the spaces descended through were as the squares of the times, and it was found to agree with the time; the stage was then adjusted to the space corresponding to  $2^s$ , on the same supposition, and was found to agree with the time; lastly, the stage was adjusted to the space corresponding to  $1^s$ , and it was found to agree exactly with the time.

Besides these experiments, a great number of others were made with hard bodies, or those whose parts so firmly cohered as not to be moved inter se by the friction; and in each experiment bodies of very different degrees of friction were chosen, and the results all agreed with those related above; we may therefore conclude, that the friction of hard bodies in motion is a uniformly retarding force.

But to determine whether the same was true for bodies when covered with cloth, woollen, &c. experiments were made in order to ascertain it; when it was found in all cases, that the retarding force increased with the velocity; but on covering bodies with paper, the consequences were found to agree with those related above.

4. Having proved that the retarding force of all hard bodies arising from friction is uniform, the quantity of friction, considered as equivalent to a weight without inertia drawing the body on the horizontal plane backwards, or acting



contrary to the moving force, may be immediately deduced from the foregoing experiments. For let  $M$  = the moving force expressed by its weight;  $F$  = the friction;  $w$  = the weight of the body on the horizontal plane;  $s$  = the space through which the moving force descended in the time  $t$  expressed in seconds;  $r = 16\frac{1}{2}$  feet; then the whole accelerative force (the force of gravity being unity) will be  $\frac{M - F}{M + w}$ ; hence, by the laws of uniformly accelerated motions,  $\frac{M - F}{M + w} \times rt^2 = s$ , consequently  $F = M - \frac{(M + w) \times s}{rt^2}$ . To exemplify this, let us take the case of the last experiment, where  $M = 7$ ,  $w = 25\frac{3}{4}$ ,  $s = 4\frac{7}{8}$  feet,  $t = 4''$ ; hence  $F = 7 - \frac{32\frac{3}{4} \times 4\frac{7}{8}}{16\frac{1}{2} \times 16} = 6.417$ ; consequently the friction was to the weight of the rubbing body as 6.417 to 25.75. And the great accuracy of determining the friction by this method is manifest from hence, that if an error of 1 inch had been made in the descent (and experiments carefully made may always determine the space to a much greater exactness) it would not have affected the conclusion  $\frac{1}{600}$  part of the whole.

5. We come in the next place to determine, whether friction, *cæteris paribus*, varies in proportion to the weight or pressure. Now if the whole quantity of the friction of a body, measured by a weight without inertia equivalent to the friction drawing the body backwards, increases in proportion to its weight, it is manifest, that the retardation of the velocity of the body arising from the friction will not be altered; for the retardation varies as  $\frac{\text{Quantity of friction}}{\text{Quantity of matter}}$ ; hence, if a body be put in motion on the horizontal plane by any moving force, if both the weight of the body and the moving force be increased in the same ratio, the acceleration arising from that moving force will remain the same, because the accelerative force varies as the moving force divided by the whole quantity of matter, and both are increased in the same ratio; and if the quantity of friction increases also as the weight, then the retardation arising from the friction will, from what has been said, remain the same, and therefore the whole acceleration of the body will not be altered; consequently the body ought, on this supposition, still to describe the same space in the same time. Hence, by observing the spaces described in the same time, when both the body and the moving force are increased in the same ratio, we may determine whether the friction increases in proportion to the weight. The following experiments were therefore made in order to ascertain this matter.

*Exper. 1.* A body weighing 10 oz. by a moving force of 4 oz. described in  $2^s$  a space of 51 inches; by loading the body with 10 oz. and the moving force with 4 oz. it described 56 inches in  $2^s$ ; and by loading the body again with 10 oz. and the moving force with 4 oz. it described 63 inches in  $2^s$ .

*Exper. 2.* A body, whose weight was 16 oz. by a moving force of 5 oz. de-



scribed a space of 49 inches in  $3^s$ ; and by loading the body with 64 oz. and the moving force with 20 oz. the space described in the same time was 64 inches.

*Exper. 3.* A body weighing 6 oz. by a moving force of  $2\frac{1}{2}$  oz. described 28 inches in  $2^s$ ; and by loading the body with 24 oz. and the moving force with 10 oz. the space described in the same time was 54 inches.

*Exper. 4.* A body weighing 8 oz. by a moving force of 4 oz. described  $33\frac{1}{2}$  inches in  $2^s$ ; and by loading the body with 8 oz. and the moving force with 4 oz. the space described in the same time was 47 inches.

*Exper. 5.* A body whose weight was 9 oz. by a moving force of  $4\frac{1}{2}$  oz. described 48 inches in  $2^s$ ; and by loading the body with 9 oz. and the moving force with  $4\frac{1}{2}$  oz. the space described in the same time was 60 inches.

*Exper. 6.* A body weighing 10 oz. by a moving force of 3 oz. described 20 inches in  $2^s$ ; by loading the body with 10 oz. and the moving force with 3 oz. the space described in the same time was 31 inches; and by loading the body again with 30 oz. and the moving force with 9 oz. the space described was 34 inches in  $2^s$ .

From these experiments, and many others which it is not necessary here to relate, it appears, that the space described is always increased by increasing the weight of the body and the accelerative force in the same ratio; and as the acceleration arising from the moving force continued the same, it is manifest, that the retardation arising from the friction must have been diminished, for the whole accelerative force must have been increased on account of the increase of the space described in the same time; and hence (as the retardation from friction varies as  $\frac{\text{Quantity of friction}}{\text{Quantity of matter}}$ ) the quantity of friction increases in a less ratio than the quantity of matter or weight of the body.

6. We come now to the last thing proposed to determine, that is, whether the friction varies by varying the surface on which the body moves. Let us call two of the surfaces A and  $a$ , the former being the greater, and the latter the less. Now the weight on every given part of  $a$  is as much greater than the weight on an equal part of A, as A is greater than  $a$ ; if therefore the friction was in proportion to the weight, *cæteris paribus*, it is manifest, that the friction on  $a$  would be equal to the friction on A, the whole friction being, on such a supposition, as the weight on any given part of each surface multiplied into the number of such parts or into the whole area, which products, from the proportion above, are equal. But from the last experiments it has been proved, that the friction on any given surface increases in a less ratio than the weight; consequently the friction on any given part of  $a$  has a less ratio to the friction on an equal part of A than A has to  $a$ , and hence the friction on  $a$  is less than the friction on A, that is, the smallest surface has always the least friction. But as this conclusion is contrary to the generally received opinion, I have thought it proper to confirm the same by a set of experiments. But before



proceeding to relate them, I beg leave to recommend to those who may afterwards be induced to repeat them, the following cautions, which are extremely necessary to be attended to. Great care must be taken that the two surfaces have exactly the same degree of roughness; in order to be certain of which, such bodies must be chosen as have no knots in them, and whose grain is so very regular that when the two surfaces are planed with a fine rough plane, their roughness may be the same, which will not be the case if the body be knotty, or the grain irregular, or if it happens not to run in the same direction on both surfaces. When you cannot depend on the surfaces having the same degree of roughness, the best way will be to paste some fine rough paper on each surface, which perhaps will give a more equal degree of roughness than can be obtained by any other method. Now as the proof which I have already given depends only on the motion of the body on the same surface, it is not liable to any inaccuracy of the kind which the preceding cautions have been given to avoid, nor indeed to any other, and therefore it must be perfectly conclusive. In the following experiments the cautions mentioned above were carefully attended to.

*Exper. 1.* A body was taken whose flat surface was to its edge as  $22 : 9$ , and with the same moving force the body described on its flat side  $33\frac{1}{2}$  inches in  $2^s$ , and on its edge 47 inches in the same time.

*Exper. 2.* A 2d body was taken whose flat surface was to its edge as  $32 : 3$ , and with the same moving force it described on its flat side 32 inches in  $2^s$ , and on its edge it described  $37\frac{1}{2}$  inches in the same time.

*Exper. 3.* I took another body and covered one of its surfaces, whose length was 9 inches, with a fine rough paper, and by applying a moving force, it described 25 inches in  $2^s$ ; I then took off some paper from the middle, leaving only  $\frac{2}{3}$  of an inch at the two ends, and with the same moving force it described 40 inches in the same time.

*Exper. 4.* Another body was taken which had one of its surfaces, whose length was 9 inches, covered with a fine rough paper, and by applying a moving force it described 42 inches in  $2^s$ ; some of the paper was then taken off from the middle, leaving only  $1\frac{2}{3}$  inches at the two ends, and with the same moving force it described 54 inches in  $2^s$ ; I then took off more paper, leaving only  $\frac{1}{4}$  of an inch at the two ends, and the body then described, by the same moving force, 60 inches in the same time. In the last 2 experiments the paper which was taken off the surface was laid on the body, that its weight might not be altered.

*Exper. 5.* A body was taken whose flat surface was to its edge as  $30 : 17$ ; the flat side was laid on the horizontal plane, a moving force was applied, and the stage was fixed in order to stop the moving force, in consequence of which the body would then go on with the velocity acquired till the friction had destroyed all its motion; when it appeared from a mean of 12 trials that the body moved,



after its acceleration ceased,  $5\frac{2}{3}$  inches before it stopped. The edge was then applied, and the moving force descended through the same space, and it was found, from a mean of the same number of trials, that the space described was  $7\frac{1}{3}$  inches before the body lost all its motion, after it ceased to be accelerated.

*Exper. 6.* Another body was then taken whose flat surface was to its edge as 60 : 19, and by proceeding as before, on the flat surface it described, at a medium of 12 trials,  $5\frac{1}{8}$  inches, and on the edge  $6\frac{1}{4}$  inches, before it stopped, after the acceleration ceased.

*Exper. 7.* Another body was taken whose flat surface was to its edge as 26 : 3, and the spaces described on these two surfaces, after the acceleration ended, were, at a mean of 10 trials,  $4\frac{3}{7}$  and  $7\frac{7}{10}$  inches respectively.

From all these different experiments it appears, that the smallest surface had always the least friction, which agrees with the consequence deduced from the consideration that the friction does not increase in so great a ratio as the weight ; we may therefore conclude, that the friction of a body does not continue the same when it has different surfaces applied to the plane on which it moves, but that the smallest surface will have the least friction.

7. Having thus established, from the most decisive experiments, all that was proposed relative to friction, it may be proper, before concluding, to give the result of my examination into the nature of the experiments which have been made by others ; which were repeated, in order to see how far they were conclusive in respect to the principles which have been deduced from them. The experiments which have been made by all the authors that I have seen, have been thus instituted. To find what moving force would just put a body at rest in motion : from which they concluded, that the accelerative force was then equal to the friction ; but it is manifest, that any force which will put a body in motion must be greater than the force which opposes its motion, otherwise it could not overcome it ; and hence, if there were no other objection than this, it is evident that the friction could not be very accurately obtained ; but there is another objection which totally destroys the experiment, so far as it tends to show the quantity of friction, which is the strong cohesion of the body to the plane when it lies at rest ; and this is confirmed by the following experiments. 1st, A body of  $12\frac{3}{4}$  oz. was laid on a horizontal plane, and then loaded with a weight of 8 lb. and such a moving force was applied as would, when the body was just put in motion, continue that motion without any acceleration, in which case the friction must be just equal to the accelerative force. The body was then stopped, when it appeared, that the same moving force which had kept the body in motion before, would not put it in motion, and it was found necessary to take off  $4\frac{1}{2}$  oz. from the body before the same moving force would put it in motion ; it appears therefore that this body, when laid on the plane at rest, acquired a



very strong cohesion to it. 2dly, A body whose weight was 16 oz. was laid at rest on the horizontal plane, and it was found that a moving force of 6 oz. would just put it in motion; but that a moving force of 4 oz. would, when it was just put in motion, continue that motion without any acceleration, and therefore the accelerative force must then have been equal to the friction, and not when the moving force of 6 oz. was applied.

From these experiments therefore it appears, how very considerable the cohesion was in proportion to the friction when the body was in motion; it being, in the latter case, almost  $\frac{1}{3}$ , and in the former it was found to be very nearly equal to the whole friction. All the conclusions therefore deduced from the experiments, which have been instituted to determine the friction from the force necessary to put a body in motion, have manifestly been totally false; as such experiments only show the resistance which arises from the cohesion and friction conjointly.

8. I shall conclude this part of the subject with a remark on art. 5. It appears from all the experiments which I have made, that the proportion of the increase of the friction to the increase of the weight was different in all the different bodies which were used; no general rule therefore can be established to determine this for all bodies, and the experiments which I have hitherto made have not been sufficient to determine it for the same body. At some future opportunity, when I have more leisure, I intend to repeat the experiments in order to establish, in some particular cases, the law by which the quantity of friction increases by increasing the weight. Leaving this subject therefore for the present, I shall proceed to establish a theory on the principles which we have already deduced from our experiments.

PROP. 1.—*Let efg, fig. 1, pl. 9, represent either a cylinder, or that circular section of a body on which it rolls down the inclined plane CA in consequence of its friction; to find the time of descent and the number of revolutions.*

As it has been proved in art. 5, that the friction of a body does not increase in proportion to its weight or pressure, we cannot therefore, by knowing the friction on any other plane, determine the friction on CA; the friction therefore on CA can only be determined by experiments made on that plane, that is, by letting the body descend from rest, and observing the space described in the first second of time; call that space  $a$ , and then, as by art. 3, friction is a uniformly retarding force, the body must be uniformly accelerated, and consequently the whole time of descent in seconds will be  $= \sqrt{\frac{AC}{a}}$ . Now to determine the number of revolutions, let  $s$  be the centre of oscillation to the point of suspension  $a$ ;

\*  $a$  and  $s$  are not fixed points in the body, but the former always represents that point of the body in contact with the plane, and the latter the corresponding centre of oscillation.—Orig.



then, because no force acting at  $a$  can affect the motion of the point  $s$ , that point, notwithstanding the action of the friction at  $a$ , will always have a motion parallel to  $CA$  uniformly accelerated by a force equal to that with which the body would be accelerated if it had no friction; hence, if  $2m = 32\frac{1}{8}$  feet, the velocity acquired by the point  $s$  in the first second will be  $= \frac{2m \times CB}{CA}$ ; now the excess of the velocity of the point  $s$  above that of  $r$ , the centre, is manifestly the velocity with which  $s$  is carried about  $r$ ; hence the velocity of  $s$  about the centre  $= \frac{2m \times CB}{CA} - 2a = \frac{2m \times CB - 2a \times CA}{CA}$ , consequently  $rs : ra :: \frac{2m \times CB - 2a \times CA}{CA} : \frac{2m \times ra \times CB - 2a \times ra \times CA}{rs \times CA}$  = the velocity with which a point of the circumference is carried about the centre, and which therefore expresses the force which accelerates the rotation; now as  $2a$  expresses the accelerative force of the body down the plane, and the spaces described in the same time are in proportion to those forces, we have  $2a : CA :: \frac{2m \times ra \times CB - 2a \times ra \times CA}{rs \times CA} : \frac{m \times ra \times CB - a \times ra \times CA}{a \times rs}$  the space which any point of the circumference describes about the centre in the whole time of the body's descent down  $CA$ ; which being divided by the circumference  $p \times ra$  (where  $p = 6.283$  &c.) will give  $\frac{m \times BC - a \times AC}{p \times a \times rs}$  for the whole number of revolutions required.

*Cor. 1.* If  $a \times CA = m \times BC$ , the number of revolutions = 0, and therefore the body will then only slide; consequently the friction vanishes.

*Cor. 2.* Let  $a'r's'$  (fig. 2) be the next position of  $ars$ , and draw  $tr'b$  parallel to  $sa$ ; then will  $s't$  represent the retardation of the centre  $r$  arising from friction, and  $a'b$  will represent the acceleration of a point of the circumference about its centre; hence the retardation of the centre: acceleration of the circumference about the centre  $:: s't : a'b ::$  (by sim.  $\Delta$ s)  $tr' : br' :: rs : ra$ .

*Cor. 3.* If  $a'$  coincides with  $a$ , the body does not slide but only roll; now in this case  $ss' : rr' :: as : ar$ ; but as  $ss'$  and  $rr'$  represent the ratio of the velocities of the points  $s$  and  $r$ , they will be to each other as  $\frac{2m \times BC}{CA} : 2a$  or as  $m \times CB : a \times CA$ ; hence, when the body rolls without sliding,  $as : ar :: m \times CB : a \times CA$ .

*Cor. 4.* The time of descent down  $CA$  is  $= \sqrt{\frac{AC}{a}}$ ; but by the last cor. when the body rolls without sliding,  $a = \frac{m \times ra \times BC}{sa \times AC}$ , hence the time of descent in that case  $= AC \sqrt{\frac{sa}{m \times ra \times BC}}$ ; now the time of descent, if there were no friction, would be  $= \frac{AC}{\sqrt{m \times BC}}$ , hence the time of descent, when the body rolls without sliding: time of free descent  $:: \sqrt{sa} : \sqrt{ra}$ .

*Cor. 5.* By the last cor. it appears, that when the body just rolls without sliding, or when the friction is just equal to the accelerative force, the time of de-



scent  $= AC \sqrt{\frac{sa}{m \times ra \times BC}}$ ; now it is manifest, that the time of descent will continue the same, if the friction be increased, for the body will still freely roll, as no increase of the friction acting at  $a$  can affect the motion of the point  $s$ .

If the body be projected from  $c$  with a velocity, and at the same time have a rotatory motion, the time of descent and the number of revolutions may be determined from the common principles of uniformly accelerated motions, as we have already investigated the accelerative force of the body down the plane and of its rotation about its axis; it seems therefore unnecessary to lengthen out this paper with the investigations.

PROP. 2.—*Let the body be projected on an horizontal plane LM (fig. 3) with a given velocity; to determine the space through which the body will move before it stops, or before its motion becomes uniform.*

Case 1.—1. Suppose the body to have no rotatory motion when it begins to move; and let  $a$  = the velocity of projection per second measured in feet, and let the retarding force of the friction of the body, measured by the velocity of the body which it can destroy in one second of time, be determined by experiment and called  $F$ , and let  $x$  be the space through which the body would move by the time its motion was all destroyed when projected with the velocity  $a$ , and retarded by a force  $F$ ; then, from the principles of uniformly retarded motion,  $x = \frac{a^2}{2F}$ ; and if  $t$  = time of describing that space, we have  $t = \frac{a}{F}$ ; and hence

the space described in the first second of time  $= \frac{2a - F}{2}$ . Now it is manifest, that when the rotatory motion of the body about its axis is equal to its progressive motion, the point  $a$  will be carried backwards by the former motion as much as it is carried forwards by the latter; consequently the point of contact of the body with the plane will then have no motion in the direction of the plane, and hence the friction will at that instant cease, and the body will continue to roll on uniformly without sliding with the velocity which it has at that point. Put therefore  $z$  = the space described from the commencement of the motion till it becomes uniform, then the body being uniformly retarded, the spaces from the end of the motion vary as the squares of the velocities, hence

$\frac{a^2}{2F} : a^2 (:: 1 : 2F) :: \frac{a^2}{2F} - z : a^2 - 2Fz =$  square of the progressive velocity when the motion becomes uniform; therefore the velocity destroyed by friction  $= a - \sqrt{a^2 - 2Fz}$ : hence, as the velocity generated or destroyed in the same time is in proportion to the force, we have by cor. 2, prop. 1,  $rs : ra :: a - \sqrt{a^2 - 2Fz} : \frac{ra}{rs} \times (a - \sqrt{a^2 - 2Fz})$  the velocity of the circumference  $efg$  generated about the

centre, consequently  $\sqrt{a^2 - 2Fz} = \frac{ra}{rs} \times (a - \sqrt{a^2 - 2Fz})$ , and hence  $z =$



$\frac{(rs^2 + 2rs \times ra) \times a^2}{as^2 \times 2F}$  the space which the body describes before the motion becomes uniform.

2. If we substitute this value of  $z$  into the expression for the velocity, we shall have  $a \times \frac{ra}{rs}$  for the velocity of the body when its motion becomes uniform; hence therefore it appears that the velocity of the body, when the friction ceases, will be the same, whatever be the quantity of the friction. If the body be the circumference of a circle, it will always lose half the velocity before its motion becomes uniform.

*Case 2.—1.* Let the body, besides having a progressive velocity in the direction LM (fig. 3) have also a rotatory motion about its centre in the direction  $gfe$  and let  $v$  represent the initial velocity of any point of the circumference about the centre, and suppose it first to be less than  $a$ ; then friction being a uniformly retarding force, no alteration of the velocity of the point of contact of the body on the plane can affect the quantity of friction; hence the progressive velocity of the body will be the same as before, and consequently the rotatory velocity generated by friction will also be the same, to which if we add the velocity about the centre at the beginning of the motion, we shall have the whole rotatory motion; hence therefore,  $v + \frac{ra}{rs} \times (a^2 - \sqrt{a^2 - 2Fz}) = \sqrt{a^2 - 2Fz}$ , consequently  $z = \frac{a^2 \times as^2 - (v \times rs + a \times ra)^2}{2F \times as^2}$  the space described before the motion becomes uniform.

2. If this value of  $z$  be substituted into the expression for the velocity, we shall have  $\frac{v \times rs + a \times ra}{as}$  for the velocity when the friction ceases.

3. If  $v = a$ , then  $z = 0$ , and the body will continue to move uniformly with the first velocity.

4. If  $v$  be greater than  $a$ , then the rotatory motion of the point  $a$  on the plane being greater than its progressive motion, and in a contrary direction, the absolute motion of the point  $a$  on the plane will be in the direction ML, and consequently friction will now act in the direction LM in which the body moves, and therefore will accelerate the progressive and retard the rotatory motion; hence it appears, that the progressive motion of a body may be accelerated by friction. Now to determine the space described before the motion becomes uniform, we may observe, that as the progressive motion of the body is now accelerated, the velocity after it has described any space  $z$  will be  $= \sqrt{a^2 + 2Fz}$ , hence the velocity acquired  $= \sqrt{a^2 + 2Fz} - a$ , and consequently the rotatory velocity destroyed  $\frac{ra}{rs} \times (\sqrt{a^2 + 2Fz} - a)$ , hence  $v - \frac{ra}{rs} \times (\sqrt{a^2 + 2Fz} - a) = \sqrt{a^2 + 2Fz}$ , therefore  $z = \frac{(rs \times v + ra \times a)^2 - a^2 \times as^2}{2 \times as^2}$  the space required.



5. If  $a = 0$ , or the body be placed on the plane without any progressive velocity, then  $z = \frac{rs^2 \times v^2}{2F \times as^2}$ .

CASE III.—1. Let the given rotatory motion be in the direction  $gef$ ; then as the friction must in this case always act in the direction  $ML$ , it must continually tend to destroy both the progressive and rotatory motion. Now as the velocity destroyed in the same time is in proportion to the retarding force, and the force which retards the rotatory is to the force which retards the progressive velocity; by Cor. 2, Prop. 1, as  $ra : rs$ , therefore if  $v$  be to  $a$  as  $ra$  is to  $rs$ , then, the retarding forces being in proportion to the velocities, both motions will be destroyed together, and consequently the body, after describing a certain space, will rest; which space, being that described by the body uniformly retarded by the force  $F$ , will, from what was proved in case 1, be equal to  $\frac{a^2}{2F}$ .

2. If  $v$  bears a greater proportion to  $a$  than  $ra$  does to  $rs$ , it is manifest that the rotatory motion will not be all destroyed when the progressive is; consequently the body, after it has described the space  $\frac{a^2}{2F}$ , will return back in the direction  $ML$ ; for the progressive motion being then destroyed, and the rotatory motion still continuing in the direction  $gef$ , will cause the body to return with an accelerative velocity, till the friction ceases by the body's beginning to roll, after which it will move on uniformly. Now to determine the space described before this happens, we have  $rs : ra :: a : \frac{ra \times a}{rs}$  the rotatory velocity destroyed when the progressive is all lost; hence  $v - \frac{ra \times a}{rs} = \frac{v \times rs - a \times ra}{rs}$  = the rotatory velocity at that time, which being substituted for  $v$  in the last article of case 2, gives  $\frac{(v + rs - a \times ra)^2}{2F \times as^2}$  for the space described before the motion becomes uniform.

3. If  $v$  has a less proportion to  $a$  than  $ra$  has to  $rs$ , it is manifest that the rotatory motion will be destroyed before the progressive; in which case a rotatory motion will be generated in a contrary direction till the two motions become equal, when the friction will instantly cease, and the body will then move on uniformly. Now  $ra : rs :: v : \frac{v \times rs}{ra}$  the progressive velocity destroyed when the rotatory velocity ceases, hence  $a - \frac{v \times rs}{ra} = \frac{a \times ra - v \times rs}{ra}$  = progressive velocity when it begins its rotatory motion in a contrary direction; substitute therefore this quantity for  $a$  in the expression for  $z$  in case 1, and we have  $\frac{(rs^2 + 2rs \times ra) \times (a \times ra - v \times rs)^2}{as^2 \times ar^2 \times 2F}$  for the space described after the rotatory motion ceases before the motion of the body becomes uniform. Now to determine the space described before the rotatory motion was all destroyed, we have (as the space from the end of a uniformly retarded motion varies as the square of the



velocity)  $a^2 : \frac{a^2}{2F} :: \frac{(a \times ra - v \times rs)^2}{ra^2} : \frac{(a \times ra - v \times rs)^2}{2F \times ra^2}$  the space that could have been described from the time that the rotatory velocity was destroyed, till the progressive motion would have been destroyed had the friction continued to act; hence  $\frac{a^2}{2F} - \frac{(a \times ra - v \times rs)^2}{2F \times ra^2} = \frac{2av \times ra \times rs - v^2 \times rs^2}{2F \times ra^2} =$  the space described when the rotatory motion was all destroyed, hence  $\frac{(rs^2 + 2rs \times ra) \times (a \times ar - v \times sr)^2}{as^2 \times ar^2 \times 2F} + \frac{2av \times ra \times rs - v^2 \times rs^2}{2F \times ra^2} =$  whole space described by the body before its motion becomes uniform.

*Definition.*—The centre of friction is that point in the base of a body on which it revolves, into which if the whole surface of the base, and the mass of the body were collected, and made to revolve about the centre of the base of the given body, the angular velocity destroyed by its friction would be equal to the angular velocity destroyed in the given body by its friction in the same time.

*PROP. III.*—*To find the centre of friction.*—Let FGH (fig. 4) be the base of a body revolving about its centre c, and suppose about a, b, c, &c. to be indefinitely small parts of the base, and let A, B, C, &c. be the corresponding parts of the solid, or the prismatic parts having a, b, c, &c. for their bases; and p the centre of friction. Now it is manifest, that the decrement of the angular velocity must vary as the whole diminution of the momentum of rotation caused by the friction directly, and as the whole momentum of rotation or effect of the inertia of all the particles of the solid inversely; the former being employed in diminishing the angular velocity, and the latter in opposing that diminution by the endeavour of the particles to persevere in their motion. Hence, if the effect of the friction varies as the effect of the inertia, the decrements of the angular velocity in a given time will be equal. Now as the quantity of friction (as has been proved from experiments) does not depend on the velocity, the effect of the friction of the elementary parts of the base a, b, c, &c. will be as  $a \times ac$ ,  $b \times bc$ ,  $c \times cc$ , &c. also the effect of the inertia of the corresponding parts of the body will be as  $A \times ac^2$ ,  $B \times bc^2$ ,  $C \times cc^2$ , &c. Now when the whole surface of the base and mass of the body are concentrated in p, the effect of the friction will be as  $(a + b + c + \&c.) \times cp$ , and of the inertia as  $(A + B + C + \&c.) \times cp^2$ ; consequently  $a \times ac + b \times bc + c \times cc + \&c. : (a + b + \&c.) \times cp :: A \times ac^2 + B \times bc^2 + C \times cc^2 + \&c. : (A + B + C + \&c.) \times cp^2$ ; and hence  $cp = \frac{(A \times ac^2 + B \times bc^2 + C \times cc^2 + \&c.) \times (a + b + c + \&c.)}{(a \times ac + b \times bc + c \times cc + \&c.) \times (A + B + C + \&c.)} =$  (if s = the sum of the products of each particle into the square of its distance from the axis of motion, T = the sum of the products of each part of the base into its distance from the centre, s = the area of the base, t = the solid content of the body)  $\frac{s \times s}{T \times t}$ .

*PROP. IV.*—*Given the velocity with which a body begins to revolve about the*



centre of its base, to determine the number of revolutions which the body will make before all its motion be destroyed.

Let the friction, expressed by the velocity which it is able to destroy in the body if it were projected in a right line horizontally in one second, be determined by experiment, and called  $F$ ; and suppose the initial velocity of the centre of friction  $P$  about  $C$  to be  $a$ . Then conceiving the whole surface of the base and mass of the body to be collected into the point  $P$ , and (as has been proved in prop. 2)  $\frac{a^2}{2F}$  will be the space which the body so concentrated will describe before all its motion be destroyed; hence if we put  $z = PC$ ,  $p =$  the circumference of a circle whose radius is unity, then will  $pz =$  circumference described by the point  $P$ ; consequently  $\frac{a^2}{2pzF} =$  the number of revolutions required.

*Cor.* If the solid be a cylinder, and  $r$  be the radius of its base, then  $z = \frac{3}{4}r$ , and therefore the number of revolutions  $= \frac{2a^2}{3prF}$ .

PROP. V.—To find the nature of the curve described by any point of a body affected by friction, when it descends down any inclined plane.

Let  $efg$  (fig. 5) be the body, the points  $a, r, s$ , as in prop. 1, and conceive  $st, rn$ , to be two indefinitely small spaces described by the points  $s$  and  $r$  in the same time, and which therefore will represent the velocities of those points; but from prop. 1, the ratio of these velocities is expressed by  $m \times CB : a \times CA$ , hence  $st : rn :: m \times CB : a \times CA$ . With the centre  $r$  let a circle  $vw$  be described touching the plane  $LM$  which is parallel to  $AC$  at the point  $b$ , and let the radius of this circle be such that, conceiving it to descend on the plane  $LM$  along with the body descending on  $CA$ , the point  $b$  may be at rest, or the circle may roll without sliding. To determine which radius, produce  $rs$  to  $x$ , parallel to which draw  $ndy$ , and produce  $nt$  to  $z$ ; now it is manifest, that in order to answer the conditions above-mentioned, the velocity of the point  $x$  must be to the velocity of the point  $r$  as  $2 : 1$ , that is,  $zx : yx :: 2 : 1$ , hence  $zy = yx = nr$ . Now  $zy : dt (:: ny : nd) :: rx : rs$ ; therefore  $dt = \frac{rs}{rx} \times zy = \frac{rs}{rx} \times nr$ , hence  $ts (= td + ds = td + nr = \frac{rs}{rx} \times nr + nr) = \frac{rs + rx}{rx} \times nr$ , consequently  $\frac{rs + rx}{rx} : 1 :: ts : nr ::$  (from what is proved above)  $m \times CB : a \times CA$ ; therefore  $a \times CA \times rs + a \times CA \times rx = m \times CB \times xr$ , hence  $rx = \frac{a \times CA \times sr}{m \times CB - a \times CA}$  the radius of the circle which, rolling down the inclined plane  $LM$ , and carrying the body with it, will give the true ratio of its progressive to its rotatory motion, and consequently that point of the circle which coincides with any given point of the body will, as the circle revolves on the line  $LM$ , describe the same curve as the corresponding point of the body; but as the nature of the curve described by any point of a circle revolving on a



straight line is already very well known, it seems unnecessary to give the investigation.

By a method of reasoning, not very different, may the nature of the curve, which is described by any point of a body moving on an horizontal plane, and affected by friction, be determined.

*XI. Observations and Experiments on the Light of Bodies in a State of Combustion. By the Rev. Geo. Cadogan Morgan, of Norwich. p. 190.*

This discussion, says Mr. M. is nothing more than a series of facts, and of conclusions which seem to flow from those facts, and from an attention to the following data. 1. That light is a body, and like all other bodies subject to the laws of attraction. 2. That light is an heterogeneous body, and that the same attractive power operates with different degrees of force on its different parts. 3. That the light which escapes from combustibles when decomposed by heat, or by any other means, was, previous to its escape, a component part of those substances.

It is an obvious conclusion from these data, that when the attractive force, by which the several rays of light are attached to a body, is weakened, some of those rays will escape sooner than others. Those which are united with the least degree of power will escape first, and those which adhere to it most strongly will be the last to quit their basis. We may here have recourse to a familiar fact, which is analogous to this, and will illustrate it. If a mixture, consisting of equal parts of water, of spirits of wine, and of other more fixed bodies, be placed over a fire; the first influence of that heat, to which all the ingredients are alike exposed, will carry off the spirits of wine only: the next will carry off the spirits of wine blended with particles of water: a still greater degree of heat will blend with the vapour which escapes a part of the more fixed bodies, till at length what evaporates will be a mixture of all the ingredients which were at first exposed to the fire. In like manner, when the surface of a combustible is in a state of decomposition, those parts which are the least fixed, or which are united to it with the least force, will be separated first. Among these, the indigo rays of light will make the earliest appearance: by increasing the heat we shall mix the violet with the indigo. By increasing it still more we shall add the blue and the green to the mixture, till at length we reach that intensity of heat which will cause all the rays to escape at the same instant, and make the flame of a combustible perfectly white. It is not my present design to show why the most refrangible rays are the first which escape from a burning body, but to enumerate the several facts which seem to show, that such a general law takes place in combustion; and that the various colours of bodies in this state are uniformly regulated by that decrease of attractive force now described.



By examining the flame of a common candle we may observe; that its lowest extremities, or the part in which the black colour of the wick terminates, discharges the least heat; and that, as the vertex of the flame is approached, a successive order of parts is passed through, in which the lowest is continually adding to the heat of what is just above it, till we come to the top of the flame, near which all the heat is collected into a focus. At the lowest extremity however, where the heat is inconsiderable, a blue colour may be always observed; and from this appearance, among others, it may be safely concluded, that the blue rays are some of those which escape from combustibles in an early period of their decomposition; and that if the decomposition could be examined in a period still more early, the colour of their flame would be violet. By an a priori deduction of this kind, I was led to watch the appearances of a candle more attentively; whence I found that to the external boundary of a common candle is annexed a filament of light, which, if proper care be taken to prevent the escape of too much smoke, will appear most beautifully coloured with the violet and indigo rays. To the preceding instance of a common candle many facts may be added, which speak a similar language. If sulphur or æther is burned, or any of those combustibles whose vapour is kindled in a small degree of heat, a blue flame will appear, which, if examined by the prism, will be found to consist of the violet, the indigo, the blue, and sometimes a small quantity of the green rays. The best mode however, of showing the escape of some rays by that degree of heat which will not separate others till increased, is the following. Give a piece of brown paper a spherical form, by pressing it on any hard globular substance: gradually bring the paper, thus formed, to that distance from the candle at which it will begin to take fire: in this case a beautiful blue flame may be seen, hanging as it were by the paper till a hole is made in it, when the flame, owing to the increased action of the air on all parts of it, becomes white, though the edges still continue of a blue or violet colour. As a confirmation of what is here concluded from the preceding facts, it may be observed, that the very flame which, when exposed to a certain degree of heat, emitted the most refrangible rays only, will, if exposed to a greater degree of heat, emit such as are less refrangible. The flames of sulphur, spirits of wine, &c. when suddenly exposed to the heat of a reverberatory, change their blue appearance for that which is perfectly white. But to gain a more striking diversity of this fact, I adopted Mr. Melvill's mode of examining bodies while on fire. I darkened my room, and placed between my eye and the combustible a sheet of pasteboard, in the centre of which I made a small perforation. As the light of the burning body escaped through this perforation, I examined it with a prism, and observed the following appearances. When the spirits of wine were set on fire, all the rays appeared in the perforation; but the violet, the blue, and



the green, in the greatest abundance. When the combustion of the spirits was checked by throwing some sal ammoniac into the mixture, the red rays disappeared; but when, by the long continuance of the flame, the sal ammoniac was rendered so hot as to increase, rather than diminish the combustion, the red rays again appeared at the perforation. If the screen was managed so that the different parts of the flame might be examined separately, I always observed that the colours varied according to the degree of heat. At the base of the flame, or where the heat was least, the indigo, the violet, and a very small tinge of the blue and green appeared. As I approached the vertex of the flame, the rays which escaped became more and more numerous, till I reached the top, when all the rays appeared in the prism. When the red rays first made their appearance, their quantity was small, and gradually increased as the eye in its examination approached that part where the heat was greatest. Mr. Melvill, when he made some of the preceding experiments, observed, that the yellow rays frequently escaped in the greatest abundance; but this singularity proceeded from some circumstances which escaped his attention. In consequence of mixing acids or salts with the burning spirits, a very dense fume of unignited particles arises, and before the rays of the burning body arrive at the perforation where the prism catches them, they must pass through a medium which will absorb a great part of the indigo and the violet. On the other hand, owing to the imperfection of the decomposition, very few of the red rays are separated from their basis, and consequently the yellow and the orange rays are those alone which pass through the unburnt smoke of the flame.

But, besides the increase or decrease of heat, there are other modes of retarding or accelerating the combustion of bodies, by which also may be examined some of the preceding illustrations. 1. A candle burns most rapidly and brilliantly in dephlogisticated air. 2. The blue colour of a sulphureous flame in pure air is changed into a dazzling white. 3. The flame of inflammable air, when mixed with nitrous air, is green. It is white strongly tinged with the indigo and violet when mixed with common air; but when mixed with dephlogisticated air, or surrounded by it, the brilliancy of its flame is most singularly beautiful.

If the preceding facts prove that light, as an heterogeneous body, is gradually decomposed during combustion; if they prove likewise, that the indigo rays escape with the least heat, and the red with the greatest; I think we may rationally account for several singularities in the colours of different flames. If a piece of paper, impregnated with a solution of copper in the nitrous acid, be set on fire, the bottom and sides of the flame are always tinged with green. Now this flame is evidently in that weak state of decomposition, in which the



most refrangible rays escape in the greatest abundance; but of these rays, the green escape most plentifully through the unignited vapour and that portion of the atmosphere which separates the eye from the flame. The peculiarity which I have now endeavoured to account for may be observed in the greatest perfection in brass foundries: the heat in this instance, though very strong, is scarcely adequate to the decomposition of the metallic vapour which escapes from the melted brass. A very singular flame therefore appears to the eye; for while its edges are green, its body is such as to give the objects around a very pallid or ghastly appearance, which is the consequence of its wanting that portion of red rays which is necessary to make a perfect white.

The most singular phenomenon attending a burning body, is perhaps the red appearance it assumes in its last stage of combustion. The preceding facts and observations may help us to explain it. 1. After a body has continued to burn for some time, its external surface is to be regarded as having lost a great portion, if not the whole, of those rays which the first application of heat was able to separate. But these rays were the indigo, the violet, the blue, and perhaps the green. Nothing therefore will remain to be separated, but the yellow, the orange, and the red. Consequently, the combustion of the body, in its last state of decomposition, can assume no other than a reddish appearance. But 2. Let us consider the external surface of the combustible as annexed to an inner surface, which may be partly, but not so perfectly decomposed as itself: for the violence of the heat will be found to lessen in its effects the nearer it approaches to the centre of the substance exposed to it. Hence we are to consider the parts just covered by the external surface as having lost less of their component light than the external surface itself. Or the former may retain the green rays when the latter has lost both indigo, violet, blue, and green. 3. Those parts which are nearer the centre of the body than either of the preceding, must, as they are farther from the greatest violence of the heat, have lost proportionally fewer of their rays. Or while the more external parts may have lost all but the red, these may have lost only the indigo and violet. 4. The most central parts may be unaffected by the heat; and whenever the fire does reach these parts, they will immediately discharge their indigo rays, and be decomposed in the gradual manner already described. A piece of rotten wood, while burning, will exemplify and confirm the preceding illustration. When influenced by the external air only, if examined through a prism, no rays will be found to escape but the orange and the red. By blowing on the burning wood with a pair of bellows, the combustion, being increased, will affect those internal parts of the body which were not acted on before. These parts therefore will begin to lose their light, and a prism will show the green, the blue, the violet, and indigo, all appearing in succession. Appearances similar to the pre-



ceding may be observed in a common kitchen fire. When it is faintest, its colour is most red, the other rays having been emitted, and the combustion at a stand; but by blowing on it in this state, its brightness will be increased, and more and more of the rays which are yielded by the internal parts of the body will come to the eye, till at length, by continuing to blow, the combustion will be made so complete as to yield all the rays, or to make it appear perfectly white.

Many are the varieties discoverable in the flames and in the appearances of fixed burning bodies, to which the preceding observations may be applied; but, to avoid unnecessary amplification, I will take notice only of what appears to me an imperfection in Sir Isaac Newton's definition of flame. He conjectures, that it may be a vapour heated red-hot. I think I should rather say, that flame is an instance of combustion whose colour will be determined by the degree of decomposition which takes place. If it be very imperfect, the most refrangible rays only will appear. If it be very perfect, all the rays will appear, and its flame will be brilliant in proportion to this perfection. There are flames however which consist of burning particles, whose rays have partly escaped before they ascended in the form of vapour. Such would be the flame of a red-hot coal, if exposed to such a heat as would gradually disperse it into vapour. When the fire is very low under the furnace of an iron foundry, at the upper orifice of the chimney a red flame of this kind may be seen, which is different from the flame that appears immediately after fresh coals have been thrown on the fire; for, in consequence of adding such a supply to the burning fuel, a vast column of smoke ascends, and forms a medium so thick as to absorb most of the rays excepting the red.

*Experiments on electric light.*—If we wish to procure any degree of certainty in any hypothesis which we may form concerning electrical light, perhaps the following general deductions may be of some service to us. 1. There is no fluid or solid body in passing through which the electric fluid may not be made luminous. In water, spirits, oil, animal fluids of all kinds, the discharge of a Leyden phial of almost any size will appear very splendid, provided we take care to place them in the circuit, so that the fluid may not pass through too great a quantity of them. My general method is to place the fluid, on which I mean to make the experiment, in a tube  $\frac{3}{4}$  of an inch in diameter, and 4 inches long. I stop up the orifices of the tube with 2 corks; through which I push two pointed wires, so that the points may approach within  $\frac{1}{8}$  of an inch to each other. The fluid in passing through the interval between the wires is always luminous, if a force be used sufficiently strong. The glass tube, if not very thick, always breaks when this experiment succeeds. To make the passage of the fluid luminous in the acids, they must be placed in capillary tubes, and two



wires introduced, as in the preceding experiment, whose points shall be very near each other. It is a well known fact, that the discharge of a small Leyden phial in passing over a strip of gold, silver, or Dutch metal leaf will appear very luminous. By conveying the contents of a jar, measuring 2 gallons, over a strip of gold leaf  $\frac{1}{8}$  of an inch in diameter, and a yard long, I have frequently given the whole a dazzling brightness. I cannot say, that a much greater length might not have been made very splendid, nor can I determine to what length the force of a battery might be made luminous in this manner. We may give this experiment a curious diversity, by laying the gold or silver leaf on a piece of glass, and then placing the glass in water: for the whole gold leaf will appear most brilliantly luminous in the water by exposing it, thus circumstanced, to the explosion of a battery. 2. The difficulty of making any quantity of the electrical fluid luminous in any body increases as the conducting power of that body increases.

*Exper. 1.* To make the contents of a jar luminous in boiling water, a much higher charge is necessary than would be sufficient to make it luminous in cold water, which is universally allowed to be the worst conductor.

*Exper. 2.* I have various reasons for believing the acids to be very good conductors. If therefore into a tube, filled with water, and circumstanced as I have already described, a few drops of either of the mineral acids are poured, it will be almost impossible to make the fluid luminous in its passage through the tube.

*Exper. 3.* If a string,\* whose diameter is  $\frac{1}{8}$  of an inch, and length 6 or 8 inches, be moistened with water, the contents of a jar will pass through it luminously: but no such appearance can be produced by any charge of the same jar, provided the same string be moistened with one of the mineral acids. To the preceding instance we may add the various instances of metals which will conduct the electrical fluid without any appearance of light, in circumstances the same with those in which the same force would have appeared luminous in passing through other bodies whose conducting power is less. But I proceed to observe,

3. That the ease with which the electrical fluid is rendered luminous in any particular body is increased by increasing the rarity of the body. The appearance of a spark, or of the discharge of a Leyden phial, in rarefied air is well known. But we need not rest the truth of the preceding observation on the several varieties of this fact; similar phenomena attend the rarefaction of ether, of spirits of wine, and of water.

*Exper. 4.* Into the orifice of a tube, 48 inches long, and  $\frac{3}{8}$  of an inch in diameter, I cemented an iron ball, so as to bear the weight which pressed on it when I filled the tube with quicksilver, leaving only an interval at the open end,

\* The thickness and diameter of the string should be regulated by the force we employ.—Orig.



which contained a few drops of water. Having inverted the tube, and plunged the open end of it into a basin of mercury, the mercury in the tube stood nearly half an inch lower than it did in a barometer at the same instant, owing to the vapour which was formed by the water. But through this rarefied water the electrical spark passed as luminously as it does through air equally rarefied.

*Exper. 5.* If, instead of water, a few drops of spirits of wine be placed on the surface of the mercury, phenomena similar to those of the preceding experiment will be discovered, with this difference only, that as the vapour in this case is more dense, the electrical spark in its passage through it is not quite so luminous as it is in the vapour of water.

*Exper. 6.* Good ether substituted instead of the spirits of wine will press the mercury down so low as the height of 16 or 17 inches. The electrical fluid in passing through this vapour, unless the force be very great indeed, is scarcely luminous. But if the pressure on the surface of the mercury in the basin be gradually lessened by the aid of an air-pump, the vapour will become more and more rare, and the electric spark in passing through it more and more luminous.

*Exper. 7.* I could not discover that any vapour escaped from the mineral acids when exposed in vacuo. To give them therefore greater rarity or tenuity, I found different methods necessary. With a fine camel hair pencil, dipped in the vitriolic, the nitrous, or the marine acid, I drew on a piece of glass a line about  $\frac{1}{8}$  of an inch broad. In some instances I extended this line to the length of 27 inches, and found that the contents of an electric battery, consisting of 10 pint phials coated, would pass over the whole length of this line with the greatest brilliancy. If by widening the line, or by laying on a drop of the acid, its quantity was increased in any particular part, the charge, in passing through that part, never appeared luminous. Water, spirits of wine, circumstanced similarly to the acids in the preceding experiment, were attended with similar, but not equal effects, because, in consequence of the inferiority of their conducting power, it was necessary to make the line through which the charge passed considerably shorter.

4. The brilliancy or splendour of the electric fluid, in its passage through any body, is always increased by lessening the dimensions of that body. I would explain my meaning by saying, that a spark, or the discharge of a battery which we might suppose equal to a sphere  $\frac{1}{4}$  of an inch in diameter, would appear much more brilliant if the same quantity of fluid be compressed into a sphere  $\frac{1}{8}$  of an inch in diameter. This observation is the obvious consequence of many known facts. If the machine be large enough to afford a spark whose length is 9 or 10 inches, this spark may be seen sometimes forming itself into a brush, in which state it occupies more room, but appears very faintly luminous. At other times



the same spark may be seen divided into a variety of ramifications which shoot into the surrounding air. In this case likewise the fluid is diffused over a large surface, and in proportion to the extent of that surface, so is the faintness of the appearance. A spark, which in the open air cannot exceed  $\frac{1}{4}$  of an inch in diameter, will appear to fill the whole of an exhausted receiver 4 inches wide and 8 inches long. But in the former case it is brilliant, and in the latter it grows fainter and fainter as the size of the receiver increases. To prove the observation, which I think may be justified by the preceding facts, I made the following experiments.

*Exper. 8.* To an insulated ball, 4 inches in diameter, I fixed a silver thread, about 4 yards long. This thread, at the end most remote from the ball, was fixed to another insulated substance. I brought the ball within the striking distance of my conductor, and the spark in passing from the conductor to the ball appeared very brilliant; but the whole length of the silver thread appeared faintly luminous at the same instant. In other words, when the spark was confined within the dimensions of a sphere  $\frac{1}{8}$  of an inch in diameter, it was bright, but when diffused over the surface of air which received it from the thread, its light became so faint as to be seen only in a dark room. If I lessened the surface of air which received the spark by shortening the thread, I never failed to increase the brightness of the appearance.

*Exper. 9.* To prove that the faintness of the electric light in vacuo depends on the enlarged dimensions of the space through which it is diffused, we have nothing more to do than to introduce 2 pointed wires into the vacuum, so that the fluid may pass from the point of the one to the point of the other, when the distance between them is not more than  $\frac{1}{16}$  of an inch. In this case we shall find a brilliancy as great as in the open air.

*Exper. 10.* Into a Torricellian vacuum, 36 inches in length, I conveyed as much air as would have filled 2 inches only of the exhausted tube, if it were inverted in water. This quantity of air afforded resistance enough to condense the fluid as it passed through the tube into a spark 38 inches in length. The brilliancy of the spark in condensed air, in water, and in all substances through which it passes with difficulty, depends on principles similar to those which account for the preceding facts.

5. In the appearances of electricity, as well as in those of burning bodies, there are cases in which all the rays of light do not escape; and that the most refrangible rays are those which escape first or most easily. The electrical brush is always of a purple or bluish hue. If you convey a spark through a Torricellian vacuum, made\* without boiling the mercury in the tube, the brush will

\* If the Torricellian vacuum be made with mercury perfectly purged of air, it becomes a perfect non-conductor. This I believe will be proved decisively by some experiments which I hope will be soon communicated to the R. S. Dr. PRICE.—Orig.



display the indigo rays. The spark however may be divided and weakened even in the open air, so as to yield the most refrangible rays only.

*Exper. 11.* To an insulated metallic ball, 4 inches in diameter, I fixed a wire a foot and a half long. This wire terminated in 4 ramifications, each of which was fixed to a metallic ball half an inch in diameter, and placed at an equal distance from a metallic plate, which communicated by metallic conductors with the ground. A powerful spark, after falling on the large ball at one extremity of the wire, was divided in its passage from the 4 small balls to the metallic plate. On examining this division of the fluid in a dark room, I discovered some little ramifications which yielded the indigo rays only: indeed at the edges of all weak sparks the same purple appearance may be discovered. It may also be observed, that the nearer we approach the centre of the spark, the greater is the brilliancy of its colour.

6. The influence of different media on electrical light is analogous to their influence on solar light, and will help us to account for some very singular appearances.

*Exper. 12.* Let a pointed wire, having a metallic ball fixed to one of its extremities, be forced obliquely into a piece of wood, so as to make a small angle with the surface of the wood, and to make the point lie about  $\frac{1}{8}$  of an inch below the surface. Let another pointed wire, which communicates with the ground, be forced in the same manner into the same wood, so that its point also may lie about  $\frac{1}{8}$  of an inch below the surface, and about 2 inches distant from the point of the first wire. Let the wood be insulated, then a strong spark which strikes on the metallic ball will force its passage through the interval of wood which lies between the points, and appear as red as blood. To prove that this appearance depends on the wood's absorption of all the rays but the red, it may be observed, that the greater the depth of the points is below the surface, the less mixed are the red rays. I have been able sometimes, by increasing or diminishing the depth of the points, to give the spark the following succession of colours. When they were deepest below the surface, the red only came to the eye through a prism. When they were raised a little nearer the surface, the red and orange appeared. When nearer still, the yellow; and so on till, by making the spark pass through the wood very near its surface, all the rays were at length able to reach the eye. If the points be only  $\frac{1}{8}$  of an inch below the surface of soft deal wood, the red, the orange, and the yellow rays will appear as the spark passes through it. But when the points are at an equal depth in a harder piece of wood, such as box, the yellow, and perhaps the orange, will disappear. As a further proof that the phenomena now describing are owing to the interposition of the wood, as a medium which absorbs some of the rays and suffers others to escape, it may be observed, that when the spark



strikes very brilliantly on one side of the piece of deal, on the other side it will appear very red. In like manner a red appearance may be given to a spark which strikes brilliantly over the inside of a tube, merely by spreading some pitch very thinly over the outside of the same tube.

*Exper. 13.* I would now give another fact, whose singularities depend very much on the influence of the medium through which the electrical light is made to pass. If into a Torricellian vacuum, of any length, a few drops of ether are conveyed, and both ends of the vacuum be stopped up with metallic conductors, so that a spark may pass through it, the spark in its passage will assume the following appearances. When the eye is placed close to the tube, the spark will appear perfectly white. If the eye be removed to the distance of 2 yards, it will appear green; but at the distance of 6 or 7 yards, the colour of the spark will be reddish. These changes evidently depend on the quantity of medium through which the light passes; and the red light more particularly, which we see at the greatest distance from the tube, is accounted for on the same principle as the red light of a distant candle or a beclouded sun.

*Exper. 14.* Dr. Priestley long since observed the red appearance of the spark when passing through inflammable air. But this appearance is very much diversified by the quantity of medium, through which you look at the spark. When at a very considerable distance, the red comes to the eye unmixed; but, if the eye be placed close to the tube, the spark appears white and brilliant. In confirmation however of some of my conclusions, I would observe, that by increasing the quantity of fluid which is conveyed through any portion of inflammable air, or by condensing that air, the spark may be entirely deprived of its red appearance, and made perfectly brilliant. I have only to add, that all weak explosions and sparks, when viewed at a distance, bear a reddish hue. Such are the explosions which have passed through water, spirits of wine, or any bad conductor, when confined in a tube, whose diameter is not more than an inch. The reason of these appearances seems to be, that the weaker the spark or explosion is, the less is the light which escapes; and the more visible the effect of any medium which has a power to absorb some of that light.

The preceding observations concerning electrical light were the result of my attempts to arrange, under general heads, the principal singularities attending it. They may perhaps assist others in determining how far they may have led my mind astray in giving birth to a theory which I would now briefly describe in a few queries.

1. If we consider all bodies as compounds, whose constituent parts are kept together by attracting each other with different forces, can we avoid concluding, that the operations of that attractive force are regulated, not only by the quality, but the quantity also of those component parts? If a union of a certain



number of one kind of particles, with a certain number of a 2d and 3d kind of particles, forms a particular body, must not the bond which keeps that body together be weakened or strengthened by increasing or diminishing any one of the different kinds of particles which enter into its constitution?

2. When, to the natural share of the electric fluid already existing in the body, a fresh quantity of the same fluid is added, must not some of the component parts of that body escape; or must not that attractive force which kept all together be so far weakened as to let loose some constituent parts, and among these the particles of light in particular?

3. Must not this separation of parts be great in proportion to the quantity of extraneous particles which are added to the body? Or, agreeable to the 4th observation, must not the spark be more splendid and brilliant, the more the electrical fluid is concentrated in any given space.

4. In the diminution or alteration of that attractive force on which depends the constitution of bodies, may there not be a gradation which, in the present case, as well as in that of burning bodies, will cause the escape of some rays sooner than others?

*Observations on phosphoric light.*—It is obvious, from Mr. B. Wilson's experiments, that there are many curious diversities in the appearances of phosphori. Some shells, prepared agreeably to his directions, after exposure to the sun or to the flash of a battery, emit a purple, others a green, and others a reddish light. If, with Mr. Wilson, we suppose that these shells are in a state of slow combustion, may we not conclude, that some are just beginning to burn, and therefore, agreeably to what I have observed on combustible bodies, emitting the most refrangible rays; while others are in a more advanced state of combustion, and therefore emitting the least refrangible. If this conclusion be right, the shells which are emitting the purple, or the green, must still retain the yellow, the orange, and the red, which will also make their appearance as soon as the combustion is sufficiently increased.

*Exper. 15.* Place a shell, while emitting its green rays, on a warm shovel, and the appearance of the shell will be soon changed into that of a yellow mixed with red. To Mr. Wilson's theory of slow combustion, the following objections may be opposed.

1°. If phosphoric shells owe their light to this cause, we must consider the word combustion, when applied to them, as implying in its signification all those circumstances which are the usual attendants of a body while on fire. Among other necessary consequences in such a case, the increase of heat must increase the decomposition of the combustible; whereas we discover an effect the very opposite to this in the appearance of a phosphoric body, which never fails to lose its light entirely in a certain degree of heat, without losing the power of



becoming phosphoric again when it has been sufficiently cooled. Besides, when a phosphoric shell has been made very hot, and while it has continued so, I have conveyed the most brilliant discharge of a battery over it without effect. In other words, heat, or the very cause which promotes combustion in all other instances, in this particular case puts an end to it. Mr. Wilson, in his *Treatise on Phosphori*, has described an experiment similar to the preceding. But the result he mentions is different from that mentioned here. However, from a regard to his authority, I have so frequently repeated my trials that I cannot justly suspect myself of any inaccuracy. 2°. When bodies are wasted by combustion, they can never be made to re-assume the appearances which they previously displayed. No power can give to ashes the phenomena of a burning coal. But phosphoric bodies are very different in this respect; for a shell may be made to lose all its light by exposure to heat, and again may be made as luminous as ever by exposure to the sun. But 3°. It is observable, that some bodies, which are most beautifully phosphoric, or which, according to Mr. Wilson's theory, are in the best state of slow combustion; it is observable, I say, that the same bodies are the most obstinate in resisting the fire. The diamond, which to be decomposed requires the force of a most powerful furnace, is, according to this theory, wasting away, owing to a separation of parts which is promoted by the weakest influence of the sun's rays.—Without determining whether the preceding objections be valid, let us now see the consequence of admitting the common hypothesis, that the detention of those rays which fall on phosphori, is owing to some force which prevents their immediate reflection, but is not adequate to their entire absorption. This force, whatever it be, cannot well be supposed to operate with equal power on all the rays. And if this be not the case, I think we cannot avoid concluding, that phosphoric shells will assume different colours, owing to the earlier and later escape of the different rays of light. This conclusion is justified by an experiment which I have already appealed to. When the force is such as to admit of the escape of the purple, the blue, and the green, we have only to lessen that force by warming the body, and the yellow, the orange, and red escape. It is proved by Beccaria's extensive experience on this subject, that there is scarcely any body which is not phosphoric, or which may not be made so by heat. But as the phosphoric force is most powerful when the purple rays only escape, so we are to conclude that it is weakest when it is able to retain the red rays only. This conclusion is agreeable to several facts. Chalk, oyster-shells, with those phosphoric bodies whose goodness has been very much impaired by long keeping; when finely powdered and placed within the circuit of an electrical battery, will exhibit by their scattered particles a shower of light; but these particles will appear reddish, or their phosphoric power will be sufficient only to detain the yellow, orange, and red



rays. When spirits of wine are in a similar manner brought within the circuit of a battery, a similar effect may be discovered; its particles diverge in several directions, displaying a most beautiful golden appearance. The metallic calces are, of all bodies, those which are rendered phosphoric with the greatest difficulty. But even these may be scattered into a shower of red luminous particles by the electric stroke.—*Norwich, Oct. 7, 1784.*

*Postscript by the Rev. Dr. Price.*—By the phosphoric force mentioned in the last paragraph of this paper, Mr. Morgan appears to mean, not the force with which a phosphoric body emits, but the force with which it absorbs and retains light. This last force is proportioned to the degree of attraction between the phosphoric body and light; and therefore must, as Mr. Morgan observes, be weakest when it emits so freely the light it has imbibed, as not to retain those rays which adhere to it most strongly. According to Mr. Morgan's theory, these rays are those which are least refrangible. The observations and experiments in this paper seem to render this theory probable. It is however an objection to it, that the less refrangibility of rays seems to imply a less force of attraction between them and the substances which refract them; but it should be considered, that possibly the force of cohesion, which unites the rays of light to bodies, may be a different power from that which refracts them.

*XII. On the Construction of the Heavens. By Wm. Herschel, Esq., F. R. S.*  
p. 213.

That the milky way is a most extensive stratum of stars of various sizes admits no longer of the least doubt; and that our sun is actually one of the heavenly bodies belonging to it is as evident. I have now viewed and gaged this shining zone in almost every direction, and find it composed of stars whose number, by the account of these gages, constantly increases and decreases in proportion to its apparent brightness to the naked eye. But in order to develop the ideas of the universe, that have been suggested by my late observations, it will be best to take the subject from a point of view at a considerable distance both of space and of time.

*Theoretical view.*—Let us then suppose numberless stars of various sizes, scattered over an indefinite portion of space, in such a manner as to be almost equally distributed throughout the whole. The laws of attraction, which no doubt extend to the remotest regions of the fixed stars, will operate in such a manner as most probably to produce the following remarkable effects.

*Formation of nebulae.*—Form 1. In the first place, since we have supposed the stars to be of various sizes, it will frequently happen that a star, being considerably larger than its neighbouring ones, will attract them more than they will be attracted by others that are immediately around them; by which means



they will be, in time, as it were condensed about a centre, or form themselves into a cluster of stars of almost a globular figure, more or less regularly so, according to the size and original distance of the surrounding stars. The perturbations of these mutual attractions must doubtless be very intricate; but in order to apply Newton's reasoning of bodies moving in ellipses to such as are here, for a while, supposed to have no other motion than what their mutual gravity has imparted to them, we must suppose the conjugate axes of these ellipses indefinitely diminished, by which the ellipses will become straight lines.

Form 2. The next case, which will also happen almost as frequently as the former, is where a few stars, though not superior in size to the rest, may chance to be rather nearer each other than the surrounding ones; for here also will be formed a prevailing attraction in the combined centre of gravity of them all, which will occasion the neighbouring stars to draw together; not indeed so as to form a regular or globular figure, but yet in such a manner as to be condensed towards the common centre of gravity of the whole irregular cluster. And this construction admits of the utmost variety of shapes, according to the number and situation of the stars which first gave rise to the condensation of the rest.

Form 3. From the composition and repeated conjunction of both the foregoing forms, a 3d may be derived, when many large stars, or combined small ones, are situated in long extended, regular, or crooked rows, hooks, or branches; for they will also draw the surrounding ones, so as to produce figures of condensed stars coarsely similar to the former which gave rise to these condensations.

Form 4. We may likewise admit of still more extensive combinations; when, at the same time that a cluster of stars is forming in one part of space, there may be another collecting in a different, but perhaps not far distant quarter, which may occasion a mutual approach towards their common centre of gravity.

Form 5. In the last place, as a natural consequence of the former cases, there will be formed great cavities or vacancies, by the retreat of the stars towards the various centres which attract them; so that on the whole there is evidently a field of the greatest variety for the mutual and combined attractions of the heavenly bodies to exert themselves in. I shall therefore now proceed to a few considerations, that will naturally occur to every one who may view this subject in the light I have here done.

*Objections considered.*—At first sight then it will seem as if a system, such as above displayed, would evidently tend to a general destruction, by the shock of one star's falling on another. It would here be a sufficient answer to say, that if observation should prove this really to be the system of the universe, there is no doubt but that the great Author of it has amply provided for the



preservation of the whole, though it should not appear to us in what manner this is effected. I shall however point out several circumstances that manifestly tend to a general preservation: as, in the first place, the indefinite extent of the sidereal heavens, which must produce a balance that will effectually secure all the great parts of the whole from approaching to each other. There remains then only to see how the particular stars belonging to separate clusters will be preserved from rushing on to their centres of attraction. And here I must observe, that though I have before, by way of rendering the case more simple, considered the stars as being originally at rest, I intended not to exclude projectile forces; and the admission of them will prove such a barrier against the seeming destructive power of attraction, as to secure from it all the stars belonging to a cluster, if not for ever, at least for millions of ages. Besides, we ought perhaps to consider such clusters, and the destruction of now and then a star, in some thousands of ages, as perhaps the very means by which the whole is preserved and renewed. These clusters may be the laboratories of the universe, if I may so express myself, wherein the most salutary remedies for the decay of the whole are prepared.

*Optical appearances.*—From this theoretical view of the heavens, which has been taken from a point not less distant in time than in space, we will now retreat to our own retired station, in one of the planets attending a star in its great combination with numberless others; and in order to investigate what will be the appearances from this contracted situation, let us begin with the naked eye. The stars of the first magnitude being in all probability the nearest, will furnish us with a step to begin our scale; setting off therefore with the distance of Sirius or Arcturus, for instance, as unity, we will at present suppose, that those of the 2d magnitude are at double, and those of the 3d at treble the distance, and so forth. It is not necessary critically to examine what quantity of light or magnitude of a star entitles it to be estimated of such or such a proportional distance, as the common coarse estimation will answer our present purpose as well; taking it then for granted, that a star of the 7th magnitude is about 7 times as far as one of the 1st, it follows, that an observer, who is inclosed in a globular cluster of stars, and not far from the centre, will never be able, with the naked eye, to see to the end of it: for since, according to the above estimations, he can only extend his view to about 7 times the distance of Sirius, it cannot be expected that his eyes should reach the borders of a cluster which has perhaps not less than 50 stars in depth every where around him. The whole universe therefore to him will be comprized in a set of constellations, richly ornamented with scattered stars of all sizes. Or if the united brightness of a neighbouring cluster of stars should, in a remarkably clear night, reach his sight, it will put on the appearance of a small, faint, whitish, nebulous cloud,



not to be perceived without the greatest attention. To pass by other situations, let him be placed in a much extended stratum, or branching cluster of millions of stars, such as may fall under the 3d form of *nebulæ* considered in a foregoing paragraph. Here also the heavens will not only be richly scattered over with brilliant constellations, but a shining zone or milky way will be perceived to surround the whole sphere of the heavens, owing to the combined light of those stars which are too small, that is, too remote to be seen. Our observer's sight will be so confined, that he will imagine this single collection of stars, of which he does not even perceive the thousandth part, to be the whole contents of the heavens. Allowing him now the use of a common telescope, he begins to suspect that all the milkiness of the bright path which surrounds the sphere may be owing to stars. He perceives a few clusters of them in various parts of the heavens, and finds also that there are a kind of nebulous patches; but still his views are not extended so far as to reach to the end of the stratum in which he is situated, so that he considers these patches as belonging to that system which to him seems to comprehend every celestial object. He now increases his power of vision, and, applying himself to a close observation, finds that the milky way is indeed no other than a collection of very small stars. He perceives that those objects which had been called *nebulæ* are evidently nothing but clusters of stars. He finds their number increase on him, and when he resolves one nebula into stars he discovers 10 new ones which he cannot resolve. He then forms the idea of immense strata of fixed stars, of clusters of stars, and of *nebulæ*; till, going on with such interesting observations, he now perceives that all these appearances must naturally arise from the confined situation in which we are placed. Confined it may justly be called, though in no less a space than what before appeared to be the whole region of the fixed stars; but which now has assumed the shape of a crookedly branching nebula; not, indeed, one of the least, but, perhaps very far from being the most considerable of these numberless clusters that enter into the construction of the heavens.

*Result of observations.*—I shall now endeavour to show, that the theoretical view of the system of the universe, exposed in the foregoing part of this paper, is perfectly consistent with facts, and seems to be confirmed and established by a series of observations. It will appear, that many hundreds of *nebulæ* of the 1st and 2d forms are actually to be seen in the heavens, and their places will hereafter be pointed out. Many of the 3d form will be described, and instances of the 4th related. A few of the cavities mentioned in the 5th will be particularized, though many more have already been observed; so that, on the whole, I believe, it will be found, that the foregoing theoretical view, with all its consequential appearances, as seen by an eye inclosed in one of the *nebulæ*, is no other than a drawing from nature, wherein the features of the original have been closely



copied; and I hope the resemblance will not be called a bad one, when it shall be considered how very limited must be the pencil of an inhabitant of so small and retired a portion of an indefinite system in attempting the picture of so unbounded an extent.

But to proceed to particulars: Mr. H. begins by giving a table of what he calls gages that have been taken. In the 1st column is the right ascension, and in the 2d the north polar distance, both reduced to the time of Flamsteed's catalogue. In the 3d are the contents of the heavens, being the result of the gages. The 4th shows from how many fields of view the gages were deduced, which have been 10 or more where the number of the stars was not very considerable; but, as it would have taken too much time, in high numbers, to count so many fields, the gages are generally single. Where the stars happened to be uncommonly crowded, no more than half a field was counted, and even sometimes only a quadrant; but then it was always done with the precaution of fixing on some row of stars that would point out the division of the field, so as to prevent any considerable mistake. When 5, 10, or more fields are gaged, the polar distance in the 2d column of the table is that of the middle of the sweep, which was generally from 2 to  $2\frac{1}{2}$  degrees in breadth; and, in gaging, a regular distribution of the fields, from the bottom of the sweep to the top, was always strictly attended to. The 5th column contains occasional remarks, relating to the gages. As it is not necessary to reprint these tables of numbers, we shall pass them over, and proceed with the remaining part of the paper.

*PROBLEM.*—*The stars being supposed to be nearly equally scattered, and their number, in a field of view of a known angular diameter, being given, to determine the length of the visual ray.*

Here, the arrangement of the stars not being fixed on, we must endeavour to find which way they may be placed so as to fill a given space most equally. Suppose a rectangular cone cut into frustula by many equidistant planes perpendicular to the axis; then, if one star be placed at the vertex, and another in the axis at the first intersection, 6 stars may be set around it so as to be equally distant from each other and from the central star. These positions being carried on in the same manner, we shall have every star within the cone surrounded by 8 others, at an equal distance from that star taken as a centre. Fig. 6, pl. 9, contains 4 sections of such a cone distinguished by alternate shades, which will be sufficient to explain what sort of arrangement is here intended.

The series of the number of stars contained in the several sections will be 1, 7, 19, 37, 61, 91, &c. which continued to  $n$  terms, the sum of it, by the differential method, will be  $na + n \cdot \frac{n-1}{2} d' + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} d''$ , &c.: where  $a$  is the first term  $d'$ ,  $d''$ ,  $d'''$ , &c. the 1st, 2d, and 3d differences. Then, since  $a = 1$ ,  $d' = 6$ ,  $d'' = 6$ ,  $d''' = 0$ , the sum of the series will be  $n^3$ . Let  $s$  be the given



number of stars;  $I$ , the diameter of the base of the field of view; and  $B$ , the diameter of the base of the great rectangular cone; and, by trigonometry, we shall have  $B = \frac{\text{Radius.}}{\text{Tang. } \frac{1}{2} \text{ field.}}$  Now, since the field of view of a telescope is a cone, we shall have its solidity to that of the great cone of stars, formed by the above construction, as the square of the diameter of the base of the field of view, to the square of the diameter of the base of the great cone, the height of both being the same; and the stars in each cone being in the ratio of the solidity, as being equally scattered, we have  $n = \sqrt[3]{B^2 s}$ . And the length of the visual ray  $= n - 1$ , which was to be determined.

*The same otherwise.*—If a different arrangement of the stars should be selected, such as that in fig. 7, where 1 star is at the vertex of a cone; 3 in the circumference of the first section, at an equal distance from the vertex and from each other; 6 in the circumference of the next section, with 1 in the axis or centre; and so on, always placing 3 stars in a lower section, in such a manner as to form an equilateral pyramid with 1 above them: then we shall have every star, which is sufficiently within the cone, surrounded by 12 others at an equal distance from the central star and from each other. And by the differential method, the sum of the two series equally continued, into which this cone may be resolved, will be  $2n^3 + 1\frac{1}{2}n^2 + \frac{1}{2}n$ ; where  $n$  stands for the number of terms in each series. To find the angle which a line  $vx$ , passing from the vertex  $v$  over the stars  $v, n, h, l$ , &c. to  $x$ , at the outside of the cone, makes with the axis; we have, by construction,  $vs$  in fig. 8, representing the planes of the 1st and 2d sections  $= 2 \times \cos. 30^\circ = \phi$ , to the radius  $ps$ , of the first section  $= 1$ . Hence it will be  $\sqrt{\phi^2 - 1} = vp = \frac{1}{2}vm$ ; or  $vm = 2\sqrt{\phi^2 - 1}$ : and, by trigonometry,  $\frac{R\phi}{2\sqrt{\phi^2 - 1}} = \tau$ . Where  $\tau$  is the tangent of the required angle to the radius  $R$ ; and putting  $t =$  tangent of half the given field of view, it will be  $\frac{\tau}{t} = B$ , the base of the cone. And  $\frac{\sqrt{\phi^2 - 1}}{\phi} = d$ , will be an expression for  $vp$  in terms of  $vs$ , which is the mutual distance of the scattered stars. Then having  $\frac{B^2 s}{2} = n^3 + \frac{3}{4}n^2 + \frac{1}{4}n$ , we may find  $n$ ; whence  $2dn - d$ , the visual ray will be obtained. The result of this arrangement gives a shorter ray than that of the former; but since the difference is not so considerable as very materially to affect the conclusions, I shall, on account of the greater convenience, make use of the first.

*We inhabit the planet of a star belonging to a compound nebula of the 3d form.*—I shall now proceed to show that the stupendous sidereal system we inhabit, this extensive stratum and its secondary branch, consisting of many millions of stars, is, in all probability, a detached nebula. In order to go upon grounds



that seem to be capable of great certainty, they being no less than an actual survey of the boundaries of our sidereal system, which I have plainly perceived, as far as I have yet gone round it, every where terminated, and in most places very narrowly too, it will be proper to show the length of my sounding line, if I may so call it, that it may appear whether it was sufficiently long for the purpose. In the most crowded part of the milky way I have had fields of view that contained no less than 588 stars, and these were continued for many minutes, so that in one quarter of an hour's time there passed no less than 116000 stars through the field of view of my telescope.\* Now, if we compute the length of the visual ray by putting  $s = 588$ , and the diameter of the field of view  $15'$ , we shall find  $n = \sqrt[3]{B^2 s} = 498$ ; so that it appears the length of what has been called the sounding line, or  $n - 1$ , was probably not less than 497 times the distance of Sirius from the sun. The same gage calculated by the 2d arrangement of stars gives  $\sqrt{\phi^2 - 1} = 1.41421$ ;

$\frac{R\phi}{2\sqrt{\phi^2 - 1}} = \text{tangent of } 31^\circ 28' 55''.77$ ;  $\frac{T}{t} = B = 280.69$ ;  $\frac{\sqrt{\phi^2 - 1}}{\phi} = d = .81649$ ;  $\frac{1}{2}B^2 s = 23163409.7 = n^3 + \frac{3}{4}n^2 + \frac{1}{4}n$ ; where  $n = 284.8$  nearly; and  $2dn - 1 = 464$ , the visual ray.

It may seem inaccurate that we should found an argument on the stars being equally scattered, when in all probability there may not be two of them in the heavens, whose mutual distance shall be equal to that of any other two given stars; but it should be considered, that when we take all the stars collectively, there will be a mean distance which may be assumed as the general one; and an argument founded on such a supposition will have in its favour the greatest probability of not being far short of truth. What will render the supposition of an equal distribution of the stars, with regard to the gages, still less exposed to objections is, that whenever the stars happened either to be uncommonly crowded or deficient in number, so as very suddenly to pass over from one extreme to the other, the gages were reduced to other forms, such as the border gage, the distance gage, &c. which terms, and the use of such gages, there will hereafter be an opportunity of explaining. And none of those kinds of gages have been admitted in this table, which consists only of such as have been taken in places where the stars apparently seemed to be, in general, pretty evenly scattered; and to increase and decrease in number by a certain gradual progression. Nor has any part of the heavens containing a cluster of stars been put in the gages;

\* The breadth of my sweep was  $2^\circ 26'$ , to which must be added  $15'$  for two semi-diameters of the field. Then, putting  $161 = a$ , the number of fields in 15 minutes of time;  $.7854 = b$ , the proportion of a circle to 1, its circumscribed square;  $\phi = \text{sine of } 74^\circ 22'$ , the polar distance of the middle of the sweep reduced to the present time; and  $588 = s$ , the number of stars in a field of view, we have  $\frac{a\phi s}{b} = 116076$  stars. —Orig.



and here I must observe, that the difference between a crowded place and a cluster may easily be perceived by the arrangement as well as the size and mutual distance of the stars: for in a cluster they are generally not only resembling each other pretty nearly in size, but a certain uniformity of distance also takes place; they are more and more accumulated towards the centre, and put on all the appearances which we should naturally expect from a number of them collected into a group at a certain distance from us. On the other hand, the rich parts of the milky way, as well as those in the distant broad part of the stratum, consist of a mixture of stars of all possible sizes, that are seemingly placed without any particular apparent order. Perhaps we might recollect, that a greater condensation towards the centre of our system, than towards the borders of it, should be taken into consideration; but, with a nebula of the 3d form, containing such various and extensive combinations, as I have found to take place in ours, this circumstance, which, in one of the first form, would be of considerable moment, may I think be safely neglected. However, I would not be understood to lay a greater stress on these and the following calculations than the principles on which they are founded will permit; and if hereafter we shall find reason, from experience and observation, to believe that there are parts of our system where the stars are not scattered in the manner here supposed, we might then make proper exceptions.

Mr. H. then tries the effect of another high gage, which, by a process of calculation and reasoning like the preceding, gives a similar conclusion in the number of the stars, &c. He then continues to remark, that these would not be so close but that a good power applied to a proper instrument might easily distinguish them; for they need not, if arranged in regular squares, approach nearer to each other than  $6''.27$ ; but what would produce the milky nebulosity which I have mentioned, is the numberless stars beyond them, which in one respect the visual ray might also be said to reach. To make this appear we must return to the naked eye, which, as before estimated, can only see the stars of the 7th magnitude so as to distinguish them; yet it is very evident that the united lustre of millions of stars, such as I suppose the nebula in Andromeda to be, will reach our sight in the shape of a very small, faint nebulosity; since the nebula of which I speak may easily be seen in a fine evening. In the same manner my present telescope has not only a visual ray that will reach the stars at 497 times the distance of Sirius so as to distinguish them, and probably much farther, but also a power of showing the united lustre of the accumulated stars that compose a milky nebulosity, at a distance far exceeding the former limits: so that from these considerations it appears highly probable that my present telescope, not showing such a nebulosity in the milky way, goes already far beyond its extent: and consequently, much more would a more powerful instrument



remove all doubt on the subject, both by showing the stars in the continuation of the stratum, and by exposing a very strong milky nebulosity beyond them, that could no longer be mistaken for the dark ground of the heavens.

To these arguments, which rest on the firm basis of a series of observation, we may add the following considerations drawn from analogy. Among the great number of nebulae which I have now already seen, amounting to more than 900, there are many which in all probability are equally extensive with that which we inhabit; and yet they are all separated from each other by very considerable intervals. Some indeed there are that seem to be double and treble; and though with most of these it may be that they are at a very great distance from each other, yet we allow that some such conjunctions really are to be found; nor is this what we mean to exclude. But then these compound or double nebulae, which are those of the 3d and 4th forms, still make a detached link in the great chain. It is also to be supposed, that there may still be some thinly scattered solitary stars between the large interstices of nebulae, which, being situated so as to be nearly equally attracted by the several clusters when they were forming, remain unassociated. And though we cannot expect to see these stars, on account of their vast distance, yet we may well presume, that their number cannot be very considerable in comparison with those that are already drawn into systems; which conjecture is also abundantly confirmed in situations where the nebulae are near enough to have their stars visible; for they are all insulated, and generally to be seen on a very clear and pure ground, without any star near them that might be supposed to belong to them. And though I have often seen them in beds of stars, yet from the size of these latter we may be certain, that they were much nearer to us than those nebulae, and doubtless belonged to our own system.

*Use of the gages.*—A delineation of the nebula, by an application of the gages in the manner proposed in a former paper, may now be attempted, and a table is calculated, and here given, for this purpose. It gives the length of the visual ray for any number of stars in the field of view contained in the 3d column of the former table of gages from  $\frac{1}{10}$  to 100000. If the number required be not found in the 1st column of this table, a proportional mean may be taken between the two nearest rays in the 2d column, without any material error, except in the few last numbers. The calculations of resolvable and milky nebulosity, at the end of the table, are founded, the first, on a supposition of the stars being so crowded as to have only a square second of space allowed them; the next assigning them only half a second square. However, we should consider that in all probability a very different accumulation of stars may take place in different nebulae; by which means some of them may assume the milky appearance, though not near so far removed from us; while clusters of stars also may



become resolvable nebulæ from the same cause. The distinctness of the instrument is here also concerned; and as telescopes with large apertures are not easily brought to a good figure, nebulous appearances of both sorts may probably come on much before the distance annexed to them in the table.

*Section of our sidereal system.*—By taking out of this table the visual rays which answer to the gages, and applying lines proportional to them around a point, according to their respective right ascensions and north polar distances, we may delineate a solid by means of the ends of these lines, which will give us so many points in its surface; I shall however content myself at present with a section only. I have taken one which passes through the poles of our system, and is at rectangles to the conjunction of the branches which I have called its length. The name of poles seemed not improperly applied to those points which are 90 degrees distant from a circle passing along the milky way, and the north pole is here assumed to be situated in R. A.  $186^{\circ}$  and P. D.  $58^{\circ}$ . The section represented in fig. 9, is one which makes an angle of 35 degrees with our equator, crossing it in  $124\frac{1}{2}$  and  $304\frac{1}{2}$  degrees. A celestial globe, adjusted to the latitude of  $55^{\circ}$  north, and having  $\sigma$  Ceti near the meridian, will have the plane of this section pointed out by the horizon, and the gages which have been used in this delineation are those which in table 1 are marked by asterisks. When the visual rays answering to them are taken out of the 2d table, they must be projected on the plane of the horizon of the latitude which has been pointed out; and this may be done accurately enough for the present purpose by a globe adjusted as above directed; for as gages, exactly in the plane of the section, were often wanting, I have used many at some small distance above and below the same, for the sake of obtaining more delineating points; and in the figure the stars at the borders which are larger than the rest are those pointed out by the gages. The intermediate parts are filled-up by smaller stars arranged in straight lines between the gaged ones. The delineating points, though pretty numerous, are not so close as might be wished; it is however to be hoped that in some future time this branch of astronomy will become more cultivated, so that we may have gages for every quarter of a degree of the heavens at least, and these often repeated in the most favourable circumstances. And whenever that shall be the case, the delineations may then be repeated with all the accuracy that long experience may enable us to introduce; for, this subject being so new, I consider what is here given partly as only an example to illustrate the spirit of the method. From this figure however, which I hope is not a very inaccurate one, we may see that our nebula, as observed before, is of the 3d form: that is, a very extensive, branching, compound congeries of many millions of stars; which most probably owes its origin to many remarkably large as well as pretty closely scattered small stars, that may



have drawn together the rest. Now, to have some idea of the wonderful extent of this system, it may be observed that this section of it is drawn on a scale where the distance of Sirius is no more than the 150th part of an inch ; so that probably all the stars, which in the finest nights we are able to distinguish with the naked eye, may be comprehended within a sphere, drawn round the large star near the middle, representing our situation in the nebula, of less than the 15th part of an inch radius.

*The Origin of nebulous Strata.*—If it were possible to distinguish between the parts of an indefinitely extended whole, the nebula we inhabit might be said to be one that has fewer marks of profound antiquity on it than the rest. To explain this idea perhaps more clearly, we should recollect that the condensation of clusters of stars has been ascribed to a gradual approach ; and whoever reflects on the numbers of ages that must have passed before some of the clusters, that will be found in my intended catalogue of them, could be so far condensed as we find them at present, will not wonder if I ascribe a certain air of youth and vigour to many very regularly scattered regions of our sidereal stratum. There are also many places in it where there is the greatest reason to believe that the stars, if we may judge from appearances, are now drawing towards various secondary centers, and will in time separate into different clusters, so as to occasion many subdivisions. Hence we may surmise that when a nebulous stratum consists chiefly of nebulae of the 1st and 2d form, it probably owes its origin to what may be called the decay of a great compound nebula of the 3d form ; and that the subdivisions, which happened to it in length of time, occasioned all the small nebulae which sprung from it to lie in a certain range, according as they were detached from the primary one. In like manner our system, after numbers of ages, may very possibly become divided so as to give rise to a stratum of 2 or 300 nebulae ; for it would not be difficult to point out so many beginning or gathering clusters in it. This view of the present subject throws a considerable light on the appearance of that remarkable collection of many hundreds of nebulae which are to be seen in what I have called the nebulous stratum of Coma Berenices. It appears from the extended and branching figure of our nebula, that there is room for the decomposed small nebulae of a large, reduced, former great one to approach nearer to us in the sides than in other parts. Nay possibly there might originally be another very large joining branch, which in time became separated by the condensation of the stars ; and this may be the reason of the little remaining breadth of our system in that very place : for the nebulae of the stratum of the Coma are brightest and most crowded just opposite our situation, or in the pole of our system. As soon as this idea was suggested, I tried also the opposite pole, where accordingly I have met with a great number of nebulae, though under a much more scattered form.



*An Opening in the Heavens.*—Some parts of our system indeed seem already to have sustained greater ravages of time than others, if this way of expressing myself may be allowed; for instance, in the body of the Scorpion is an opening, or hole, which is probably owing to this cause. I found it while gaging in the parallel from 112 to 114 degrees of north polar distance. As I approached the milky way, the gages had been gradually running up from 9.7 to 17.1; when, all of a sudden, they fell down to nothing, a very few pretty large stars excepted, which made them show 0.5, 0.7, 1.1, 1.4, 1.8; after which they again rose to 4.7, 13.5, 20.3, and soon after to 41.1. This opening is at least 4 degrees broad, but its height I have not yet ascertained. It is remarkable, that the 80 Nebuleuse sans étoiles of the Connoissances des Temps, which is one of the richest and most compressed clusters of small stars I remember to have seen, is situated just on the western border of it, and would almost authorize a suspicion that the stars of which it is composed were collected from that place, and had left the vacancy. What adds not a little to this surmise is, that the same phenomenon is once more repeated with the 4th cluster of stars of the Connoissance des Temps; which is also on the western border of another vacancy, and has a small, miniature cluster, or easily resolvable nebula of about  $2\frac{1}{2}$  minutes in diameter, north following it, at no very great distance.

*Phenomena at the Poles of our Nebula.*—I ought to observe, that there is a remarkable purity or clearness in the heavens when we look out of our stratum at the sides, that is, towards Leo, Virgo, and Coma Berenices, on one hand, and towards Cetus on the other; whereas the ground of the heavens becomes troubled as we approach towards the length or height of it. It was a good while before I could trace the cause of these phenomena; but since I have been acquainted with the shape of our system, it is plain that these troubled appearances, when we approach to the sides, are easily to be explained by ascribing them to some of the distant, straggling stars, that yield hardly light enough to be distinguished. And I have indeed often experienced this to be actually the cause, by examining these troubled spots for a long while together, when, at last, I generally perceived the stars which occasioned them. But when we look towards the poles of our system, where the visual ray does not graze along the side, the straggling stars of course will be very few in number; and therefore the ground of the heavens will assume that purity which I have always observed to take place in those regions.

*Enumeration of very compound Nebulæ or Milky-Ways.*—As we are used to call the appearance of the heavens, where it is surrounded with a bright zone, the milky-way, it may not be amiss to point out some other very remarkable nebulæ which cannot well be less, but are probably much larger than our own system; and being also extended, the inhabitants of the planets that attend the stars which



compose them must likewise perceive the same phenomena. For which reason they may also be called milky-ways by way of distinction. My opinion of their size is grounded on the following observations. There are many round *nebulæ*, of the first form, of about 5 or 6 minutes in diameter, the stars of which I can see very distinctly; and on comparing them with the visual ray calculated from some of my long gages, I suppose, by the appearance of the small stars in those gages, that the centres of these round *nebulæ* may be 600 times the distance of Sirius from us. In estimating the distance of such clusters I consulted rather the comparatively apparent size of the stars than their mutual distance; for the condensation in these clusters being probably much greater than in our own system, if we were to overlook this circumstance and calculate by their apparent compression, where, in about 6 minutes diameter, there are perhaps 10 or more stars in the line of measures, we should find, that on the supposition of an equal scattering of the stars throughout all *nebulæ*, the distance of the centre of such a cluster from us could not be less than 6000 times the distance of Sirius. And perhaps in putting it, by the apparent size of the stars, at 600 only, I may have considerably under-rated it; but my argument, if that should be the case, will be so much the stronger. Now to proceed:

Some of these round *nebulæ* have others near them, perfectly similar in form, colour, and the distribution of stars, but of only half the diameter: and the stars in them seem to be doubly crowded, and only at about half the distance from each other: they are indeed so small as not to be visible without the utmost attention. I suppose these miniature *nebulæ* to be at double the distance of the first. An instance, equally remarkable and instructive, is a case where, in the neighbourhood of two such *nebulæ* as have been mentioned, I met with a 3d, similar, resolvable, but much smaller and fainter nebula. The stars of it are no longer to be perceived; but a resemblance of colour with the former two, and its diminished size and light, may well permit us to place it at full twice the distance of the 2d, or about 4 or 5 times that of the 1st. And yet the nebulosity is not of the milky kind; nor is it so much as difficultly resolvable, or colourless. Now in a few of the extended *nebulæ*, the light changes gradually, so as from the resolvable to approach to the milky kind; which appears an indication that the milky light of *nebulæ* is owing to their much greater distance. A nebula therefore, whose light is perfectly milky, cannot well be supposed to be at less than 6 or 8000 times the distance of Sirius; and though the numbers here assumed are not to be taken otherwise than as very coarse estimates, yet an extended nebula, which in an oblique situation, where it is possibly fore-shortened by  $\frac{1}{2}$ ,  $\frac{2}{3}$ , or  $\frac{3}{4}$  of its length, subtends a degree or more in diameter, cannot be otherwise than of a wonderful magnitude, and may well outvie our milky way in grandeur.



The first I shall mention is a milky ray of more than a degree in length. It takes  $\kappa$  (Fl. 52) Cygni into its extent, to the north of which it is crookedly bent so as to be convex towards the following side; and the light of it is pretty intense. To the south of  $\kappa$  it is more diffused, less bright, and loses itself with some extension in 2 branches, I believe; but for want of light I could not determine this circumstance. The northern half is near 2' broad, but the southern is not sufficiently defined to ascertain its breadth.

The next is an extremely faint milky ray, above  $\frac{3}{4}$  degree long, and 8 or 10' broad; extended from north preceding to south following. It makes an angle of about 30 or 40 degrees with the meridian, and contains 3 or 4 places that are brighter than the rest. The stars of the Galaxy are scattered over it in the same manner as over the rest of the heavens. It follows  $\epsilon$  Cygni 11.5 minutes in time, and is  $2^{\circ} 19'$  more south.

The 3d is a branching Nebulosity of about a degree and a half in right ascension, and about 48' extent in polar distance. The following part of it is divided into several streams and windings, which, after separating, meet each other again towards the south. It precedes  $\zeta$  Cygni  $16^m$  in time, and is  $1^{\circ} 16'$  more north. I suppose this to be joined to the preceding one; but having observed them in different sweeps, there was no opportunity of tracing their connection.

The 4th is a faint, extended milky ray, of about 17' in length, and 12' in breadth. It is brightest and broadest in the middle, and the ends lose themselves. It has a small, round, very faint nebula just north of it; and also, in another place, a spot, brighter than the rest, almost detached enough to form a different nebula, but probably belonging to the great one. The ray precedes  $\alpha$  trianguli  $18^m.8$  in time, and is 55' more north.

The 5th is a streak of light about 27' long, and in the brightest part 3 or 4' broad. The extent is nearly in the meridian, or a little from south preceding to north following. It follows  $\beta$  Ceti  $5^m.9$  in time, and is  $2^{\circ} 43'$  more south. The situation is so low, that it would probably appear of a much greater extent in a higher altitude.

The 6th is an extensive milky Nebulosity divided into 2 parts; the most north being the strongest. Its extent exceeds 15'; the southern part is followed by a parcel of stars which I suppose to be the 8th of the Connoissance des Temps.

The 7th is a wonderful, extensive Nebulosity of the milky kind. There are several stars visible in it, but they can have no connection with that nebulosity, and are, doubtless, belonging to our own system scattered before it. It is the 17th of the Connoissance des Temps.

In the list of these must also be reckoned the beautiful nebula of Orion. Its extent is much above  $1^{\circ}$ ; the eastern branch passes between two very small stars, and runs on till it meets a very bright one. Close to the 4 small stars, which



can have no connection with the nebula, is a total blackness; and within the open part, towards the north-east, is a distinct, small, faint nebula, of an extended shape, at a distance from the border of the great one, to which it runs in a parallel direction, resembling the shoals that are seen near the coasts of some islands.

The 9th is that in the girdle of Andromeda, which is doubtless the nearest of all the great nebulae; its extent is above a degree and a half in length, and, in even one of the narrowest places, not less than 16' in breadth. The brightest part of it approaches to the resolvable nebulosity, and begins to show a faint red colour; which, from many observations on the colour and magnitude of nebulae, I believe to be an indication that its distance in this coloured part does not exceed 2000 times the distance of Sirius. There is a very considerable, broad, pretty faint, small nebula near it; my sister discovered it August 27, 1783, with a Newtonian 2-feet sweeper. It shows the same faint colour with the great one, and is, no doubt, in the neighbourhood of it. It is not the 32d of the *Connoissance des Temps*; which is a pretty large round nebula, much condensed in the middle, and south following the great one; but this is about  $\frac{2}{3}$  of a degree north preceding it, in a line parallel to  $\beta$  and  $\nu$  Andromedæ.

To these may be added the nebula in Vulpecula: for, though its appearance is not large, it is probably a double stratum of stars of a very great extent, one end of which is turned towards us. That it is thus situated may be surmised from its containing, in different parts, nearly all the 3 nebulosities; viz. the resolvable, the coloured but irresolvable, and a tincture of the milky kind. Now what great length must be required to produce these effects may easily be conceived when, in all probability, our whole system, of about 800 stars in diameter, if it were seen at such a distance that one end of it might assume the resolvable nebulosity, would not, at the other end, present us with the irresolvable, much less with the colourless and milky sort of nebulosities.

*A perforated nebula, or ring of stars.*—Among the curiosities of the heavens should be placed a nebula, that has a regular, concentric, dark spot in the middle, and is probably a ring of stars. It is of an oval shape, the shorter axis being to the longer as about 83 to 100; so that, if the stars form a circle, its inclination to a line drawn from the sun to the centre of this nebula must be about 56 degrees. The light is of the resolvable kind, and in the northern side 3 very faint stars may be seen, as also 1 or 2 in the southern part. The vertices of the longer axis seem less bright and not so well defined as the rest. There are several small stars very near, but none that seem to belong to it. It is the 57th of the *Connoissance des Temps*. Fig. 10 is a representation of it.

*Planetary nebulae.*—I shall conclude this paper with an account of a few heavenly bodies, that from their singular appearance leave me almost in doubt where



to class them. The 1st precedes  $\nu$  Aquarii  $5^m.4$  in time, and is  $1'$  more north. I have examined it with the powers of 71, 227, 278, 460, and 932; and it follows the laws of magnifying, so that its body is no illusion of light. It is a little oval, and in the 7-feet reflector pretty well defined, but not sharp on the edges. In the 20-feet, of 18.7 inch aperture, it is much better defined, and has much of a planetary appearance, being all over of a uniform brightness, in which it differs from *nebulæ*: its light seems however to be of the starry nature, which suffers not nearly so much as the planetary discs are known to do, when much magnified.

The 2d of these bodies precedes the 13th of Flamsteed's Andromeda about  $1^m.6$  in time, and is  $22'$  more south. It has a round, bright, pretty well defined planetary disc of about  $12''$  diameter, and is a little elliptical. When it is viewed with a 7-feet reflector, or other inferior instruments, it is not nearly so well defined as with the 20-feet. Its situation with regard to a pretty considerable star is, distance (with a compound glass of a low power)  $7' 51'' 34'''$ . Position  $12^\circ 0' s.$  preceding. Diameter taken with 278,  $14'' 42'''$ .

The 3d follows  $\beta$  (Fl. 44) Ophiuchi  $4^m.1$  in time, and is  $23'$  more north. It is round, tolerably well defined, and pretty bright; its diameter is about  $30''$ .

The 4th follows  $\gamma$  Sagitta  $17^m.1$  in time, and is  $2'$  more north. It is perfectly round, pretty bright, and pretty well defined; about  $\frac{3}{4}$  min. in diameter.

The 5th follows the 21st Vulpeculæ  $2^m.1$  in time, and is  $1^\circ 46'$  more north. It is exactly round, of an equal faint light throughout, and about  $1'$  in diameter.

The 6th precedes  $h$  (Fl. 39) Cygni  $8^m.1$  in time, and is  $1^\circ 26'$  more south. It is perfectly round, and of an equal faint light; its diameter near  $1'$ , and the edges well defined.

The planetary appearance of the first 2 is so remarkable, that we can hardly pose them to be *nebulæ*; their light is so uniform, as well as vivid; the diameters so small and well defined, as to make it almost improbable they should belong to that species of bodies. On the other hand, the effect of different powers seems to be much against their light being of a planetary nature, since it preserves its brightness nearly in the same manner as the stars do in similar trials. If we would suppose them to be single stars with large diameters, we shall find it difficult to account for their not being brighter; unless we should admit that the intrinsic light of some stars may be very much inferior to that of the generality, which however can hardly be imagined to extend to such a degree. We might suspect them to be comets about their aphelion, if the brightness as well as magnitude of the diameters did not oppose this idea; so that after all, we can hardly find any hypothesis so probable as that of their being *nebulæ*; but then they must consist of stars that are compressed and accumulated in the highest degree. If it were not perhaps too hazardous to pursue a former surmise of a renewal in



what I figuratively called the laboratories of the universe, the stars forming these extraordinary nebulæ, by some decay or waste of nature, being no longer fit for their former purposes, and having their projectile forces, if any such they had, retarded in each other's atmosphere, may rush at last together, and either in succession, or by one general tremendous shock, unite into a new body. Perhaps the extraordinary and sudden blaze of a new star in Cassiopea's chair, in 1572, might possibly be of such a nature. But lest I should be led too far from the path of observation, to which I am resolved to limit myself, I shall only point out a considerable use that may be made of these curious bodies. If a little attention should prove that, having no annual parallax, they belong most probably to the class of nebulæ, they may then be expected to keep their situation better than any one of the stars belonging to our system, on account of their being probably at a very great distance. Now to have a fixed point somewhere in the heavens, to which the motions of the rest may be referred, is certainly of considerable consequence in astronomy; and both these bodies are bright and small enough to answer that end\*.

W. HERSCHEL.

*Datchet, near Windsor, January 1, 1785.*

*XIII. Remarks on Specific Gravities taken at Different Degrees of Heat, and an Easy Method of Reducing them to a Common Standard. By Rich. Kirwan, Esq., F. R. S. p. 267.*

One capital advantage derivable from a table of specific gravities, is the knowledge of the absolute weight of any solid measure of the substances, or that of the solid measure of a given weight of those substances, a cubic foot of water being supposed to weigh 1000 oz. avoirdupois, and consequently a cubic inch of water weighing 253.182 grs. But all those who have treated this subject, have neglected to inform us of the temperature at which this agreement takes place; yet that it cannot take place in all temperatures is evident from the experiments of Dr. Halley and others, who have found, that from a few degrees above the

\* Having found two more of these curious objects, I add the place of them here, in hopes that those who have fixed instruments may be induced to take an early opportunity of observing them carefully.

Feb. 1, 1785. A very bright planetary nebula, about half a minute in diameter, but the edges not very well defined. It is perfectly round, or perhaps a very little elliptical, and all over of a uniform brightness: with higher powers it becomes proportionally magnified. It follows  $\gamma$  Eridani  $16^m 16^s$  in time, and is  $49'$  more north than that star.

Feb. 7, 1785. A beautiful, very brilliant globe of light; a little hazy on the edges, but the haziness goes off very suddenly, so as not to exceed the 20th part of the diameter, which I suppose to be from  $30$  to  $40''$ . It is round, or perhaps a very little elliptical, and all over of a uniform brightness: I suppose the intensity of its light to be equal to that of a star of the 9th magnitude. It precedes the third  $b$  (Fl. 6) Crateris  $28^m 36^s$  in time, and is  $1^\circ 25'$  more north than that star.—Orig.



freezing to the boiling point, water is dilated about  $\frac{1}{8}$  of its bulk; and consequently, if 1000 ounces at the freezing point be equal to one cubic foot, they must be equal at the boiling point to one cubic foot and 66.46 cubic inches. And if dilatations are proportional to the degrees of heat throughout the scale, there must be an augmentation of 3.136 cubic inches per cubic foot, produced by every 10 degrees of heat. Both these points remain therefore to be determined; first, at what temperature a cubic foot of water weighs exactly 1000 ounces avoirdupois; and 2dly, whether the dilatations produced by successive degrees of heat are proportional to the degrees that produce them. This last point has indeed been handled by others, but with different views; and their determinations are not easily applicable to the present question.

To examine this matter experimentally, I ordered a hollow tinned iron cone to be made, of 4 inches diameter in the base,  $\frac{1}{8}$  of an inch diameter in the summit inside, and 10 inches perpendicular height, whose solid contents should be 42.961 cubic inches; but by a slight diminution of the diameter, and a protuberance arising from the soldering, I found it to contain, in the temperature of 62°, only 42.731 cubic inches, according to the estimation of 1000 ounces to the cubic foot; and having filled it by immersion in boiling water, and taking it up at different degrees of heat, and weighed it when cold, I found its contents as expressed in the following table; the first column of which shows the degrees of heat at which it was taken up; the 2d, the weight of the water contained in it; the 3d, the diminution of weight occasioned by those degrees of heat; the 4th, the sum of the diminutions of weight in the cubic foot by the preceding degrees of heat; the 5th the weight of a cubic inch of water in each of those degrees of heat; and the 6th, the augmentation of bulk in the cubic foot by every 20° of heat. The horizontal lines, marked thus\*, are added from the experiments of Mr. Bladh, in the Memoirs of the Academy of Stockholm for the year 1776, whose determinations, as far as they reached, agreed very nearly with mine. The water used was common water well boiled and filtered. The experiments were for the most part 3 times repeated, and the difference in each trial amounted to a very few grains.



I.	II.	III.	IV.	V.	VI.	
Deg.	Contents of the cone in grains.	Dimin. in grains.	Sum of dim. in a cubic foot.	Weight of a cubic inch.	Increase in cubic inches.	
			Grs.			
212	10418.75	29.5	16589	243.8	4.892	
202	10448.25	77.5	15354	244.51	12.818	
182	10525.75	71.75	12133	246.33	11.533	
162	10596.00	62.60	9171	247.97	10.209	
142	10658.60	56.15	6602	249.43	9.103	
122	10714.75	49.00	4310	250.75	7.920	
102	10763.75	35.5	2226	251.89	5.7	
82	10799.25	19.5	788	252.72	3.120	
*75	.....	.....	.....	252.8	.....	
*70	.....	.....	.....	252.97	.....	
*66	.....	.....	.....	253.06	.....	
62	10818.75	0	0	253.182	0	Total increase of
*56	.....	.....	.....	253.3	.....	bulk from 62° to
		Increase.	Increase			212° = 65.327 cu-
*50	.....	.....	.....	253.46	.....	bic inches.
					Decrease.	Total from 36° to
42	10830.75	12	485.3	253.463	1.936	212 = 67.327 cubic
*36.5	.....	.....	.....	253.5	0.064	inches.

Hence we see, that a cubic foot of water weighs 485.3 grains more at 42° than at 62°, and consequently is equal to 1001.109 avoirdupois ounces, and in the temperature of 82° it weighs less than at 62° by 788.5 grains, and therefore is equal to 998.198 ounces. At the boiling point it wants 16589 grains, or 37.915 ounces of the weight it possesses at 62°, and consequently weighs only 962.085 ounces, &c. Hence also we see, that the expansions of water are not proportional to the degrees of heat; for by 20 degrees of heat from 62° to 82° a cubic foot of water is dilated only 3.12 inches, but by the next 20 degrees of heat, that is, from 82° to 102°, it is expanded 5.7 inches, &c. Mr. Bladh found the volume of water at 32° to be equal to that at 53°.6; but that this irregular expansion ceased at 36°.6, and according to Mr. De Luc (who first discovered it) at 43°.

As the expansion of liquids by equal degrees of heat is much greater than that of solids, it happens, that the specific gravities of the same solid taken at different temperatures will be different; and, what appears more extraordinary, the same solid will appear specifically heavier in higher than in lower temperatures; for the same volume of water being lighter in higher than in lower temperatures, the solid will lose less of its weight in it in the former than in the latter case: this mistake we may remedy by inspecting the 5th column of the foregoing table and the following analogy: as the weight of a cubic inch of water at the temperature of 62°, is to the weight of a cubic inch of water at  $n$  degrees of temperature, so is the specific gravity found at  $n$  degrees of temperature, to that which will be found at 62°. Thus, if 1000 grains of iron be weighed in



water of the temperature of  $62^{\circ}$ , and it loses in it 13.333 grains, if the same piece of iron be weighed in water of the temperature of  $75^{\circ}$ , it will lose but 13.313 grains; for the losses of weight will be as the weights of equal volumes of water at those temperatures, which we have seen are as 253.18 to 252.8; therefore its specific gravity in water of the temperature of  $62^{\circ}$  will be 7.49; and in water of the temperature of  $75^{\circ}$ , 7.511; but we may correct this by the above analogy, for  $253.8 : 252.18 :: 7.511 : 7.49$ . By this means we obtain the advantage of discovering the true weight of a cubic foot of any substance whose specific gravity is known, which it is now plain cannot be known when bodies are hydrostatically weighed at any temperature a few degrees above or below  $62^{\circ}$ , without such reduction, or subtracting the quantities in the 4th column.

This method is equally applicable, and with equal necessity, to other means of finding specific gravities, as areometers, the comparison of the weights of equal measures of liquids, the different losses of weight of the same solid, when weighed in different liquids, &c. In all which cases the weight of water at  $62^{\circ}$ , or the loss of weight of a solid in water at  $62^{\circ}$ , should be found by the above analogy. Dr. Hales and some others have estimated the weight of a cubic inch of water at 254 grains, which is an evident mistake, as it is true in no degree of temperature, and produces an error of more than 3 ounces in the cubic foot.

*XIV. Electrical Experiments made to ascertain the Non-conducting Power of a Perfect Vacuum, &c. By Mr. Wm. Morgan. p. 272.*

The non-conducting power of a perfect vacuum is a fact in electricity which has been much controverted among philosophers. The experiments made by Mr. Walsh, F. R. S. in the double barometer tube, clearly demonstrated the impermeability of the electric light through a vacuum; nor was it, I think, precipitate to conclude from them the impermeability of the electric fluid itself. But this conclusion has not been universally admitted, and the following experiments were made with the view of determining its truth or fallacy. When I first attended to the subject, I was not aware that any other attempts had been made besides those of Mr. Walsh; and though I have since found myself to have been in part anticipated in one of my experiments, it may not perhaps be improper to give some account of them, not only as they are an additional testimony in support of this fact, but as they led to the observation of some phenomena which appear to be new and interesting.

A mercurial gage B (fig. 11, pl. 9,) about 15 inches long, carefully and accurately boiled till every particle of air was expelled from the inside, was coated with tin-foil 5 inches down from its sealed end A, and being inverted into mercury through a perforation D in the brass cap E which covered the mouth of the cistern H; the whole was cemented together, and the air was exhausted from the



inside of the cistern through a valve *c* in the brass cap *E*, which producing a perfect vacuum in the gage *B*, formed an instrument peculiarly well adapted for experiments of this kind. Things being thus adjusted (a small wire *F* having been previously fixed on the inside of the cistern to form a communication between the brass cap *E* and the mercury *G*, into which the gage was inverted) the coated end *A* was applied to the conductor of an electrical machine, and notwithstanding every effort, neither the smallest ray of light, nor the slightest charge, could ever be procured in this exhausted gage. I need not observe, that if the vacuum on its inside had been a conductor of electricity, the latter at least must have taken place, for it is well known (and I have myself often made the experiment) that if a glass tube be exhausted by an air-pump, and coated on the outside, both light and a charge may very readily be procured. If the mercury in the gage be imperfectly boiled, the experiment will not succeed; but the colour of the electric light, which in air rarefied by an exhauster is always violet or purple, appears in this case of a beautiful green, and, what is very curious, the degree of the air's rarefaction may be nearly determined by this means; for I have known instances, during the course of these experiments, where a small particle of air having found its way into the tube *B*, the electric light became visible, and as usual of a green colour; but the charge being often repeated, the gage has at length cracked at its sealed end, and in consequence the external air, by being admitted into the inside, has gradually produced a change in the electric light from green to blue, from blue to indigo, and so on to violet and purple, till the medium has at length become so dense as no longer to be a conductor of electricity. I think there can be little doubt from the above experiments of the non-conducting power of a perfect vacuum; and this fact is still more strongly confirmed by the phenomena which appear on the admission of a very minute particle of air into the inside of the gage. In this case the whole becomes immediately luminous on the slightest application of electricity, and a charge takes place, which continues to grow more and more powerful in proportion as fresh air is admitted, till the density of the conducting medium arrives at its maximum, which it always does when the colour of the electric light is indigo or violet. Under these circumstances the charge may be so far increased as frequently to break the glass. In some tubes, which have not been completely boiled, I have observed, that they will not conduct the electric fluid when the mercury is fallen very low in them; yet on letting in air into the cistern *H*, so that the mercury shall rise in the gage *B*, the electric fluid, which was before latent in the inside, shall now become visible, and as the mercury continues to rise, and of consequence the medium is rendered less rare, the light shall become more and more visible, and the gage shall at last be charged, though it has not been near an electrical machine for 2 or 3 days. This seems to prove,



that there is a limit even in the rarefaction of air, which sets bounds to its conducting power; or, in other words, that the particles of air may be so far separated from each other as no longer to be able to transmit the electric fluid; that if they are brought within a certain distance of each other, their conducting power begins, and continually increases till their approach also arrives at its limit, when the particles again become so near as to resist the passage of the fluid entirely, without employing violence, which is the case in common and condensed air, but more particularly in the latter. These experiments however belong to another subject, and may possibly be communicated at some future time.

It is surprising to observe, how readily an exhausted tube is charged with electricity. By placing it at 10 or 12 inches from the conductor, the light may be seen pervading its inside, and as strong a charge may sometimes be procured as if it were in contact with the conductor: nor does it signify how narrow the bore of the glass may be; for even a thermometer tube, having the minutest perforation possible, will charge with the utmost facility; and in this experiment the phenomena are peculiarly beautiful. Let one end of a thermometer tube be sealed hermetically. Let the other end be cemented into a brass cap with a valve; or into a brass cock, so that it may be fitted to the plate of an air-pump. When it is exhausted, let the sealed end be applied to the conductor of an electrical machine, while the other end is either held in the hand or connected to the floor. On the slightest excitation the electrical fluid will accumulate at the sealed end, and be discharged through the inside in the form of a spark, and this accumulation and discharge may be incessantly repeated till the tube is broken. By this means I have had a spark 42 inches long; and, had I been provided with a proper tube, I do not doubt but that I might have had a spark of 4 times that length. If, instead of the sealed end, a bulb be blown at that extremity of the tube, the electric light will fill the whole of that bulb, and then pass through the tube in the form of a brilliant spark, as in the foregoing experiment; but in this case I have seldom been able to repeat the trials above 3 or 4 times before the charge has made a small perforation in the bulb. If again a thermometer filled with mercury be inverted into a cistern, and the air exhausted in the manner I have described for making the experiment with the gage, a Torricellian vacuum will be produced; and now the electric light in the bulb, as well as the spark in the tube, will be of a vivid green; but the bulb will not bear a frequent repetition of charges before it is perforated in like manner as when it has been exhausted by an air-pump. It can hardly be necessary to observe, that in these cases the electric fluid assumes the appearance of a spark,\* from the nar-

\* By cementing the string of a guitar into one end of a thermometer tube, a spark may be obtained as well as if the tube had been sealed hermetically.—Orig.



rowness of the passage through which it forces its way. If a tube, 40 inches long, be fixed into a globe 8 or 9 inches in diameter, and the whole be exhausted, the electric fluid, after passing in the form of a brilliant spark throughout the length of the tube, will, when it gets into the inside of the globe, expand itself in all directions, entirely filling it with a violet and purple light, and exhibiting a striking instance of the vast elasticity of the electric fluid.

I cannot conclude this paper without acknowledging my obligations to the ingenious Mr. Brook, of Norwich, who, by communicating to me his method of boiling mercury, has been the chief cause of my success in these experiments.\* I have lately learned from him, that he has also ascertained the non-conducting power of a perfect vacuum; but what steps he took for that purpose I know not. Of his accuracy however I am so well convinced, that had I never made an experiment myself, I should, on his testimony alone, have been equally assured of the fact. To most of the preceding experiments Dr. Price, Mr. Lane, and some others of my friends, have been eye-witnesses, and I believe that they were as thoroughly satisfied as myself with the results of them. I must beg leave to observe to those who wish to repeat them, that the first experiment requires some nicety, and no inconsiderable degree of labour and patience. I have boiled many gages for several hours together without success, and was for some time disposed to believe the contrary of what I am now convinced to be the truth. Indeed, if we reason a priori, I think we cannot suppose a perfect vacuum to be a perfect conductor without supposing an absurdity: for if this were the case,

\* Mr. Brook's method of making mercurial gages is nearly as follows. Let a glass tube  $\text{L}$  (see fig. 12, pl. 9), sealed hermetically at one end, be bent into a right angle within 2 or 3 inches of the other end. At the distance of about an inch or less from the angle let a bulb  $\kappa$ , of about  $\frac{3}{4}$  of an inch in diameter be blown in the curved end, and let the remainder of this part of the tube be drawn out at  $\text{I}$ , so as to be sufficiently long to take hold of, when the mercury is boiling. The bulb  $\kappa$  is designed as a receptacle for the mercury to prevent its boiling over, and the bent figure of the tube is adapted for its inversion into the cistern; for by breaking off the tube at  $\text{M}$ , within  $\frac{1}{8}$  or  $\frac{1}{4}$  of an inch of the angle, the open end of the gage may be held perpendicular to the horizon when it is dipped into the mercury in the cistern, without obliging us to bring our finger, or any other substance, into contact with the mercury in the gage, which never fails to render the instrument imperfect. It is necessary to observe, that if the tube be 14 or 15 inches long, I have never been able to boil it effectually for the experiments mentioned in this paper in less than 3 or 4 hours, though Mr. Brook seems to prescribe a much shorter time for the purpose; nor will it even then succeed, unless the greatest attention be paid that no bubbles of air lurk behind, which to my no small mortification I have often found to have been the case; but experience has at length taught me to guard pretty well against this disappointment, particularly by taking care that the tube be completely dry before the mercury is put into it; for if this caution be not observed, the instrument can never be made perfect. There is however one evil which I have not yet been able to remedy; and that is, the introduction of air into the gage, owing to the unboiled mercury in the cistern; for when the gage has been a few times exhausted, the mercury which originally filled it becomes mixed with that into which it is inverted, and in consequence the vacuum is rendered less and less perfect, till at last the instrument is entirely spoiled.—Orig.



either our atmosphere must have long ago been deprived of all its electric fluid by being every where surrounded by a boundless conductor, or this fluid must pervade every part of infinite space, and consequently there can be no such thing as a perfect vacuum in the universe. If, on the contrary, the truth of the preceding experiments be admitted, it will follow, that the conducting power of our atmosphere increases only to a certain height, beyond which this power begins to diminish, till at last it entirely vanishes; but in what part of the upper regions of the air these limits are placed, I will not presume to determine. It would not perhaps have been difficult to have applied the results of some of these experiments to the explanation of meteors, which are probably owing to an accumulation of electricity. It is not however my present design to give loose to my imagination. I am sensible, that by indulging it too freely, much harm is done to real knowledge; and therefore, that one fact in philosophy well ascertained is more to be valued than whole volumes of speculative hypotheses.—  
*Chatham Place, Feb. 12, 1785.*

*XV. Experiments and Observations relating to Air and Water. By the Rev. Joseph Priestley, LL. D., F. R. S. p. 279.*

This paper may be consulted in the collection of the author's works, p. 70, vol. 3, anno 1786.

*XVI. Of the Rotatory Motion of a Body of any Form whatever, Revolving, without Restraint, about any Axis passing through its Centre of Gravity. By Mr. John Landen, F. R. S. p. 311.*

The substance of this paper may be consulted in Mr. Landen's Mathematical Memoirs, vol. 2, p. 83, published anno 1789.

END OF THE FIFTEENTH VOLUME.

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*Erratum.*—In p. 37, l. 28, for 1750, read 1570.



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